SCIENCE PROGRESS
IN THE TWENTIETH CENTURY
A QUARTERLY JOURNAL OF SCIENTIFIC WORK & THOUGHT

EDITOR

SIR RONALD ROSS, K.C.B., F.R.S., N.L.,
D.Sc., LL.D., M.D., F.R.C.S.

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HISTORICAL SKETCH OF THE CHEMISTRY OF RUBBER

By S. C. BRADFORD, B.Sc.
The Science Museum, South Kensington, London

Indiarubber, or caoutchouc, is essentially a hydrocarbon, produced from a watery emulsion, or latex, obtained by tapping many tropical and sub-tropical trees, notably *Hevea braziliensis*. The first rumours of the existence of rubber are said to have reached Europe after the second voyage of Columbus to the New World in 1493–4, during which the natives were found to be in the habit of making playing-balls, bottles, waterproof boots and various other articles of a curious elastic gum. The earliest known reference to the substance occurs in a description by P. Martyr d'Anghiera in 1525, of some playing-balls seen by him in Mexico. Father Xavier de Charlevoix describes the ball as of a solid matter, but "extremely porous and light. It soars higher than our balls, falls on the ground, and rebounds much higher than the level of the hand which it quitted; it falls back again, and rebounds once more, although not to such a height this time, and the height of the bounce gradually diminishes." The Indians in Ecuador and Peru called the material "cahuchu" or "caucho," from which names the term "caoutchouc" was derived. Occasional samples were brought to Europe by travellers to adorn the curiosity cabinets of collectors. It was worth a guinea an ounce.

In 1731 two expeditions were organised by the Académie des Sciences to determine the figure of the earth. One of these, under the naturalist La Condamine, and Bouguer, an astronomer

and mathematician, sailed to the tropical regions of South America. La Condamine occupied his leisure in studying the flora and fauna of Peru and Brazil. And in 1736 he sent a small piece of rubber to the French Academy with a covering note in which he said: "There grows, in the forests of the province of Esmeraldas, a tree called by the natives of the country 'Hévé'; there flows from it, by simple incision, a liquor, white as milk, which gradually hardens and blackens in the air. The inhabitants make flambeaux of it . . . and boots which do not draw water, which, after having been blackened by holding them in the smoke, have all the appearance of real leather. They coat earthen moulds in the shape of a bottle with it, and, when the resin is hardened, they break the mould and force out the pieces through the neck and mouth; they thus get a non-fragile bottle, capable of containing all kinds of liquid." La Condamine was unable further to pursue his investigations, but, at his suggestion, Fresneau, an engineer, stationed at Cayenne, made an exhaustive inquiry into the source and preparation of rubber, the account of which was communicated by La Condamine to the Académie des Sciences in 1751.

The first chemical study of caoutchouc was published by Hérissart and Macquer in 1763, who examined the behaviour of the material towards various solvents. But the substance appears to have remained a mere curiosity in Europe, until in 1770 Priestley, the discoverer of oxygen, suggested its use, under the name of India rubber, for erasing lead pencil marks. And this was, for many years, the main purpose to which it was devoted, although Grossart showed how to make small tubes by cutting the bottles into strips, which were softened in ether, or essential oil, rolled on a mandrel and allowed to dry, when the surfaces amalgamated. In 1785 M. Charles, who made the first ascent in a balloon filled with hydrogen, coated his aerostat with rubber dissolved in turpentine. Suggestions for making clothing waterproof were made by Peal and Besson in 1791, Johnson in 1797, Champion in 1811, and Clark in 1815. None of these met with much success. In 1820 Nadier discovered a method of cutting indiarubber into thread for making elastic fabrics. And in the same year Hancock founded in England the first rubber factory. But it was not until about 1825 that Mackintosh discovered the

solubility of rubber in naphtha and successfully applied the solution to the fabrication of waterproof garments, from which time the modern rubber industry may be said to commence. In 1836, in consequence of the researches of Hancock, it was found that rubber could be welded into masses by energetic kneading under the action of moderate heat. The process was termed mastication and solved the problem of the manufacture of articles of daily use.

The material was quickly applied to the preparation of goloshes, bottles and many other objects. And, in the United States particularly, vast sums of money were invested in mills and plant for the manufacture of rubber goods. The discovery that these articles were subject to the influences of the seasons caused widespread consternation, so many had an interest in the success of the gigantic speculation. Experience showed that in hot weather the substance melted and became adhesive, while in the cold it became obstinately brittle. A panic ensued, thousands of tons of rubber were thrown upon the market, and the immense capital became literally valueless.

It was now that Goodyear of Newhaven, Connecticut, determined, at all hazards, to save something from the wreck. Inspired with the belief that Nature would scarcely have disclosed so many of the merits of indiarubber unless it possessed additional qualities of value to mankind, and oblivious of opposition and scorn, he devoted himself to the prosecution of the research. After suffering much hardship and even imprisonment for debt, he was rewarded in 1839 by the discovery of the process of vulcanisation, which overcame the influence of seasonal changes and adapted the material to countless purposes of mankind. The result was attained by heating the gum with an admixture of sulphur to a temperature of 270° F. In 1844 Hancock observed that the same result was obtained by dipping crude rubber into melted sulphur. Two years later he patented a method for moulding objects in caoutchouc, which was the starting-point of moulds for solid and hollow articles. The greatest impetus it ever received was undoubtedly given to the industry by the reinvention, in 1888, of the pneumatic tyre. An air-filled tyre for use with carriages had been patented by Thomson in 1845, but, partly owing to faulty construction and partly because its

advantages at low speeds were not sufficient to counterbalance the high cost of the large tyres employed, the invention had been allowed to drop. The advent of the safety bicycle, with small wheels to which springs could not easily be attached, led Dunlop independently to discover and patent the idea.

Previous to the utilisation of rubber in the chemical laboratory, apparatus was connected, if possible, by means of glass tubes bent twice at suitable angles. Occasionally one piece of apparatus was ground to fit another. Otherwise, for such purposes as connecting retorts with receivers, lutes were used. Thomson says, in his *System of Chemistry*, sixth edition, 1820: "The lute most commonly used by chemists, when vessels are exposed to heat, is fat lute, made by beating together in a mortar fine clay and boiled linseed oil. . . . The accuracy of chemical experiments depends almost entirely, in many cases, upon securing the joinings properly with luting. The operation is always tedious; and some practice is always necessary before one can succeed in luting accurately." The lutes were covered with strips of bladder or linen dipped in glue, made fast with cord and allowed to dry before commencing the experiment. It is not surprising that indiarubber was adopted almost as soon as it became available. The use of caoutchouc connecting-pieces was first described by Berzelius in 1814, in connection with his method of ultimate organic analysis. He says: "I take a thin piece of [unvulcanised] caoutchouc, and heat it a little. I bend it and cut off from the bendings a small portion with a pair of scissors. The cut surfaces unite together, and form a tube. If they do not unite, let them be pressed with the nails against each other, taking care not to touch them with the fingers." In his *Chemical Manipulation*, 1827, Faraday remarks: "Caoutchouc connecting pieces are easily made, and are of such constant use in attaching tubes and apparatus for the conveyance of vapours and gases, that a number of them, from an inch to two inches long, and from a quarter to half an inch in diameter, should be kept ready. . . . They are most easily made of the sheet caoutchouc, prepared by Mr. Hancock, which is about the tenth of an inch thick, and may be had in pieces ten or twelve inches square. A piece of this caoutchouc about an inch and a half square, is to be slightly warmed until it becomes

flexible and soft, and then put round a glass rod or other cylindrical body, rather smaller than the intended tube; the projecting edges are to be pinched together, and when they have slightly adhered, cut through with a pair of sharp scissors; this will . . . leave the two edges slightly adhering together. The junction is to be completed by immediately bringing these edges into contact throughout the whole extent of the cut surface . . . by applying a thumb-nail. When firmly pressed together whilst warm, the adhesion is such that the tube will tear elsewhere as readily as at the junction. . . . They are frequently useful of a conical form." A patent for an apparatus for squirting gutta-percha or other plastic material through an annular opening for the purpose of making continuous tubing was granted to Hunt in 1850. Soon after rubber tubing appears to have been adopted for chemical purposes, though this was probably made from cut sheet. The first reference to such use occurs in Abel and Bloxam's *Handbook of Chemistry*, 1854: "Small pieces of vulcanised Indian rubber tubing, which is now made of almost any dimensions, answer the purpose of these connectors exceedingly well; they may not adhere to the glass quite so tightly, a defect which may, however, be remedied by tying them firmly upon the tubes at each extremity." But the old form was not easily superseded, and in 1857 we find in Greville Williams's *Handbook of Chemical Manipulation*: "Vulcanised India-rubber tubes . . . are in almost every case preferable to those made in the laboratory from sheet caoutchouc; but those which are used to connect the chloride of calcium tube with the potash bulbs in organic analysis are much better of the latter kind. The reason is that the vulcanised ones are less adhesive."

In 1844 Hancock patented a process for moulding stoppers of gutta-percha, or gutta-percha and caoutchouc. But rubber stoppers for chemical purposes do not seem to have come into use until about 1865. The introduction may be attributed to Sir William Perkin. Mr. Tutin relates, on personal authority, that, impressed with the unsuitability of ordinary corks for organic analysis, Sir William was walking one day in London, when he happened to see a block of rubber in a shop window. The idea occurred to him of cutting stoppers of rubber, and was put into execution forthwith. A reference to their use

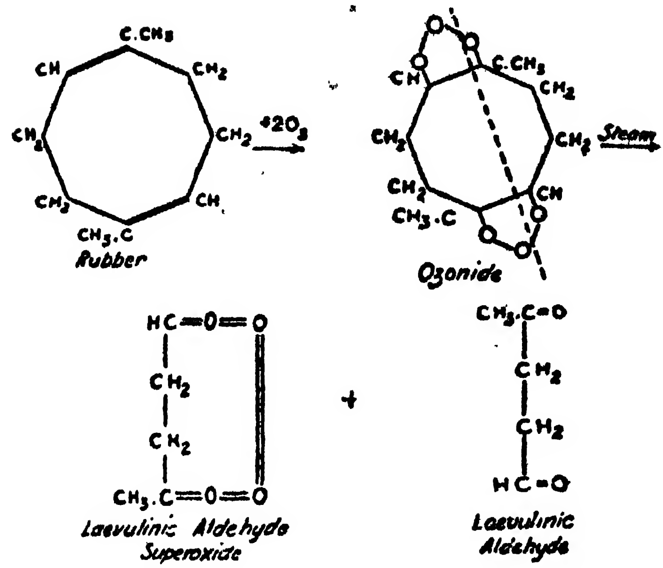
appears in 1872 in the *Chemical News*, where Donkin advocates dipping the knife or cork-borer in solution of caustic potash when desiring to cut or bore indiarubber corks.

Natural rubber is obtained from the latex by a process of coalisation apparently depending on the coagulation of the protein, or other protective colloid. The coagulation is brought about by heat, by the addition of acids, generally acetic, and by other methods. The globules of rubber rise through the liquid, coalesce and yield a tough elastic mass, which may be regarded as an emulsoid gel. Raw rubber contains as its principal constituent a hydrocarbon of the composition $C_{10}H_{16}$, the amount of which may reach as much as 95 per cent. Much early work was done on the destructive distillation of rubber from the time of Dalton. Its empirical constitution was determined from analyses made by Faraday, Berzelius, Ure and others. The first important chemical research on this subject was made by Greville Williams in 1860. He distilled rubber in an iron vessel at a low temperature, and obtained isoprene, C_5H_8, and cautchine, $C_{10}H_{16}$, which latter substance Wallach subsequently showed to be identical with dipentine. Greville Williams also noticed the transformation of isoprene into a rubber-like body, of which the quantity was insufficient for identification, but he evidently considered caoutchouc to be a polymer of isoprene. In 1875 Bouchardat investigated the products of the distillation of rubber, and came to the conclusion that the substances he obtained, $C_{10}H_{16}$, $C_{15}H_{24}$, etc., including rubber itself, are polymers of isoprene. In 1879, while preparing the hydrochloride of isoprene, by shaking with concentrated hydrochloric acid, a rise of temperature indicated that combination had occurred, but on distillation a solid residue remained which was found to have " the elasticity and other properties of rubber itself."

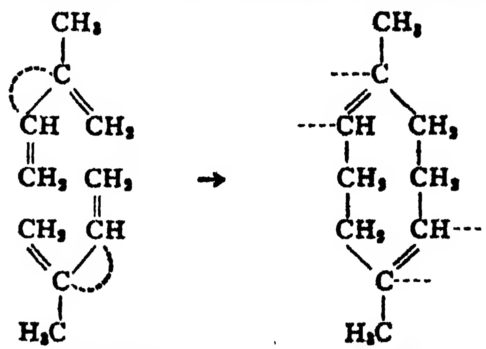
In 1882 Tilden showed that the colourless syrupy substance resulting from the atmospheric oxidation of isoprene is converted into true rubber when brought into contact with strong aqueous hydrochloric acid, or nitrosyl chloride, remarking that " It is this character of isoprene which gives it a somewhat practical interest, for, if it were possible to obtain this hydrocarbon from some other and more accessible source, the synthetic production of rubber could be accomplished." At the same time Tilden proposed the constitutional formula

for isoprene, $CH_2 : C(CH_3) . CH : CH_2$. Two years later he showed that isoprene can be obtained by the destructive distillation of turpentine, indicating the first possible process for the commercial preparation of rubber. He noted that polymerisation took place most readily after a preliminary heating which yielded an oily body, and suggested that the analogues of isoprene, C_4H_6, C_6H_{10}, etc., might be made to polymerise to a series of rubber hydrocarbons. In 1887 Wallach showed that isoprene changed to a rubber-like body under the action of light alone, thereby anticipating Tilden's independent discovery five years later. In a paper published in 1892 Tilden remarks: " I was surprised . . . at finding the bottles containing isoprene from turpentine entirely changed in appearance. In place of a limpid, colourless liquid, the bottles contained a dense syrup in which were floating several large masses of solid, of a yellowish colour. Upon examination this turned out to be indiarubber. . . . The artificial rubber unites with sulphur in the same way as ordinary rubber, forming a tough elastic compound." Tilden was thus the first to show that synthetic rubber can be vulcanised like natural caoutchouc. Tilden's and Bouchardat's work was confirmed by Weber in 1894, so that the scepticism expressed by Klages and Harries as to the methods employed, and the identity of the product obtained, is remarkable.

The constitution of rubber has been considerably elucidated by the extensive researches of Harries, dating from 1901 and onwards. He had found that when unsaturated substances were treated with ozone, a molecule of ozone added itself at each double bond, yielding an ozonide, which, on treatment with steam, split up into two fractions at the point of addition. In this way he determined the constitution of oleic acid. From rubber he obtained a body of the molecular weight indicated by the formula $C_{10}H_{16}O_6$. This was decomposed by steam into lævulinic aldehyde, $CH_3 . CO . CH_2 . CH_2 . CHO$, lævulinic acid, $CH_3 . CO . CH_2 . CH_2 . COOH$, and lævulinic aldehyde-superoxide, $CH_3 . \underset{\underset{O}{\|}}{CO} . CH_2 . CH_2 . \underset{\underset{O}{\|}}{CHO}$ (the two O joined: O————O). Harries explained the action by the following structural formulæ :



This indicates that the rubber nucleus is an eight-membered ring. And its formation from isoprene is represented thus :



2 molecules Isoprene. Rubber.

The dotted lines denote partial valencies according to Thiele's theory. And Harries supposed that an indefinite number of rings might be linked together at the double bonds. This view was criticised by Pickles in 1910, who preferred to think that the C_5H_8 nuclei unite to form long chains as :

$$—CH_2.\overset{\overset{\displaystyle CH_3}{|}}{C}:CH.(CH_2)_2.\overset{\overset{\displaystyle CH_3}{|}}{C}:CH.(CH_2)_2.\overset{\overset{\displaystyle CH_3}{|}}{C}:CH.CH_2—$$

In 1914 Harries proposed a new formula for rubber consisting of five C_5H_8 groups united to form a C_{20} ring. Rubber

forms a number of addition compounds with the halogens, sulphur, etc. The simplest and most definite of these is the tetrabromide, $C_{10}H_{16}Br_4$, in which two atoms of bromine have attached themselves at each double bond. The sulphur derivatives are obtained in the process of vulcanisation. Weber concluded that there is a series of sulphur compounds between the limits $(C_{10}H_{16})_{10}S_2$ and $C_{10}H_{16}S_2$, the latter corresponding to ebonite, though later work has thrown considerable doubt on the accuracy of his deductions.

The determination of the molecular weight of caoutchouc has not so far been accomplished. In the liquids usually employed to dissolve it, rubber is present in the colloidal state, and each particle probably consists of a large aggregate of molecules. So that osmotic pressure, freezing point and boiling point methods are inapplicable. As, however, in 1912 Walden discovered that starch formed a true solution in formamide, with the aid of which he found the molecular weight to correspond to the comparatively simple formula $(C_6H_{10}O_5)_4$, a true solution of rubber may eventually be obtained. In the meantime, as its simpler soluble derivatives have only ten carbon atoms in the molecule, it may be assumed that rubber contains no more than this number. That the molecules of caoutchouc and those of many of its derivatives are associated with each other to a high degree is in accordance with their colloidal condition.

From 1907 onwards the attention of a number of chemists began to be directed to the problem of the artificial production of rubber. Activity in this country was centred in the Synthetic Products Company, while in Germany the question was taken up by the Bayer Company and the Bädische Anilin und Soda Fabrik. Previous observations had shown that the homologues of isoprene polymerise to substances related to caoutchouc possessing properties varying from sticky resin to brittle solids. So far as the consumer is concerned, the exact constitution of a body is unimportant, provided its physical properties are suitable. So that the product required is not necessarily true rubber, but a substance having similar physical properties. The more important members of the isoprene series that have been considered are: erythrene, or buta-1 : 3-diene, $CH_2 : CH . CH : CH_2$; piperylene, or 1-methyl-buta-1 : 3-diene, $CH(CH_3) : CH . CH : CH_2$; and isoprene, or 2-methyl-buta-1 :

3-diene. Many methods for the preparation of these bodies have been suggested. Turpentine was originally proposed as the raw material, but, owing to its limited supply and frequent fluctuations in price, attention was directed to other substances, such as wood, starch, sugar, acetylene, coal-tar and petroleum. In 1910 the Synthetic Products Company patented a method of preparing isoprene from fusel oil. The fraction distilling at 128°–131° consists of iso-amyl alcohol, $(CH_3)_2 : CH . CH_2 . CH_2OH$, and active amyl alcohol, $CH_3 . CH_2 . CH(CH_3) . CH_2OH$, which are converted into their monochlorides with hydrochloric acid, and then chlorinated in such a way that only dichlorides result, such as $(CH_3)_2CH . CHCl . CH_2Cl$, $(CH_3)_2CCl . CH_2 . CH_2Cl$ and $\genfrac{}{}{0pt}{}{CH_2Cl}{CH_3}\!>\!CH . CH_2 . CH_2Cl$. These are passed over soda-lime at 470°, giving a 40 per cent. yield of isoprene. As the amount of fusel oil available is small, Fernbach and Stange endeavoured to devise a means for its production in larger quantities. And in 1911 they patented a method involving the cheap production of butyl alcohol and acetone by the fermentation of starch with the aid of the butylic bacillus. Butyl chloride obtained from the alcohol is then carefully chlorinated with the production of the dichlorides $CH_3 . CH_2 . CHCl . CH_2Cl$, $CH_3 . CHCl . CH_2 . CH_2Cl$ and $CH_2Cl . CH_2 . CH_2 . CH_2Cl$ which yield butadiene on passing over heated soda-lime.

Various means have been proposed for the polymerisation of the hydrocarbons obtained. In 1909 the Bayer Company employed the application of heat to the substance contained in sealed tubes. The following year Harries found that the presence of acetic acid accelerated the change. The Bayer Company showed that the acetic acid probably acted as a diluent. And the use of other agents such as alkalies, alkaline earths, urea, blood, etc., with or without preliminary heating, have been suggested.

The most dramatic incident in the race between the English and German firms was the almost simultaneous discovery of the value of sodium as a polymerising agent. Harries found, at the end of 1910, that sodium causes the change to take place quickly, and practically quantitatively, at a low temperature. The method was patented by the Bayer Company early in 1911. Dr. Matthews had, however, been investigating the action of sodium on dimethyl-allene, and in July 1910 it

occurred to him to seal up some isoprene with sodium. Reluctantly returning to town during a holiday in August, he discovered that the tube contained a portion of remarkably good rubber. By September the contents had set to a solid amber-coloured mass. Further work indicated the importance of sodium as a general polymerising agent for these hydrocarbons, and a patent was applied for on October 25, three months before the German application.

While possessing the same physical properties as synthetic rubbers produced by other methods, the rubbers obtained by the action of sodium appear to have a somewhat different chemical constitution. Ozonides are formed less readily, and give rise to other products than lævulinic acid and aldehyde. The constitution of sodium isoprene caoutchouc is believed to be dimethylcycloöctadiene with conjugated ethylenic linkages.

As the result of these researches it is hoped that before long synthetic rubber will be placed on the market.

A CONTRIBUTION TO THE BIONOMICS OF ENGLISH OLIGOCHÆTA

PART II.—BRITISH ENCHYTRÆIDS: THE RÔLE OF PACHYDRILUS[1]

By the Rev. HILDERIC FRIEND

Enchytræids with Red Blood.—Among the many genera which go to make up that family of Oligochæts or Annelids which is known as the Enchytræidæ there are two in particular which are characterised by the possession of red blood. These are at present known among zoologists as Lumbricillus and Marionina; but for some time they were better known as Pachydrilus—a name which was first applied to the genus in 1861 by Claparède (1). Since the colour of the blood seems to bear a distinct relationship to their habitat and economy they may be regarded as forming a group by themselves, and this group it is convenient to designate the Pachydrilid section of Enchytræids. While the rest of the Enchytræidæ have, with very few exceptions, colourless blood, and as a rule lead a terrestrial life, the Pachydrilids with their coloured blood are either purely aquatic or show a decided preference for very moist localities. If we may state the facts in another way, the red-blooded Enchytræids are aquatic with weak terrestrial tendencies, while the white-blooded genera are terrestrial with but weak aquatic tendencies.

I have, it is true, occasionally found Pachydrilids in garden soil, but their presence in cultivated ground is always regarded as an unusual fact. At the same time Enchytræids with colourless blood are much more frequently found leading a semi-aquatic life, and a few species belonging to this group prefer such conditions to a purely terrestrial existence. It will, however, be obvious to the careful observer that the two

[1] See Science Progress, No. 29, July 1913, vol. viii. pp. 99 *et seq.*, for the first part of our study.

groups of worms are adapted, from the standpoint of their blood constituents, to different modes of life. The answer to the question, What is the rôle of the Pachydrilids or red-blooded Enchytræids, therefore, is to be discovered by experiment and a careful study of their life-history. We proceed, therefore, to a summary of the principal investigations which have been carried out in this connection up till the present time.

A Brief Historical Survey.—As already stated, the term Pachydrilus was first employed by Claparède in 1861, in which year he published what may be regarded as a pioneer study in this group of annelids. Although his work appeared in French, it had reference to our British Isles. Having spent some time in the investigation of the worms found on the seashore in the Hebrides, he gave the results in the *Mémoires de la Société de Physique et d'Histoire naturelle de Genève* (vol. 16, i. 75 *seq.*).

Later research, it is true, showed that the eminent Danish zoologist, O. F. Müller (6) so long ago as 1771, or nearly a century before the time of Claparède, had described some species of Pachydrilus under such names as Gordius, Lumbricus, or Nais. But each of these terms has in the course of time been allocated to other families or genera, and all are to-day in systematic use. But in 1843–4 Oersted (8) employed the term Lumbricillus, and of recent years this name has been given precedence over Claparède's Pachydrilus. It is still, however, convenient to speak of the red-blooded forms as Pachydrilids, since, though there are two genera in the group, they are so closely related that they can only be differentiated by the structure of one of the sexual organs after they have reached the adult stage. The distinction is minute, but it serves to break up a large number of species into smaller sections which are easier to handle than the larger and more unwieldy genera.

The Earliest Known Species.—Perhaps the first species to become the subject of diagnosis was Lumbricillus lineatus O. F. Müller, which was described in 1771 as "Gordius pallidus linea longitudinale rufa," and was three years later named by the author (7) Lumbricus lineatus. This worm has been frequently studied, but in spite of all the attention bestowed upon it and its allies, we are unable even to-day to define it with such exactitude as to ensure its being readily and un-

erringly distinguished from a number of other species which more recent authors have created. Thus Michaelsen (5) says it is doubtful whether the Lumbricillus subterraneus of Vejdovsky can be distinguished from L. lineatus; while Southern (9) remarks on the difficulty of dividing therefrom the verrucosus of Claparède and Hesse's Pachydrilus litoreus. My own researches (2) tend to show that verrucosus is only a variety of lineatus, and that in a series from the Manchester sewage works which I recently examined with great care all stages between the one form and the other were to be obtained from the one consignment.

It may at this point, however, be affirmed that the work of Claparède in 1861 laid the foundations for a scientific classification, and since that date an enormous amount of work has been done on the group by such investigators as Bretscher, Eisen, Friend, Hesse, Michaelsen, Southern, Stephenson, Vejdovsky, and Welch. It is not necessary here to discuss the definitions, synonyms, habitats, or distribution of the worms under consideration; it being sufficient for our present purpose to state that the term Pachydrilus or Pachydrilid is intended to designate the red-blooded Enchytræids generically known as Lumbricillus and Marionina.

About fifty to sixty species have so far been described, and although some of these will have to be eliminated there must yet be many other species which have not hitherto been defined. It will be taking a very conservative view of the subject if we say that there are probably no fewer than a hundred species of Pachydrilids in existence, and that the bulk of these are European or even British.

Seashore Scavengers.—As already indicated, Claparède found his material on the seashore, and this he distributed under five species. One of these, however, had white blood, and was therefore named by him Pachydrilus lacteus, but this proves to be a synonym for the widely distributed Enchytræus albidus Henle, which is frequently found in such localities, as well as in manure heaps and among vegetable débris inland.

Any one who will be at the pains to examine the decaying algæ found in backwashes on the shore, especially in spring time, will discover that the material is packed with tiny worms from half-an-inch to an inch in length. I have seen them at times numbering not hundreds but myriads, in every stage of

development. The material must be moist, for as soon as it begins to dry the worms disappear. The species found in such localities include not only Claparède's P. semifuscus, crassus, verrucosus, lacteus, and ebudensis, but L. litoreus, lineatus, and evansi, together with M. georgiana, arenaria, and a variety of others. They feed on the algæ, sponges, zoophytes and similar decaying material cast up by the tides, and in this way act as an army of scavengers to clear away useless and putrid matter. There is no part of the British coast where they may not be found, if only the moist débris can be stranded by the waves and kept in a sufficiently humid condition. Much good service might be rendered to science by visitors to the less frequented shores if they would forward to me in tin boxes samples of the material to be collected in these localities.[1]

Drain Purifiers and Stream Workers.—A second group of Pachydrilids is to be found associated with gutters, drains, and ditches; the muddy margins of ponds, streams, and rivers where cattle drink, or into which the filth from large industrial works, and the offal and sewage of towns and cities finds its way; and especially at our sewage works up and down the land. It would be impossible to estimate the number of red-blooded worms supported by the ooze of such a stream, for example, as the Aire, which flows in the neighbourhood of Bradford and Leeds. Should a collector go down to the margin of the river at Apperley Bridge, for instance, and dip up a handful of the greasy ooze, he would in all probability find it teeming with life. There may be Tubificids or other worms present, and in some instances white-blooded Enchytræids may be dominant, but as a general rule the bulk of the scavengers will be species of Pachydrilus. By way of contrast it may be noted that at Burton-on-Trent, where there is a clean margin to the river, no such worms are to be found.

What is true of the Aire and similar rivers applies also to the streamlet into which the drainage of a country village flows. Let the mud, refuse, roots of grass, or floating débris be examined and the wealth of annelid life will be amazing. As concrete illustrations are better than mere postulates, let me give an example. About three miles from Ashby-de-la-Zouch is the secluded village of Blackfordby through which flows a tiny rill. It is this watercourse which supplies much of the drinking

[1] Address: Department of Zoology, Birmingham.

water in use by the villagers. Let the algæ, weeds, and mud at the lower end of the village be examined, and they will be found to contain myriads of Pachydrilids. They are here at all times of the year, but at certain seasons are accompanied by Naidids, Tubificids, and other annelids, the genus Lumbricillus, however, being invariably the predominant form.

It must further be observed that it is quite impossible in many such instances to distinguish the fresh-water forms from those which occur among algæ on the shore. On the other hand the many inland habitats do undoubtedly supply us with species which would not live in the ooze of estuaries where the water is brackish, or on decaying matter which is impregnated with salt water. The point to be noted is that these red-blooded annelids abound alike in the black ooze of estuaries, rivers, streams, and ponds, and among the decaying forms of vegetable and animal life which are frequently so abundant in sluggish waters and backwashes.

Denizens of Sewage Works.—Turning our attention to the sewage works and farms established in various parts of the country we shall find that here also they are thriving vigorously. They choose the localities for which they are best adapted, or where their work can be most successfully carried out. I have personally inspected such works, farms, or effluents in places as widely apart as Battle and Hastings in the South, Worcester and Nottingham in the Midlands, and Carlisle in the North—to mention but a few—and have at various times received samples from Manchester, Birmingham, and elsewhere. In all these instances such species as L. lineatus, verrucosus, and subterraneus have been found, together with forms which are not so widely known. Welch (12) has shown that L. rutilus is an important agent under similar conditions in certain parts of America. And here one may be allowed to draw attention to the fact that while hardly any thought has been given to this subject under official direction in England—my own researches having been entirely voluntary and unremunerated—in America Lederer (3), Hering, Welch and others have shown the way to the carrying out of systematic and valuable investigations. At the same time it is but fair to mention that the Rivers Board of the West Riding of Yorkshire has recently been issuing some Reports on the Organisms found in Sewage Filters. These are at present

confidential, and are published solely for the use of the Committee; but it is not too much to hope that in time the Board will authorise the scientific investigation, among other things, of the vast number of Pachydrilids which are to be found in the localities under their direction.

It is not without interest to note in this connection that the common worm frequently known as the gilt-tail or lesser brandling (Dendrobæna subrubicunda Eisen) is found in association with Pachydrilus among sewage, alike in Chicago and in Manchester, while nematodes, springtails, watermites, and the larvæ of sundry gnats and flies are equally so found.

Among the annelids recently examined by me in clinkers from the percolating filter beds at the Staleybridge and Dukinfield Sewage Works were Lumbricus rubellus Hoffm., D. subrubicunda Eisen, Allurus tetrædrus Sav., Enchytræus minimus Br., and L. lineatus O.F.M. Of special interest also is the fact already noted that the series of the latter was so large and varied that one could find specimens which agreed on the one hand with the typical lineatus, and on the other with verrucosus, while the extremes were joined by other specimens representing all the intermediate stages. It would thus appear that verrucosus may be merely an extreme form of lineatus. Since, therefore, lineatus is in all probability, as Michaelsen has shown (4), the earliest name, verrucosus and other later species may have to be subordinated to it as varieties and sub-species.

The Genus Marionina.—Thus far our attention has been mainly directed to the study of the genus Lumbricillus, and it is a significant fact, which in the future will demand much more attention than it has hitherto received, that the related genus Marionina is not as a rule so intimately associated with decaying vegetable matter as with the soil. Again and again one finds such sentences as "Unter Steinen am Meeresstrande" in German accounts of the different species of Marionina. One species, indeed, is found, as its name (M. sphagnetorum Vejd.) implies, in mossy moorlands, and Southern informs us (9) that "this interesting species is a characteristic member of the alpine fauna of Ireland. It is almost invariably to be found in the soil of moors and hills above 500 feet." Concerning M. glandulosa Mich. it is written—"Im wasserdurchtränkten Detritus an Flussufern," and M. lobata Bret. is stated to occur

in Switzerland—" Im Schlamm und zwischen vermodernden Schilfhaufen." This genus evidently plays a part more nearly related to that of the white-blooded Enchytræids, and has the terrestrial tendency.

In Relation to our Water Supply.—There remains one other point of interest to consider. Again and again during the past quarter of a century have I received from medical men, chemists, sanitary officials, naturalists, and others specimens of worms found in tap water at places so distant from each other as King's Lynn, Nottingham, Mildenhall, Worcester, Chelmsford, and the New Forest. In most instances the specimens were undoubtedly to be referred to Lumbricillus subterraneus Vejd. (10). This worm is about 12 to 18 mm. in length, and usually has the setæ arranged in four groups of 5 to 7 in each segment. The originals were obtained from the underground waters of Lille and Prague (11). In 1907 Prof. Gregg Wilson sent a large number of these worms to Mr. Southern (9), " from the sewage works at Belfast, where they occurred in such numbers as to be a serious nuisance." They were also, Southern thinks, of subterranean origin. Further, in April 1908 the same authority found large numbers of this Pachydrilid in a stream at Adlington in Lancashire. " This stream is excessively contaminated with trade effluents. A preparation of iron and aluminium is used to purify the stream, and this forms a thick gelatinous layer on the bed of the river. This layer is crowded with vast numbers of this worm, accompanied by Tubifex, Limnodrilus, and a species of the Nematode genus Mermis."

A Note on Well-Worms.—In order that we may not be thrown off our guard in this study it is desirable to bear in mind that there are quite a number of other worms found from time to time in wells, taps, and drinking water at home and abroad. Many years ago I described one of these as Diachæta curvisetosa. It has since been shown that the genus is Haplotaxis. We have two species of this genus in England, viz. H. gordioides found by me at Hastings, and often recorded in Europe; and H. curvisetosa, hitherto found only in Essex. While resident some years ago in East Anglia I obtained specimens of another waterworm from wells at Mildenhall in Suffolk. They were very similar to our Pachydrilids, but belonged to no known genus of British worms, and up till the present remain undescribed. Finally I have to report the somewhat frequent occur-

rence in the neighbourhood of the New Forest, especially at Ringwood, of yet another worm known as Rhynchelmis. It is not at present definitely recorded for any other part of England, though Beddard has an impression that he once saw it at Oxford. It is, however, abundant in certain parts of Europe, and has been carefully studied. It gets into tap water from the reservoirs.

The foregoing must suffice as typical illustrations, drawn from a vast amount of material and long years of study, to show how numerous and widespread the Pachydrilids are, how closely they are associated with decomposing animal and vegetable matter, and how great a force, either for good or evil, they may prove to be. Naturally the question of questions is—" What is their rôle ? Are they beneficent or injurious ? Are they scavengers pure and simple, or can they be suspected of doing harm, especially if they are unconsciously swallowed by one who is drinking water from the main or the well ? " To this matter we devote our concluding remarks.

Pachydrilus and the Human Body.— Medical men and chemists are from time to time shown strange things which are said to have passed through the stomachs of human beings, or to have found a temporary lodgment there. On more than one occasion Enchytræids have been submitted to me with the assurance that they have been vomited or voided, and the question has arisen—Was it possible that they had been taken into the system either as eggs or after they had been hatched ; and if so had they been swallowed with the drinking water or taken in with some vegetable or salad from the garden ?

In each instance the following facts are to be noted : (i) The observation was open to suspicion. The contents of the stomach may have been allowed to come into contact with the soil, or may have been discharged into a receptacle which contained vegetable or earthy matter. The vomit may have been left to stand for a time in some situation which enabled the ubiquitous annelids to gain admission, and in any of these cases the presence of Enchytræids would not be remarkable.

(ii) Water might be passed into receptacles which were unclean, and again it would not be matter for surprise if minute worms were discovered in it.

(iii) There is at present before me no evidence to show that red-blooded worms (Pachydrilids) have ever been found under

such conditions. It is very unlikely that the minute cocoons, if inadvertently swallowed, would hatch out their young in the human stomach; or that the worms if swallowed unwittingly with a salad would pass through the entire body and be evacuated in a perfectly whole and sound condition. They are so tender, and so utterly destitute of integuments of an enduring nature, that they could not survive so severe an ordeal.

(iv) The worms which have been submitted to me belonged to the white-blooded group. The genera Fridericia and Enchytræus are rarely if ever found under such conditions as would render us liable to swallow them in our food or drink them in our water. They might be found in earth around radishes, onions, or other salads, or even find their way into minute crevices and disease spots, and so by a very rare chance be eaten, if the usual care was not taken in washing them; but I cannot conceive of their finding a way into the human body by any other means.

(v) The Pachydrilids most frequently met with are not usually less than ¾ inch in length. Should they, therefore, find their way into our drinking water, their size and colour would render them sufficiently conspicuous for the most casual observer to discover them. But even in the event of their being accidentally swallowed when a draught of water is hurriedly taken, the softness of their bodies, and the absence of all weapons of offence and defence, such as suckers, forceps, and horny jaws, would render them perfectly innocuous.

We may certainly conclude, therefore, that Pachydrilids are never harmful to man, whatever may be their rôle as the carriers of disease to other animals.

Their Beneficent Action.—On the other hand it is equally certain that their rôle is, on the whole, a beneficent one. In the first place, feeding as they do on decaying vegetable and animal matter, they greatly aid in the disintegration of material which might easily prove dangerous. Thus they prepare the way for bacteria and other lowly forms of life, aid the chemical processes which are in action, and convert dead matter into a rich and valuable humus. In mud and slime they help by their movements to carry on oxygenation so that larger forms of life may be able to exist. In sewage works they are equally serviceable, and may possibly do much to keep in check disease germs and injurious bacteria; while their abundance

in ponds and ditches, streamlets and rivers, estuaries and backwaters makes them exceedingly dainty food for fishes, ducks, waterfowl, and other forms of life. They multiply with astonishing rapidity when once they have established themselves in a suitable locality, and are able in a very short time to convert the worthless refuse on which they feed into living matter suitable for the sustenance of animals which can be utilised as human food.

The Claim of Pachydrilids to Research.—When we remember that there are possibly a hundred different species of Pachydrilus in Great Britain, that they reproduce with great rapidity, and may under suitable conditions be found in untold myriads ; and when we further reflect that they have never yet been shown to be injurious to man or to plant life, but that they act as nature's scavengers wherever putrid and decaying material is to be found, aerating the foul ooze in which they flourish, and supplying fish and fowl with wholesome food, it will surely be admitted that their task is a beneficent one, and that the Pachydrilids deserve much more attention than has hitherto been bestowed upon them in this country.

Conclusion.—We have in the foregoing remarks endeavoured to establish the following points :

1. The Pachydrilids are a group of Enchytræids possessed of red blood. They are divided into two genera, Lumbricillus and Marionina, and are known to number fifty or sixty species.

2. They are found wherever moist decaying matter accumulates—by shore and backwash, in ponds, ditches, streams, and rivers, and especially among sewage.

3. They are never injurious to man.

4. Their mission is to purify ooze, oxygenate the mud and slush in which they flourish, disintegrate decaying material, and finally surrender their lives as a delicate aliment for creatures which serve as human food.

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THE POLLUTION OF THE SEA

BY A BIOLOGIST

EARLY in 1915 the Royal Commission on Sewage Disposal made its final Report, and thus brought to an end an inquiry which has been noteworthy in many respects. Let us note that the British method of dealing with a technical question likely to involve legislation by means of a Royal Commission, or a Parliamentary or Departmental Committee, is one which has many disadvantages. The process of inquiry is usually a needlessly protracted one, and it may happen that the object for which it was instituted has been forgotten before the time of publication of the Report. One suspects that this may be a consummation desired by the officials of the Department promoting the inquiry, and it even may be admitted that the delay is often beneficial, for hasty and injudicious legislation may thus be avoided: if an agitation for legislative measures can survive the appointment and Report of a Commission of Inquiry there must, evidently, be something in it. The means by which such a body obtains its information may also be not altogether the best ones. Evidence is obtained from persons who are presumed to know something about the matters undergoing investigation, and it has always been the case that reliable information has been obtained in this way. But the evidence of many witnesses has certainly been prepared without that sense of responsibility and care which, as a rule, characterises the publication of a memoir by a scientific man who knows that all the world may read and criticise the results of his work. It is also the case that the opinions of men, who only by accident of official position are interested in the inquiry, are invited. Further, a Commission has not always among its members men who are accustomed to weigh the value of evidence, or who possess sufficient technical knowledge to cross-examine with success. Finally, it is rarely the case that a Royal Commission or Committee of Inquiry has instituted for itself adequate scientific or statistical investigation.

All of these defects have been traced in the Reports of the Sewage Commission. First of all it was appointed in 1898 and has thus sat throughout one complete reign and parts of two others. When all charitable allowances are made for the complexity of the matters with which the Commission dealt, this lengthy deliberation can hardly be excused. Among the Commissioners were some scientific men and associated experts of high attainments, yet one can hardly fail to be disappointed by their contributions to the results of the inquiry. The Commission did indeed institute a series of scientific investigations the results of which are included among its publications, yet it cannot be said of this work that it has done more than state a series of problems of economic and scientific interest, and suggest the possible lines along which these problems may successfully be attacked. Above all it cannot be said that the Commission stimulated scientific investigations to the extent that the magnitude of the interests concerned requires investigation. Finally, the only apparent result of its Reports appears to have been the institution of machinery dealing with the problems in question in an unsatisfactory manner, in so far as the very necessary foundation of thorough scientific research has not been attempted, nor is at all likely to be attempted.

It is from this point of view, the tendency to set up administrative machinery which is likely to have no adequate scientific counterpart, that I propose to consider the Sewage Commission's Report.

First of all let us notice the legal and natural conditions and problems that have to be considered, in so far as the pollution of the sea and the fishing grounds is concerned. We have to reckon with a population, inhabiting the sea coast and the immediate neighbourhoods of the great estuaries and rivers, that has been rapidly increasing and is likely still to increase. The result is that an increasingly large volume of domestic sewage and manufacturing effluent is yearly entering tidal waters. The difficulties in dealing with this discharge of sewage are twofold: (1) defective administrative machinery, and (2) defective methods of rendering the effluents harmless. We may, rather briefly, first consider the administrative machinery. The pollution of truly fresh-water rivers and lakes can (theoretically at least) be prevented by the applications of the provisions of the Rivers Pollution Prevention Acts, but these do not apply

to tidal waters, and therefore whatever virtue may reside in them cannot be exercised so as to prevent or minimise the pollution of the sea. It is true that the Local Government Board may declare a tidal river to be a river or watercourse within the meaning of the Rivers Pollution Prevention Acts, but this power has been exercised very reluctantly and sparingly for some reason or other. These statutes do not, therefore, help us. The Sea Fisheries Regulation Acts seem, at first sight, to promise better results, for they prohibit the discharge into tidal waters of any substances which are detrimental to fish-life. But we find that this prohibition is so qualified as to be useless. If the discharge is that of a manufacturing effluent it can usually be dealt with under these Acts, but such cases are far less numerous than those of the discharge of sewage into estuarine or open sea waters. If a local authority is empowered by Parliament to discharge sewage into sea or tidal waters the prohibitions of the Sea Fisheries Regulation Acts do not apply, and it has been held that the Public Health Acts do confer this power to pollute the sea and the fisheries upon the local sanitary authorities. Also it must be proved that the discharge of sewage into sea water is detrimental to fish-life, and this proof is unobtainable, as a rule. Shellfish, such as oysters and mussels, live well and even " fatten " in sea water containing a large admixture of crude domestic sewage, and so long as the pollution is not injurious to the health of the mussels or oysters it cannot be prohibited by the fishery authorities, even if the other difficulty—that of the supposed statutory privilege of the health authorities to pollute the fisheries—could be overcome. Further, it is the case that the only authority (until very recent times) which could prohibit the fishery for oysters or mussels from a polluted shore was the Fisheries Committees, but these bodies could only do so in the interests of the fisheries, and not in that of the public health. On the other hand the health authorities which *could* intervene for the protection of the public health had no power to prohibit fishery in a polluted area. This was a real *impasse*, not a made-up, comic-opera sort of situation, and the actual attempt of a Fisheries Committee to close a highly polluted mussel bed was prevented (as *ultra vires*) by the Board of Trade. In the present year (1915) this power to close polluted shell-fisheries has been given to the local health authorities but it has been given in

such a way as to render the remedy no real prevention of injurious pollution.

I have referred to these legal matters here since they illustrate one tendency of legislation made by lawyers. The layman who criticises the provisions and mode of operation of Acts of Parliament is usually regarded by the lawyers as an amateur and a meddler. But one can hardly fail to see that the study of the laws and the forms of laws made by man removes the mind from the contemplation of *real natural things and conditions*. Legal subtleties and niceties of procedure; the inclination to find restrictions and limits to human activities; and the creation of an altogether artificial environment—these are unfortunately rather typical products of the legal-administrative mind. "Unpractical" and visionary though he may be described as being, the working scientific investigator is still in contact with the real things of life and their physical environment.

I turn now to the latter aspect of our subject, that is, the consideration of the means, extent, and effects of the pollution of the sea-fisheries. Now what may most generally be regarded as the sea-fisheries are hardly at all affected by the discharge of sewage into the sea. There are indeed parts of the north-west coast of England where the extensive weathering of slag from ironworks has produced a coastal margin of sea bottom detrimental to most forms of marine life, but this is very local and unusual. The inshore and deep-sea trawl and line fisheries are quite unaffected, and there can hardly be said to be any clear evidence of the dissemination of disease by means of sewage-infected fishes. There are, indeed, some cases where rather extensive outbreaks of enteric fever have been traced to fish, such as plaice, obtained from fried-fish shops in the East End of London, but in spite of the evidence of an investigation conducted in a very brilliant and accurate manner one can hardly accept the conclusion that the fish were infected *in situ* by sewage of human origin, and it would appear that some other source of dissemination of the disease must be expected. Neither are crustacean shellfish—crabs, lobsters, prawns or shrimps—likely to become infected by intestinal bacteria of human origin, and we are practically restricted to such molluscan shellfish as oysters, mussels, and cockles for the carriers of infection received from sewage-infected estuarine water.

Such shellfish are not found in economic abundance on the shore, nor beneath the shallow waters fronting the open sea. The shallow water in the great bays and estuaries, such as the Wash, the Tyne, the Thames, the Mersey, Morecambe Bay and so on, are the great breeding and rearing grounds of these animals. They require, as a most favourable environment, a sea bottom just above and below the margin of extreme low water of springtides, that is, shallow water possessing a moderately high temperature in the spring, summer, and autumn months. They flourish in water of low salinity (10 to 30 per mille of total dissolved solids; normal high-seas water is about 35 per mille), so that the fresh water brought down by rivers is an important factor in their development and growth. The low-salinity water is itself a factor, but it is also the case that the relatively large proportion of dissolved organic matter contained in river water, and derivable from the washings from cultivated land, is both a direct and an indirect factor: directly inasmuch as these shellfish are probably capable of assimilating dissolved organic matter, that is, of living saprophytically, and indirect inasmuch as the soluble organic matter encourages the growth of diatoms and microscopic algæ, and these organisms then form the food of the shellfish. It is just such estuarine water that contains a relatively large proportion of sewage matters, either crude and recently discharged, or already partially resolved by bacterial action into soluble "albuminoid" substances, or even into salts of nitrous or nitric acids. Into all the great estuaries and bays there open rivers which drain land areas thickly populated and all of which are, to some extent, charged with sewage matters.

A relatively high proportion of sewage, even such a proportion that the polluting matter is recognisable without resort to chemical or bacteriological analyses, does no harm to mussels, oysters, or cockles—indeed, the former molluscs, at least, grow very rapidly in such sewage-contaminated water, and one may often see large and "well-fed" mussels growing within a few feet of the mouth of an outfall sewer. This is true, but to a less extent of oysters and cockles also. The natural conditions are therefore such that these shellfish tend to establish themselves in such areas of foreshore and very shallow estuarine water as receive great volumes of fresh water coming either from cultivated land or from the river water highly charged

with the domestic sewage of great human communities. Even if we had no direct evidence that they were capable of acting as the carriers of such diseases, as enteric fever, which are spread by water-borne infection, there would still be an *à priori* case for this assumption.

There is, of course, a strong body of evidence that establishes the fact that all three species of shellfish do actually convey enteric fever. Oysters, mussels, and cockles share with fruit, milk, and vegetables the condition of being foods that are eaten raw. The shellfish are, indeed, cooked to a certain extent, but large quantities are eaten in this country just as they are taken from the fishing grounds. Oysters are nearly always eaten in the uncooked condition, yet the risk of the transmission of epidemic disease attending the use, as human food, of this mollusc is now relatively small, and this is because the higher value of the oyster has led to much greater care in the selection of storing places for the relaying of the shellfish before being sent to the market. The cockle is rather less exposed to sewage pollution than the mussel, but it has been shown that it can be a cause of conveyance of enteric fever. Mussels are the most abundant, by far, of the three species of molluscs and they are the cheapest. Enormous quantities are (or used to be) sent to the poorer quarters of the great towns, and there is now little doubt that disease is to be traced to their consumption.

Various ailments can so be traced. Mussel-poisoning—a rather obscure affection, not well described—may result from the consumption of healthy unpolluted mussels: it is due to the presence of a toxin (mytilotoxin) which is apparently normally present in the tissues of the animal, but which affects only a small proportion of people displaying some kind of idiosyncrasy. Gaertner-poisoning, also produced by eating raw mussels, seems to be due to a bacterial infection; it is characterised by diarrhœa appearing very soon after consumption of the shellfish. Finally there is enteric fever, which appears after an incubation period of about a fortnight, but which may be much less in the case of mussels acting as the medium. Now the evidence of the conveyance of enteric by shellfish taken altogether is fairly strong, but taken case by case it does not seem to be easily established. Practically the only evidence that a particular case of enteric fever is to be traced to the consumption of shellfish is that the patient consumed

these articles of food within a period antecedent to the onset of illness consistent with the hypothesis that they were the carriers of the infection. It is usually impossible in such cases to cause a bacteriological examination of the presumed medium of infection to be made. A knowledge of the source from which the shellfish were obtained does not help greatly, since all mussel-beds are sewage-polluted to some extent. Finally it is very difficult to exclude all other possible sources of infection such as water, milk and vegetables, though there are criteria which enable an epidemiologist to distinguish between an outbreak due to water or milk, and that due to the accidental pollution of other articles of food by flies, or by enteric carriers, or by contamination *in situ* as in the case of shellfish. Nevertheless there are " classic " investigations on record where the cause (shellfish) of the outbreak of disease has been traced beyond doubt, and there are other lines of evidence. The residuum of enteric fever in this country—a residuum reducible with very great difficulty in spite of sanitation—points to the presence of some cause or source of infection untouched so far by public health measures. The rise in the incidence of the disease in the autumn and early winter months, immediately after the beginning of the season of fishing for mussels, is also very significant. On the whole one cannot doubt that the consumption of shellfish (raw or cooked, for cooking is a very imperfect means of sterilisation) is a competent cause of the maintenance of outbreaks of enteric fever, yet the application of this thesis in any particular case, and the attachment of legal responsibility to any particular vendor, fisherman, or locality, must be very difficult, or even impossible in the present condition of our knowledge.

It is here that one feels justified in complaining of the very imperfect results of the investigation made by the Sewage Commission and of the equally poor results of public health investigation during the last dozen years or so. It is hopeless to attempt to prove the unwholesomeness of any particular mussel, cockle, or oyster laying by trying to demonstrate the presence of the specific organism of enteric fever in the molluscs in question. *Bacillus typhosus* can only be recognised with difficulty after a laborious analysis. It is very exceptionally that it has been found in shellfish taken from natural layings and its actual occurrence there must be exceptional; for like

other micro-organisms inhabiting the intestinal tracts of warm-blooded animals, it finds an unfavourable habitat in sea water, or in the tissues of cold-blooded marine animals, and it rapidly dies out in such environments. The ordinary bacteriological examination of sea water or marine shellfish is therefore one for the presence of the organism called *Bacillus coli*. This is a microbe which inhabits the large intestine of man, and which is always present in domestic sewage in very large numbers. Recognition of *B. coli* in sea water or shellfish is therefore taken as a proof of the contamination of these media by micro-organisms of human intestinal origin, and their presence is regarded as a proof that the shellfish are unwholesome. *B. coli* itself is not, in general, a pathogenic organism, but its source is such that it *may* be accompanied by much more significant micro-organisms. It does not necessarily point to actual, but rather to potential danger.

Thus the ordinary bacteriological examination of sewage-polluted shellfish simply indicates that these *may* be dangerous. Obviously if the inhabited area drained by a river that discharges in the neighbourhood of a shellfish laying contains no enteric fever, nor enteric fever carriers, no amount of pollution of the shellfish by *B. coli* can be regarded as indicative of dangerous pollution. But in this dependence on the presence of *B. coli* as an index of possibly dangerous contamination the weakness really lies in the inconclusive nature of the tests usually adopted for the recognition of the organism. In spite of a very large mass of evidence, and of a number of special investigations, the Commissioners did not find a generally applicable and unequivocal method of identifying *B. coli* in shellfish and sea water, and it must be contended that they left this question in a thoroughly unsatisfactory condition, as it has turned out. At the present time there is no generally practised method of bacteriological examination for the identification of, and the estimation of the abundance of *B. coli* in shellfish. Each public health laboratory in this country has its own method except in so far that the very inadequate method of Houston has been adopted. The question of the degree of significance to be attached to a bacteriological analysis therefore turns upon whose method was employed, and it must be concluded that the results of the various methods cannot be compared, or at least can only be compared with some con-

siderable margin of error. The fact is that the identification of the typical human *Bacillus coli communis* of Esherich is a tedious and laborious undertaking applicable with difficulty in public health laboratories where large numbers of analyses are made. Often the search for this organism in shellfish or sea water samples gives negative results ; and the not unnatural tendency is for such methods to be employed as will give always *some* positive results. The question now turns upon the degree of certainty (as Mr. Balfour would say) to be attached to the various reactions to be employed. One medical officer of health says that glucose-fermenting organisms may be regarded as being *B. coli* or at least " coliform " or " atypical " members of a group all of which have the same significance for the public health administration. Others say that the suspected organisms must, at the least, ferment lactose. A compromise has been effected by others who employ all the common tests, that is the fermentation of glucose, lactose, cane sugar and milk, and the fluorescence of neutral-red, but who give to each of these tests a numerical value, and then sum the figures so obtained so as to get a measure of the degree of contamination. It is not difficult to see the confusion of thought implied in this extraordinary procedure. Either *Bacillus coli communis* is specific to man, or it is not. If it is specific, and if there are organisms living free in nature, or in the intestinal canals of animals other than man, which resemble it, then the bacteriological analysis must be thorough and conclusive. If it is not specific, the method is obviously faulty. If *B. coli* inhabits the alimentary canals of domestic animals, sea birds and fishes, the significance of its presence in shellfish and sea water is conditional upon the proof that enteric fever is, or is not, communicable from man to these animals and *vice versa*, or at least that a pathogenic organism may pass from man to these animals and persist there, retaining all its pathogenicity.

One is surprised to find how very little real, conclusive, scientific investigation has been carried out along these lines. As I have already indicated, the Sewage Commission, either through its evidence or its special investigations, elicited little that has been of service in the actual administration of the public health with respect to the questions considered here. Less has since been done by the public health laboratories, and the most unfortunate thing is that the apparently very full

Report of the Commissioners has produced an impression in the official mind that the whole subject has been adequately dealt with. Another most unfortunate result of all this imperfect research is the growing scepticism with regard to bacteriological methods—I shall refer to this later. And one is tantalised by the evident fact that exact knowledge of the rôle and distribution of intestinal micro-organisms in nature is only a matter of sufficient well-planned investigation. I have said that remarkably little of such investigation has been carried out in this country since the publication of Part III of the Report of the Sewage Commission. The fact, however, that some investigations of the greatest possible value and promise have been made in this country by A. McConkey, and by Clemesha in India, shows that we need not wait longer for the knowledge that we require than the period necessary to arrange and equip research work. This must not be the traditional laboratory bacteriological investigation of the past, but research into the actual natural conditions under which bacteria exist in nature—research such as that made by Clemesha.

We may consider now how matters actually stand with regard to administration. The Sewage Commission in an interim report published in 1904 (over ten years ago!) recommended strongly the formation of a new central authority and a series of local boards. The local boards were to take cognisance each of a natural drainage area, and to them was to be committed all matters dealing with sewerage, as well as the control of the shell-fisheries in so far as these affected the public health by reason of sewage contamination. The Central Board was to exercise a general control over the local authorities. There was to be a scientific staff attached to each board—a staff the duty of which was apparently to be very much like that of the county and city chemical and bacteriological laboratories at present existing in relation to the Foods and Drugs Acts. It is doubtful whether the Commissioners ever contemplated the provision of a scheme of scientific research pure and simple; and it seems to be the case that they thought that the necessary knowledge of the distribution in nature, the significance, and the means of recognising intestinal micro-organisms was already in existence. Nothing therefore remained to be done but to create administrative machinery and provide the necessary technical staffs for routine analyses and

surveys. The scheme was of course a highly desirable one, and it is regrettable that it was not put into operation before the war. It would have been easy—or at least it would have been possible—to graft on to it the machinery for scientific research into the rationale and perfection of methods of analysis. But apparently the Local Government Board did not, at any time, contemplate legislation designed to give effect to the recommendations of the Commissioners. Pressure was brought to bear upon the Board by the local fishery authorities, and the British Science Guild lent its influence in the same direction, but without any apparent result. Much happened between 1904 and 1914. A series of "scares" followed upon local outbreaks of illness attributed to polluted shellfish, and there was a consequent depreciation of the industry. The late Dr. H. T. Bulstrode made a very searching inquiry into the shellfish layings on the coasts of England and Wales, and his report only made things worse without leading towards remedial legislation. We have seen that the existing local authorities had no power to stop the fishing on obviously contaminated areas, but it was found that a certain amount of power could be exercised by the Fishmongers' Company and by the markets committees in the provincial towns. The result was that analyses were made by the Fishmongers' Company and inspections were carried out, and as a result of these measures mussels and cockles from undesirably polluted layings were prohibited from being sold at Billingsgate and certain provincial fish-markets. In the case of the latter these prohibitory measures are, perhaps, irregular, and in any case nothing could prevent the sale of polluted shellfish to retail shops if this were effected otherwise than through the publicly controlled markets. It is doubtful if this procedure did any real good, and in most cases the methods of sampling and bacteriological analysis did not enable a distinction to be made between contamination acquired naturally on the shellfish beds and that which may have been acquired after removal of the molluscs from the fishery, perhaps as the result of storage in insanitary premises. At any rate the net result of the measures just indicated was to lead to a great reduction in the produce of the shell-fisheries and much inconvenience and hardship was doubtless caused locally.

The result of all this confusion was finally the Shellfish Regulations issued, under Order in Council, by the Local

Government Board in March of 1915. Legislation made by lawyers seldom presents any real innovations, and its features are regulated by precedent to a large extent. In this case the form of the regulations was doubtless suggested by a study of the exceptional powers possessed by one borough authority (Blackburn, in Lancashire). They conferred power on the local health authorities to hold inquiry should illness be alleged by the medical officer of health as the result of eating shellfish; and if the local fishermen were unable to prove that the shellfish they gathered were free from sewage pollution the local authority could then prohibit the fishing. Mark that proof of the allegation that particular shellfish caused disease need not be given by the local authority closing the fishery, nor need the latter necessarily satisfy themselves that the natural conditions on the layings were dangerous to the public health. The onus of showing that the fishery was free from pollution lay with the fishermen. The local authority was, indeed, directed to make inquiry into the natural conditions of the fishery, but what adequate investigation (for this is always a difficult matter even for the expert) could, as a rule, be made by the ordinary medical officer of health and sanitary inspector of the usual small urban district council? Note also that bacteriological analyses were deprecated by the Board in their covering instructions issued with the Regulations. Here we find the result of the confusion generated by the inadequacy of our knowledge of the natural history of intestinal bacteria and the imperfectly developed methods of applied bacteriology. The very complete investigation into the shell-fisheries of England and Wales made by Dr. Bulstrode in 1907 did not include a single bacteriological analysis. Thoroughly sceptical regarding the value of this method, the Inspector made his inquiry on the basis of a topographical survey and an examination of the epidemiological evidence available for the various suspected layings. The Board took the same lines in the preparation of the Regulations. It is quite obvious that bacteriological methods had fallen into disrepute at Whitehall, but surely that ought to have suggested the urgent need for a thorough scientific investigation. No one who has worked at bacteriological analysis and studied, even cursorily, the literature relating to *Bacillus coli*, *B. typhosus* and their congeners, or who has considered the question of variability and mutability in bacteria, or that of the criteria

for the distinction of species from species, or the question of the validity of the concept of species among bacteria, can fail to see that the proper thing to do was not to discard the bacteriological method but to take steps to amplify and perfect it for the particular purpose in view. Here we have an instance (and it is of particular interest at the present time, for these Regulations must have been drafted during the period of the war) of a Government Department deliberately counselling the disuse of a scientific method. That the method may be a faulty one does not justify the action, for we can be very sure that this faultiness is simply the result of insufficient scientific research. What the Board should have done, when the Report of the Sewage Commission had been fully considered, was to take immediate and ample measures to institute well-planned investigations.

Let us note, in conclusion, that the whole history of the depreciation of the shellfish industry which has followed upon the interim Report, in 1904, of the Sewage Commission is not only an instance of the failure of administrative machinery inadequately based upon scientific research, but it is also the history of the failure of the fishery and public health administrations to develop what are in reality enormously valuable national resources. I hasten to add that this failure is not so much that of the fishery administrations themselves as that of the inadequate way in which these authorities were provided for by the Treasury, and I speak, of course, of the years preceding the war. The local fishery committees are, in theory, empowered to undertake schemes of shellfish development provided that their programmes receive the sanction of the Board of Agriculture and Fisheries, a sanction which has always been given in a very sympathetic manner. I say "in theory" because it is found that adequate schemes of development are impracticable for financial reasons. The Board itself can also undertake such schemes of development, and even now two such promising undertakings are in progress. Further, both the Board and the local committees have undertaken programmes of scientific investigation having for their object the further development of the shell-fisheries. This investigation—which, one must insist upon, is absolutely essential—is, of course, altogether inadequately planned. There the matter remains, and since no particular public authority can be singled out for blame, one must conclude that the failure to make the

best of our national resources, and the fact that the utilisation of these is even now dwindling away under the repeated enteric fever scares, point to a general lack of earnestness in State Kultur, if we may use that misunderstood term.

Quite a number of years ago Sir William Crookes, in a very remarkable address to the British Association, spoke of the progressive diminution of our supplies of available nitrogen compounds. It is true that chemical science has since then shown the way to make use of atmospheric nitrogen, but it is still true, as the address insisted, that enormous quantities of combined nitrogen run down the drains into the sea and are lost. How practicably to make use of this nitrogen waste was then, and is still, a difficult problem. Schemes of recovery by means of the treatment of sewage, or the application of the latter to the land have not, so far, been very successful, and one therefore grasps all the more eagerly at the suggestions outlined in this paper. Every estuarine sewage-polluted sea-area contains nitrogen compounds which are capable of utilisation by oysters, mussels, cockles and other shellfish either directly or as the result of intermediate metabolic processes. Even now it has been shown to be possible, on the large scale be it noted, to increase the yield of a shellfish bed enormously by transplantation of the molluscs into sewage-polluted waters. The subsequent removal of sewage bacteria so taken up by the animals as an accident of their feeding can also be ensured by redeposition for a day or two at the most in clean sea water. All these measures are practicable and have, indeed, already been undertaken; and only sufficiently planned scientific investigation and industrial enterprise (but always scientific research) are necessary to make them generally applicable. It will, for ever, be a bitter reflection that expenditure on the grand scale should have been so willingly sanctioned by the nation for war-purposes, while the expenditure on scientific and industrial research, infinitesimal as it has been by comparison, should have been so niggardly sanctioned. The necessity of our national existence has made the former expenditure an imperative one; but, thinking far ahead, and in a general way, is not the latter expenditure just as imperative?

FLINT FRACTURE AND FLINT IMPLEMENTS

By J. REID MOIR

The correct determination of human flaking upon a flint has always been a vexed question with archæologists. The difficulty arose apparently, in the first place, with the discovery of the Neolithic axes and arrow-heads, as the late Sir John Evans (*Ancient Stone Implements of Great Britain*, 2nd edition, pp. 56 and 362) mentions that in certain old books these specimens are described respectively as "Thunder-bolts" and "Elf Arrows," clearly proving that a supernatural origin was accorded to them. Though we have no evidence that advanced archæologists were in existence in those somewhat remote times, yet it seems feasible to suppose that even then there were some unable to accept the supernatural theory and who claimed that the flint axes and arrow-heads represented the work of human brains and hands. Be that as it may, the fact remains that no one has for many long years disputed the human origin of these particular flaked flints, which are now universally accepted as affording conclusive evidence of human intention.

But the condition of quiescence so long prevailing in prehistoric circles was rudely shattered by the discoveries of Boucher de Perthes in the valley of the Somme. About the year 1841 it became noised abroad that definite flint implements of man had been found in undisturbed river-gravels of this district, associated with the bones of extinct animals, and that these discoveries afforded evidence of the existence of man at the remote period when these animals existed, and prior to the time when the ancient gravels containing their remains were laid down. Immediately these discoveries were made known to the scientific world, archæologists were plunged into a most violent and bitter controversy regarding the flaked flints discovered. The majority of authorities made haste to repudiate the claim of Boucher de Perthes, and asserted that the flints

had been flaked by natural causes, and if not, then they had been made by the workmen at the pits, who had surreptitiously inserted them in the gravel from whence they were removed by the enthusiastic but misled archæologist. The various and extraordinary arguments put forward against the human origin of the palæolithic implements discovered by Boucher de Perthes are fortunately preserved and can be studied in a book entitled *The Antiquity of Man*, consisting of papers selected chiefly from the *Transactions of The Victoria Institute* (Elliot Stock, 62, Paternoster Row, London, E.C.).

There were, however, a few men who, having seen and handled some of Boucher de Perthes' specimens, stated they were in agreement with his opinion as to their human origin.

But these opinions made little headway, and for many years the majority of archæologists held to the belief that the flints found in the valley of the Somme had been flaked by some unknown natural forces.

Gradually, however, opinion began to change. Investigations carried out in river-gravels in this country and elsewhere established the fact of the occurrence in these deposits of similar flaked flints to those found by Boucher de Perthes, and at length, after much searching of heart on the part of the archæological world, palæolithic man came into his own. With the general acceptance of the palæoliths, a condition of comparative tranquillity once more descended upon the question of man's antiquity; and though there were some bold spirits who held that an elaborately flaked palæolithic implement could not, in the nature of things, represent man's first effort in the fashioning of flint, and that in consequence his earlier efforts would probably be found in deposits more ancient than the palæolithic river-gravels, yet these were but "voices crying in the wilderness," and little heed was paid to them. The mutterings of the coming storm, which was to sweep away the orthodox limits set to the antiquity of the human race, were first heard from Ightham in Kent, where Mr. Benjamin Harrison, having found palæolithic implements in the oldest river-gravels of his district, began to investigate still more ancient deposits situated on the high plateau of Kent. These investigations resulted in the discovery of a series of rough flints exhibiting flaking along one or other of their edges. This discovery and the announcement that several archæologists, including the

late Sir Joseph Prestwich, regarded these specimens as affording evidence of the former existence of a race of beings much more ancient than the makers of the palæoliths, initiated another period of unrest, and for the third time the archæological world was grievously disturbed, and the usual division of forces took place. Those who supported Mr. Harrison asserted that the flaked flints he had discovered were undoubtedly the work of man, and represented the type of implement which they would expect to find in such an ancient deposit; while his opponents contended that the flaking on the flints was undoubtedly due to natural pressure or percussion and therefore not indicative of human handiwork. Since the publication of Mr. Harrison's discoveries, a number of pre-palæolithic implements which seem to link up the primitive Kentian implements with the later palæoliths have been found in various Pliocene and early Pleistocene deposits in East Anglia, and these specimens, though accepted by an ever-widening circle of prehistorians, have in their turn been regarded by certain investigators as representing the work of nature. It will thus be seen that the science of prehistory has advanced only by slow and somewhat painful steps, and it may be of interest to inquire into the causes of this tardy progress.

The prolonged and heated controversies which have inevitably followed any new discoveries of flaked flints tending to extend the antiquity of the human race indicate clearly that the acceptance or non-acceptance of these specimens has not been actuated by any clear and definite scientific knowledge of the differences exhibited by flints flaked by man or by the unguided forces of nature.

If such knowledge had existed the controversies which have raged round the neoliths and palæoliths, and which are now being conducted in regard to the pre-palæoliths, would either have not occurred at all or, if commenced, would have been of a less prolonged and heated character. But in the absence of definite scientific data, other and less satisfactory means for arriving at a decision had to be employed. A prehistorian expressed himself to be in favour of accepting or not accepting any given flaked flints, simply by reason of his preconceived views and personal prejudices, and the majority of investigators are still swayed by such unscientific influences. In the long run what is known as "common sense" prevailed in regard to

the neoliths and palæoliths, and there seems but little doubt that the pre-palæoliths will also be finally accepted on the same grounds; but the processes of common sense are slow and not necessarily scientifically adequate, and other and more satisfactory means of determining whether a flint has been flaked by man or by nature ought to be available. It appears that the only method likely to lead to definite and satisfactory results would be to conduct a series of experiments in which flints would be subjected to the effects of fortuitous percussion, and pressure, and to critically compare the fractures so produced with others caused by a hammer-stone held in the hand, and used in the manner in which ancient man probably used it. It might be urged that it is impossible to simulate experimentally the effect of natural forces upon flints, but on the other hand it seems reasonable to suppose that fortuitous blows and pressure do not differ fundamentally, whether brought into action by nature or in experiments conducted by a human being. The "mightiness" of natural forces cannot certainly be imitated experimentally, but it has been ascertained that flints will only stand, without disintegration, a certain amount (by no means very large and easily equalled in experiments) of pressure and force of blows, and that in consequence when the natural forces in operation are too mighty, the flint must be shattered and reduced to fragments. The author concludes, therefore, that the experiments he has conducted in the flaking of flint are of value in this discussion, and though further researches may be necessary before the problems connected with human and natural flaking are finally and completely solved, yet he feels that the results already obtained may help to dispel some of the doubts and difficulties, and thus place the question of the antiquity of man upon a firmer and more scientific foundation. It is proposed in the first place to tabulate and describe some of the details of flint fracture, to which reference will be made in the description of the experiments carried out. The nature of the experiments in which flints were subjected to fortuitous percussion, and the conclusion arising therefrom, will then be described, and finally in a like manner the experiments in which fortuitous pressure was employed.[1]

[1] A complete series of the flints fractured experimentally can be seen and examined in the Department of Ethnography at the British Museum (Bloomsbury).

Details of Flint Fracture

Striking Platforms.—When an ordinary nodule of flint is selected with a view to produce an implement from it by flaking, it is necessary, owing to the difficulty of removing flakes from a rounded surface, to break off a portion of the nodule, so that a flat surface is produced, upon which blows removing flakes can be struck with precision. The flakes which are removed will exhibit, in the immediate vicinity of the point where the blows fell which detached them (known as "the point of impact"), a portion of this flat surface, and this is termed the "Striking Platform."

The Cone of Percussion.—When a certain type of blow is delivered in the centre of a flat surface of sound flint, and the portions of this surface surrounding the point of impact tapped gently with a hammer, these portions will generally fall away, revealing a more or less perfect cone of flint. This is known as "the cone of percussion," and is due to an inherent property in the flint itself to fracture in this manner. This peculiar method of fracture is also found to be present in some other substances, such as glass, obsidian, etc.

Positive and Negative Cones of Percussion.—When a flake exhibits just below the point of impact a conical protuberance, and the block from which it was struck a corresponding hollow or depression, the former is known as the "positive," and the latter as the "negative" cone of percussion. The detached flake may as often exhibit the negative cone and the parent block the positive, but that does not affect the validity of the description given above.

Éraillures.—If a number of flakes removed by blows are examined it will be seen that many of them exhibit on their bulbar surfaces (that is, the surfaces upon which the positive cone of percussion is visible) a scar, where a small and isolated flake has become detached. This small flake is apparently removed simultaneously with the detachment of the larger flake, but the exact reason for its removal does not appear up to the present to have been satisfactorily explained. This éraillure or "mark" is sometimes seen upon flakes removed by pressure (fig. 1).

Ripple Marks or Conchoidal Ripplings.—These ripple marks, which are often seen upon flakes and implements of human manufacture, and also upon flints flaked by fortuitous means

(they appear to be more prominent and frequent upon the latter), are due to the tendency of flint under certain conditions to fracture in a conchoidal or shell-like manner. It seems that these conchoidal ripplings are somewhat analogous to the ripples produced upon a surface of water when some heavy object is thrown on to it. The water is "jarred" by the impact and ripples produced, and it may be that certain blows jar the flint and so produce ripplings on the fractured surface. Conchoidal ripplings are sometimes formed when a flint is fractured by pressure.

Fissures.—If a series of flakes is examined it will be noticed that their surfaces exhibit a series of fissures of varying length and width, which radiate from the point of impact. Some of

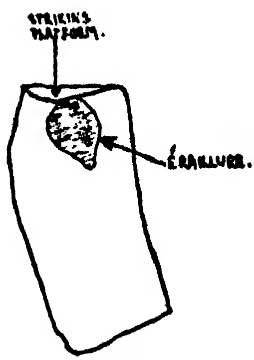

Fig. 1.

these markings are easily visible to the naked eye, while others require a strong lens to discern them. It seems that their presence can be accounted for by supposing that when the flake is removed the flint is in a sense torn and that these markings represent the tears produced by the cleaving effect of the blow. Fissures are sometimes produced upon a flint which has been fractured by pressure.

Truncated Flakes.—When the manufacture of a flint implement is commenced, large flakes are removed from the nodule of flint, which in the further process of flaking get reduced in size or truncated. Thus it is possible to observe the remains of flakes which have been thus reduced in size on various portions of the surfaces of many implements. These flakes are termed "truncated flakes."

Opposing Cones of Percussion.—Under certain conditions a

flake removed by pressure may exhibit a well-marked cone of percussion at each end. This is due to the flint having been squeezed between two hard objects, and the cones represent the points at which, respectively, pressure and resistance acted. These cones are known as "opposing cones of percussion."

Experiments with Flints subjected to Fortuitous Percussion.—Seven or eight flint nodules of various shapes and sizes were placed in a sack and violently shaken about for some little time, and in such a manner that the flints were in almost continual collision. It was found that only the stones of a certain shape—and conforming more or less to a wedge in form (the thin edge of the wedge being usually attacked)—were flaked in this process. The rounded flints in the sack were not flaked, but escaped with

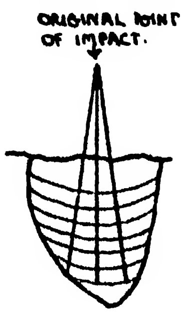

FIG. 2.

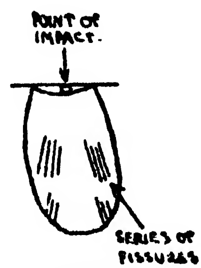

FIG. 3.

contusion of their surfaces only. A careful examination of the flaking produced by these fortuitous means, and a comparison of it with that due to human blows, appears to show that there is a fundamental difference between the two methods of fracture. This difference is, primarily, the angles at which the flakes have been removed. The method adopted to ascertain the direction in which any given flake has been removed from a flint is as follows: A line is drawn down the centre of the flake and at right-angles to the ripple-marks (these in many cases require locating with a good lens). Then two other lines are drawn one on each side of the central line, and also at right-angles to the ripple-marks as they curve upwards. The three lines are then continued to their point of junction, which point indicates the area upon which fell the blow that detached the flake (fig. 2).[1]

[1] This method is adopted only when dealing with a truncated flake; when the actual bulbar cavity is present such a method is not needed.

The correctness of this method can be checked by observing the direction of the small fissures or "splits," which gives an additional indication of the angle at which the flake was semoved (fig. 3). Careful outline drawings were then made of the flints flaked in the sack experiment, and also of those shaped by human blows.

Each flake area was accurately drawn and the direction of the blows responsible for the flaking indicated by arrows.

When this was done it was seen that the flints from the sack had had their flakes removed at divergent angles to the edge of the stone (fig. 4), while those fractured by human blows had had them removed at a constant angle (fig. 5). It does not appear possible that fortuitous haphazard blows would remove flakes at a constant angle to the edge of any flint,

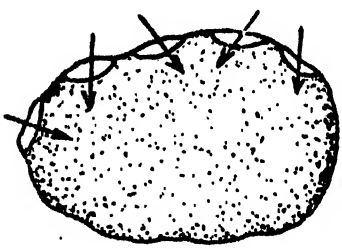

FIG. 4.

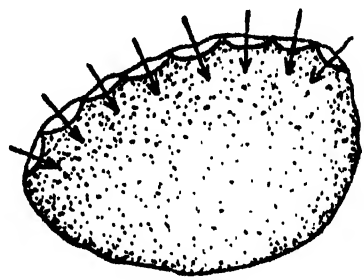

FIG. 5.

neither does it seem reasonable to suppose that any human flaker would go to the trouble of making a sharp edge by means of divergent blows, as the result would be much less satisfactory than one produced by the normal method. It appears then that we have here a good and simple means of distinguishing between the work of man and of nature.[1]

A further examination of the specimens fractured in the sack experiment demonstrated that the flakes which had been removed differed in appearance from those due to human blows, this difference being—

(*a*) The "squatness" of the fortuitous flakes as compared with those removed by human agency (fig. 6).

[1] The author has examined and made flaking diagrams of a large number of flints found upon sea-beaches, and the flakes removed from these specimens by fortuitous battering are at markedly divergent angles.

(*b*) The fact that the former had cut deeper into the flint, causing a step or ledge to appear at the point of their final separation from the parent block (fig. 6); and

(*c*) The occurrence of numerous prominent ripple-marks upon nearly all of the flakes produced by fortuitous blows, as compared with their comparative scarcity upon the "human" flakes (fig. 6).

The explanation of these differences may be explained as follows:

In intentional human-flaking, a flat striking platform is produced, and blows are delivered on one or other side of this platform, and in such a manner that the flakes removed are generally the reverse of squat, being of greater length than

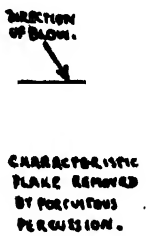

FIG. 6.

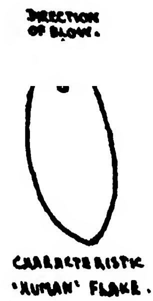

FIG. 7.

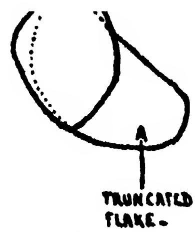

FIG. 8.

width, and thinnest at the point of their final separation from the parent block (fig. 7). Such flakes are also generally free from prominent ripple-marks, due, in the author's opinion, to the fact that the blows which detached them were delivered almost vertically to the striking platform. In flaking produced by fortuitous percussion the great majority of the blows fall, not upon the side of the edge of the flint, but almost directly upon the edge itself, and it is known that blows so delivered remove flakes squat in form and productive of a step or ledge at the point of their final separation from the parent block.

It has been ascertained that consistently to remove flakes exhibiting prominent ripple-marks upon their surfaces, it is necessary to strike the flint an oblique blow. As has already been mentioned, a blow delivered vertically to a flint surface

will remove a flake almost devoid of ripple-marks, and it is supposed that such a blow has less jarring effect upon the flint, and consequently produces less rippling of the fractured surface, than one delivered in an oblique direction. The explanation of the frequent occurrence of prominent ripple-marks upon fortuitous flakes, and their comparative scarcity upon those produced by human blows, may be as follows. It may be assumed that there are 180 angles at which a flake may be removed from any given length of edge, but it is only more or less vertical blows, delivered at a few of the higher angles, that will remove flakes exhibiting insignificant ripple-marks. Thus in fortuitous haphazard blows the chances are that the greater number of blows will be delivered at the lower angles, which are oblique to the edge of the flint, and which, as has been shown, remove flakes exhibiting prominent ripple-marks upon their surfaces. It seems, then, that in the shape of the flakes, their character at the point of final separation from the parent block, and the presence or absence of prominent ripple-marks upon their surfaces, afford further criteria for determining whether any particular series of flints has been flaked by man or by nature.[1]

It was noticed that the edges of all the wedge-shaped stones fractured in the sack experiment showed a tendency to assume a sinuous outline due to blows having fallen upon either side of these edges.

The majority of the stones exhibited this sinuosity in an incomplete manner, but one specimen showed a markedly sinuous edge 4½ inches in length. A critical examination (based upon the tests already described in this paper) of the flaking to be seen upon this flint demonstrated clearly its "natural" origin, but another characteristic of the flaking was detected which seems to still further differentiate it from the work of man. It was found that the edge of this flint, though only 4½ inches in length, exhibited seventeen truncated flakes (fig. 8).

On the other hand an examination of various neolithic and palæolithic implements with sinuous edges demonstrated that the average number of truncated flakes on an edge 7 inches long is only six.

[1] In arriving at a decision it is necessary to examine not less than twenty or thirty specimens; a lesser quantity examined might lead to erroneous conclusions.

The explanation of the greater number of truncated flakes upon an edge produced by fortuitous blows as compared with one formed by human agency may be that in the former case the rain of blows is almost continuous and the flint is continually being reflaked and truncated flakes formed. It is, however, somewhat difficult to imagine any human flaker being so inept at his work as to be compelled to form seventeen truncated flakes in the production of an edge 4½ inches in length.

The author considers therefore that the proportion of truncated flakes upon a flint may furnish a further criterion for the determination of human as against natural flaking.[1] Various other experiments in fortuitous percussion, such as the hurling of a flint upon others lying on the ground, were

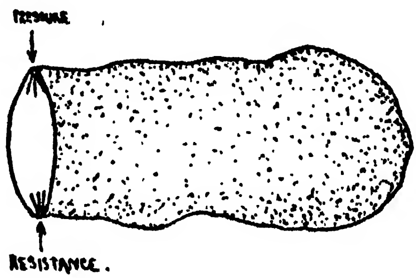

FIG. 9.

carried out, and the results found to be similar to those obtained in the sack experiment.

Experiments with Flints subjected to the Effects of Fortuitous Pressure.—Experiments were conducted with an adapted letter-press, and a differential screw press, with which considerable pressure was obtained.

It was found that when a suitably shaped stone was placed upon the floor of the press, and another smaller stone placed on the top of it and near its edge, a flake could be removed from the lowermost flint by forcing the ram of the press down upon the uppermost. Such flakes exhibited "opposing cones of percussion" (fig. 9), due to the pressure upon the upper portion of the flint being equalled by the resistance afforded

[1] In the re-sharpening of many neolithic scrapers and Moustier (palæolithic) racloirs a number of small truncated flakes were produced, but these are not of the [illegible] order, or comparable to the truncated flakes described above.

by the floor of the press. The presence of a cone of percussion at each end of these flakes differentiates them from those detached by human blows, on which only one bulb of percussion is visible. When a duster was placed upon the floor of the press so that the flint rested upon a soft basis, it was found that a flake could be removed, but only with the exertion of considerably more pressure than was needed in the former case. It was noticed also that such flakes exhibited only one cone of percussion, at the point where the small upper stone impinged upon the flint, as in the case of flakes removed by human blows. The majority of these cones of percussion, however, are flat and appear to be only partly developed, and in this respect they differ from the majority of cones of percussion produced by human blows. Pressure flakes, too, seldom exhibit well-marked éraillures, while the fissures and ripple-marks, if present, are of a different order from those present upon humanly-struck flints.

These differences are readily observable when a representative series of each kind of flake is put out for examination, and appear to be due to the different nature of the two fracturing forces. The human flaker is able to guide the line of fracture, within limits, in any direction he pleases, while in the case of pressure the flint is squeezed until it can stand the strain no longer, and finally fractures along the line of least resistance. Future investigations may perhaps modify this explanation, but at present the author regards it as affording a likely reason for the difference, which undoubtedly exists, between flakes removed by pressure and those detached by human blows. Experiments were also conducted in which a hard resistant stone was placed upon the floor of the press, and another with a sharp edge placed against it in such a manner that the descending ram would cause this sharp edge to move under pressure over the surface of the other specimen. By this means a flaked hollow was produced in the sharp edge of the uppermost stone.[1] Several of these hollows were produced and compared with others formed by human blows. This comparison demonstrated that there was a marked difference in the appearance of the two series of hollows.

It appears that when the sharp-edged flint is being moved

[1] A similar result can be obtained by placing a sharp-edged flint upon another stone lying on the ground and turning one's heel round on them, under pressure.

under pressure over the surface of the underlying stone very thin flakes or scalings are removed. This is due apparently to the fact that the pressure is able only to attack a very narrow area of the edge of the moving stone, and in consequence only very thin flakes are removed.

In flaking produced by human blows, it is impossible to strike so near to the edge of the flint as to remove flakes as thin as those detached by pressure, and a hollow flaked by the former means always appears, and is, more uneven and less finished than one produced by pressure.

The pressure hollows appear " smoother " than those of the other kind, because, the flakes being thinner, the ridges intervening between them are less prominent (fig. 10).

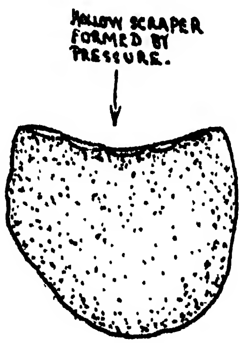

FIG. 10.

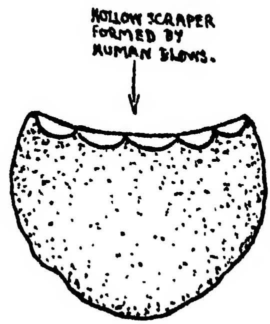

FIG. 11.

In the case of the human blows, which cut more deeply into the flint, and remove thicker flakes, these ridges are correspondingly more marked (fig. 11).[1]

The foregoing experiments in fortuitous percussion and pressure have been repeated many times and the results found to be uniform in each case.[2] It is to be hoped that prehistorians

[1] The author has lately examined the Eocene flint-bed resting upon the chalk at Bramford, near Ipswich, where many specimens have been fractured by natural pressure in the bed in which they now lie. This examination has shown that the fractured Eocene flints reproduce in a marked degree the characteristics of those fractured by pressure in the experiments described. See *Proc. Prehis. Soc. of E. Anglia*, vol. i. Part IV. pp. 397–404.

[2] Further experiments are in progress, and if the results obtained throw any further light upon the problems discussed in this paper, these will in due course be published.

will be able in future to give more attention to the question of the natural and artificial fracture of flint as affording the only means of arriving at satisfactory conclusions regarding the "humanity" or otherwise of the specimens which are the subject of their study. Mere speculations as to the capabilities of man or nature to flake flints in the remote past are, in the author's opinion, of little value.

LEGITIMATE PRESS CRITICISM OR TREASONABLE PUBLICATION; WHICH?

BY A LEGAL CORRESPONDENT

ON several occasions, writings in the press have given rise to uneasiness lest the limit of criticism permissible to a loyal citizen has been dangerously approached, if, indeed, it has not been crossed.

Undoubtedly the press is justified in directing attention to defects in administration if no detriment to ourselves or our Allies results. If, however, weakness in organisation is laid bare in the newspaper, or lamentable shortness of men or *matériel* is disclosed in print, or mistaken strategy is proclaimed in periodic literature, the action may clearly assume a treasonable hue. As between flagrant treason and patently innocuous discussion there is no question of right or wrong; but in some cases great difficulty is encountered in the attempt to distinguish justifiable criticism from treasonable pamphleteering. In difficult instances, by what standard are we to judge whether the border-line has been passed?

If the result of a publication leads to decisive improvement, an individual may pardon what otherwise in his own mind he would reprehend. On the other hand, timid folk might defer wholly to authority and condone a total suppression of criticism. But so long as criticism is permitted, criticism may proceed to limits and in the end perhaps enhance the very evils against which it is directed. Where clear distinction is seen between legitimate discussion and enemy-aiding criticism, no excuse can avail the offender even though his motives be of the highest; but in border-line cases where guidance is wanting, how can it be determined whether an offence has been committed? In view of keen competition between newspaper and periodical, and the intense desire evinced by the public to learn the causes of failure and to discuss how best they may be avoided, the extent to which criticism may safely proceed is of deep concern.

Few desire to obtain this knowledge through an experience of trial and imprisonment, and yet somewhere or other, either directly or by implication, the region of safe criticism must be outlined, dimly perhaps, or else discussion of matters of the gravest import must be pretermitted.

In the presence of difficulty in determining what is justifiable and what culpable, appeal must be made to law, so that the matter at the outset becomes an affair for the lawyer. And this leads naturally to a consideration of the powers conferred upon the military authorities by the Defence of the Realm Acts, and by Regulations made thereunder.

Stated shortly, these powers are well-nigh absolute. The Court of Appeal in the *Zadig* case (*The Times*, February 10, 1916) seems even to have decided that when a person is subjected to non-punitive detention, the writ of *habeas corpus* may be withheld. The only deduction from absolutism is to be found in a pair of notable safeguards. In the first place, an Act of March 1915 gives a civilian the right to demand for alleged major offences a trial by jury before a civil court. Despotic exercise of power is therefore excluded. A further restraint upon the unfettered jurisdiction of an army tribunal is the existence of a democratic Government whereby at any time the powers it has conferred may easily be abridged or suspended or any officer guilty of harsh or arbitrary conduct removed. In the case of "press offences," the definition of which is wide, regulations provide for a reference to a law officer of the Crown before proceedings are taken.

Although a civilian, a British subject, has the right to trial by jury for major offences—unless by proclamation the right is withdrawn—the trial may be *in camera*, when, of course, no publication of the proceedings takes place. Even when the trial is not *in camera*, publication of details may be interdicted.

The result is that the average citizen has little or no precedent to guide him; nor indeed has the lawyer, for he is in no better position. In the words of the great Duke of Wellington, "Martial law is neither more nor less than the will of the general who commands the army. In fact, martial law means no law at all." Criticism issues at the risk of the writer and publisher. True there is the Censor to whom drafts may be submitted; but it by no means follows that all which passes the censorial sieve may be scattered broadcast. Although an

individual conceived that he was committing no offence, yet he might speedily find himself imprisoned for a supposedly innocent, or even meritorious act.

To a considerable extent, then, the law relating to publication of criticism is *ex post facto* and on that account to be deprecated. In this respect, however, it is not dissimilar to the law relating to criminal libel, to which every one is accustomed. In the instance of civil libel there is abundance of principle and precedent upon which to draw, but in the case of criminal libel a statute enjoins upon the judge a course of action which is analogous to the performance of a scientific experiment. Just as the scientific man when confronted with a problem which is not governed by ascertained principles makes a test, so in the case of the libel the judge carries out a test by which he discovers the attitude of the average mind towards the matter with which he is dealing. He submits it to a dozen citizens, who between them are taken to represent the juridical average man and who determine exclusively whether guilt is present or absent. If in the special circumstances of the case before it, the jury considers that the writing was not blameworthy, a verdict of "not guilty" follows. If the jury thinks the libel ought not to have issued, condemnation proceeds. It becomes then a question of what the jury thinks ought not to have taken place; and where questions of "ought" arise, law vanishes, and, as a guide to future action, no court precedent is available.

Similarly, where a British subject, not under naval or military incapacity, is to be tried for a press offence, the jury would consider what the present and immediate circumstances dictate to be reasonable and proper. Guidance is to be expected from military experts, guidance which would naturally vary from time to time and place to place, possibly from witness to witness.

Although it is unlikely that where important issues were involved the evidence of the naval or military expert would be given publicly or subsequently published in the press so that the public could learn what they may or may not discuss or criticise, yet it is clear that the standard by which the citizen has ultimately to adjust his conduct and to measure his action is that which the expert deems right and proper at the time and place and in view of all the circumstances.

The determination then of what is right and proper is of the essence of the whole question, and it may be taken as a postulate that the publication of news, real or fictitious, must be judged by reference to the final object indicated in the Defence Acts, viz. "securing the public safety and defence of the realm." Whether or no in particular instances prosecution should be set on foot raises other considerations. In certain events, popular hostility might be engendered; where failure to convict ensued, action by the Government might be accounted persecution. These considerations, however, need not here be pursued.

To return to the original question—how to distinguish legitimate criticism from treasonable writing. In the absence of precedent, an absence likely to continue, the distinction is that which the tribunal before whom the case is brought chooses to draw, the tribunal having in mind the ultimate object to be attained by the legislation from which its jurisdiction is derived.

Any one then who in writing criticises the action or reflects upon the lethargy of the Government may be called upon to satisfy a court that the criticism did not transcend the bounds which in the circumstances were to be deemed just and reasonable. And what is just, reasonable, or fair comment is to be judged by what the Defence of the Realm Acts, the Regulations, and the naval and military experts indicate as hurtful to the country. It is the securing of public safety and the defence of the realm by which every action is to be judged and to which every consideration must be made subservient.

POPULAR SCIENCE

THE ROMANCE OF RADIUM. Will England or Germany control this Commodity after the War? By G. W. C. Kaye, Head of the Radium Department, National Physical Laboratory, Teddington, and in charge of the "British Radium Standard."

As in diplomatic and commercial spheres of activity, so also in the field of scientific research, Germany has, particularly of recent years, proved an increasingly formidable rival to this country. And nowhere perhaps has this been so markedly exhibited as in the spade-work necessary to discover and exploit the curative properties of radium. It should be our business, when normal conditions are reached, to extend our operations in this very important matter.

The story of radium and its discovery is tinged with the spirit of romance. It is a story unequalled in the world's history as radium stands unequalled at present among the world's metals. Looking back, from the laboratory of Prof. Curie, down a long vista of years one can trace the gradual evolution of a scientific certainty out of the half-formed and crude beliefs of the Middle Ages.

From Figment to Fact

First, working from that period in our history, we find the alchemist, an object of considerable persecution, and yet obstinately pursuing his objective with a remarkable faith, busily striving to transmute the elements. His motive was not perhaps of the loftiest, since it confined itself to a realisation of profit by turning the baser metals into the nobler. Yet that motive represented the foundation upon which was built presently the discovery of radium.

Then we arrive at the advent of chemistry as an ordered science, the foundation stones of which were the immutable and completely independent chemical elements. The orthodox followers of chemistry did not press to experiment the notion of

transmuting the elements, and, had these had their way, perhaps the struggles of the alchemists might have so been brought to a cul-de-sac. There were, however, some who were not unmindful of the alchemists' dream and the hypothesis of Prout, which assumed without absolute proof a primordial substance common to all elements. And these were bold enough to speculate on the problem of the fundamental nature of matter, and to seek an answer to this riddle which has excited interest and taxed ingenuity since the beginning of civilisation. But, in most instances, ordered experiment was dominated by idle speculation; and so it came that only within the last decade or so, true transmutation has been established as an experimental fact. Even in the early 'nineties, it was not infrequently maintained that, the science of physics having put its house in order, further advances could only be looked for along the lines of precise measurement. Such pessimism was, nevertheless, soon to be utterly confounded by a sequence of discoveries such as the boldest imagination had never projected. And indeed no reflecting mind can even now survey the present state of physics without realising that we have not yet extracted a tithe of the potency which lies in the shadowy boundaries of the unknown.

The first stepping-stone to the "new physics" was the discovery of "electrons" by Sir J. J. Thomson at Cambridge in 1897. The discovery resulted in the present accepted theory of the constitution of matter. It paved the way for a ready recognition of the amazing properties of the radioactive elements, then on the point of discovery; and it led to the establishment of a new school of physics which accepted as a creed the theory of the transmutation of the elements. It can be readily understood that the placid existence of chemistry was rudely startled; but, as events proved, the atomic foundation-stones, though threatened, were left undisturbed. What happened was that a fresh storey had been added, though not, as was at first anticipated, at the expense of the stability of the structure.

Meantime, in 1895, Prof. Röntgen, working in Bavaria, had discovered the X rays, and achieved world-wide fame. In 1896, only two months later, Becquerel showed in Paris that uranium salts were spontaneously "radioactive" and, like X rays, could affect a photographic plate even through a sheet of paper or metal. This was followed presently by the crowning achievement of Prof. and Mme. Curie in Paris. They per-

ceived from their experiments that the natural activity of the uranium compounds was largely due to the presence of a trace of some other substance. They set to work on a large quantity —more than a ton—of uranium residues; and, by an almost endless series of precipitations and crystallisations, they finally succeeded in isolating a few milligrams of a salt of a new element of intense activity. In short, they had discovered radium.

A ton of material contains roughly a thousand million milligrams; and the laborious work entailed will stand for all time as a monument to the skill and patience which the Curies brought to bear upon a problem calling for the highest technical skill. Since then, radium has been obtained as a shining white metal; but the name in its everyday use always implies the salts of radium. These are white powders which, in the dark, can be seen to phosphoresce somewhat like a glow-worm.

A Methuselah among Metals

Radium is the most amazing and revolutionary substance ever known to man. Unceasingly it gives out heat, light, electricity, and X rays, all of which is effected entirely at the expense of the energy within the atom. And the atom of radium is unstable. So that, though transmutation—the dream of the alchemist—has in the discovery of radium become a reality, it is limited by this restriction, that the change is uncontrollable by man. We cannot alter by one iota the rate at which radium disintegrates; we have as yet found no means of controlling the stupendous forces within the atom.

We now know radium to be one of a number of unstable intermediate products constituting a family-tree, whose original ancestor is uranium and the last surviving member lead. The lives of these various radioactive elements range from a fraction of a second to several million years. Radium itself is the most energetic member of the family and possesses a long life. Two thousand years pass before its activity sinks to half value.

During the process of disintegration, it emits three different types of rays together with a heavy gas, or "emanation," itself radioactive. These rays are to be regarded as the outward and visible signs of the breaking up of the various atoms. The existence of the rays can be most readily demonstrated by their

property of "ionising," or rendering "electrically conducting" any gas through which they pass. The effect is produced by the shattering of some of the gas molecules by the radium rays: the extent of the ionisation may be measured by means of an electroscope.

It was soon found that, of the three kinds of rays, one (called the alpha rays) was readily absorbed by a few sheets of paper; another (the beta rays) was only stopped by a thick sheet of metal such as lead; while a third (the gamma rays) was still more penetrating.

In the alpha rays we were introduced to a new type of atoms which possesses two features quite out of the common; the first, an electric charge (of positive sign); the other, an enormous speed—some 12,000 miles a second. The alpha rays have also proved to consist of atoms of helium, and their energy is such that their impact against a fluorescent screen is attended by a visible flash of light, as in the spinthariscope of Sir William Crookes. Such experiments provided the first means of counting individual atoms. The expulsion of the alpha ray, it should be added in conclusion, is responsible for the recoil of the remainder of the radium atom, just as a gun recoils after a shot is fired out of it.

The beta ray was early identified with the electron to be the tiniest entity of which we have knowledge. It carries a negative charge of electricity and its mass amounts to no more than about 1/1800 of that of the atom of hydrogen. The ray consists of a considerable number of groups of a variety of speeds, each group of rays moving with the same speeds. These speeds are inconceivably prodigious, the highest approaching within ½ per cent. of that of light—186,000 miles a second. It requires about half an inch of lead to stop particles such as these.

The third great group of rays—the gamma rays—is identical in kind with X rays, which have lately been shown to be an extreme form of ultra-violet light. They are almost certainly conditioned by the expulsion of the beta rays, and as a consequence the radium gives out a "spectrum" of gamma rays, each of the component parts corresponding to one of the group of beta rays. Some gamma rays are less penetrating than X rays; but others are much more penetrating, and will in fact pass readily through six inches of lead. Against such rays it is almost impossible to obtain protection; but, happily for

radium workers, such protection is unnecessary, as the rays pass through the body without any ill effect. In respect of the human body, it is only against the alpha and beta rays that one must and fortunately can guard.

A Costly Commodity

The distribution of radium in the earth's crust is universal; but it no more pays to extract radium from low-grade ores than it does, for example, to separate from the sea-water the gold which we know is contained in it in appreciable amounts. The labour of extraction goes up enormously as the concentration falls. Sir Ernest Rutherford has calculated that the total amount of radium in the earth's crust is of the order of 500 tons; but the majority of this is so diffusely distributed as to be inseparable by any known process.

It is upon uranium ores that the world depends for its supply of the precious metal; and, as the proportion of radium to uranium is always about one in three millions, it is sufficient for the radium prospector to establish the presence of uranium. At present radium is derived from the richer ores of uranium, such as the pitchblende found in Australia and in Cornwall. In Colorado the mineral carnotite is being successfully worked, while Portugal has sent us considerable quantities of radium extracted from the mineral autunite. The first consignment of radium from Australia came over quite recently. It is interesting, finally, in this regard, to note that the lava from Vesuvius is steadily growing more radioactive with each eruption.

It is believed, however, that not much more than one ounce of radium has yet been separated. It will therefore be recognised that it is by far the most costly substance ever yet sold in commerce. Its price indeed has been steadily advancing in consequence of the great demand, and has now reached a figure in the neighbourhood of £20 a milligram, which is at the rate of £600,000 an ounce! Such a high-priced commodity is naturally liable to many commercial abuses, and a prospective purchaser of radium should emphatically insist on its previous standardisation by the National Physical Laboratory at Teddington. The British Radium Standard (which may be found described in Kaye's *X Rays*, published by Messrs.

Longmans) was acquired by the Laboratory in the interests of the many users of radium in this country.

Radium and the Geologist

It was at one time supposed that all the elements would prove to be radioactive in greater or lesser degree; but the trend of observation and experiment is that this property is confined to relatively few elements. One piece of evidence which has led to this result is to be found in Prof. Joly's work on the pleochroic halos in certain kinds of mica. These halos are seen as small coloured patches under the microscope, and were long an object of mystery to the geologist. Joly has recently shown, however, that the halos are due to coloration of the mica by alpha rays expelled from a minute nucleus of radioactive matter. But, at the same time, mica is found unaltered and uncoloured when associated with large amounts of other elements, a fact which seems to show that such elements do not emit ionising rays even over geological epochs of time.

Few would have ventured to prophesy ten years ago that the new science would put an arresting finger on the estimates of the longevity of the sun and the age of the earth—subjects of exceptional interest both to the biologist and the geologist. Yet, in the earth's crust, the amount of radioactive matter and, with it, the quantity of heat spontaneously emitted far exceed that which is necessary to reconcile the older physical estimate of the earth's age—based on the earth's internal heat—with the much longer period which geologists claim as necessary.

Radium and the Physician

If unshielded, radium carried in close proximity to the body for any considerable time produces an ulcer that is extremely difficult to cure. It would, on the contrary, be anticipated that an agency possessing such vital characteristics would, like X rays, find application in the treatment of disease. And, as is well known, this has proved to be the case. It has resolved itself now chiefly into a question of improving the technique and finding the appropriate "dose." It is in this field of work that, as I have stated above, Germany has so greatly exerted herself. It is a field which is now receiving more attention in

this country. Public interest is, in fact, becoming more and more focussed upon the curative values of radium, especially in regard to cancer. At present it is as well to be cautious and say that radium will probably form a valuable ally in fighting cancer rather than to speak of it as a cure. It is early days to claim anything more than that the outlook is hopeful. There are, however, a number of other complaints, such as arthritis, rodent ulcers, neuralgia, diabetes, gout, sciatica, etc., in some of which treatment by radium and radium waters has worked wonders. The beneficial qualities of certain spa waters, by the way, are now attributed partly to the radium emanation they contain.

The public should, however, be cautioned against uncertified radium treatment. The warning is very necessary at the present time ; and the establishment of Radium Institutes in all the large towns would give both the public and the medical profession the assistance and safeguards to which they are entitled. This is a scheme to which the Commissioners of the National Health Insurance might well give close attention.

ESSAY-REVIEWS

A GREAT COSMOGONIST, by H. SPENCER JONES, M.A., B.Sc.: on **Scientific Papers**, by Sir GEORGE HOWARD DARWIN, K.C.B., F.R.S. Vol. V, Supplementary Volume containing Biographical Memoirs, by Sir FRANCIS DARWIN and Prof. E. W. BROWN, Lectures on Hill's Lunar Theory, etc. Edited by F. J. M. STRATTON, M.A., and J. JACKSON, M.A., B.Sc. [Pp. lvi + 81.] (Cambridge: at the University Press, 1916. Price 6*s.* net.)

SIR GEORGE HOWARD DARWIN was a striking example of a man whose mathematical power developed slowly and late. Although possessed of considerable innate mathematical ability, he was long in realising this fact, and when he obtained the high position of second wrangler in the Mathematical Tripos at Cambridge he, himself, was probably more surprised than were his friends. Lord Moulton, who was Senior Wrangler, in the same examination, says that Darwin was expected to take a high place, but did not at that time display any of that colossal power of work and of taking infinite trouble which characterised him afterwards. So little did he expect such success that he stayed in bed on the morning the results were read. The result produced the following characteristic letter from his father:

"DOWN, *January* 24 [1868].

"MY DEAR OLD FELLOW,

"I am so pleased. I congratulate you with all my heart and soul. I always said from your early days that such energy, perseverance, and talent as yours would be sure to succeed; but I never expected such brilliant success as this. Again and again I congratulate you. But you have made my hand tremble so I can hardly write. The telegram came here at eleven. We have written W. and the boys.

"God bless you, my dear old fellow—may your life so continue.

"Your affectionate father,

"CH. DARWIN."

After his tripos, Darwin's attentions were for a time diverted from mathematics to study for the Bar, but after a few years

this was given up and he returned to Cambridge. On the other hand, peculiarly enough, Lord Moulton soon gave up mathematics for the Bar. After a few scattered papers on various subjects, Darwin seems at once to have started on the line of research along which all of his later work was directed. His earliest paper in this connection, "On the influence of geological changes in the Earth's axis of rotation," attracted the attention of Lord Kelvin (then Sir William Thomson) and was published by the Royal Society. In this way started an association of great value to Darwin in his subsequent life. This success was commended by his father in the following terms:

"My dear Old George,

"I have been quite delighted with your letter and read it all with eagerness. You were very good to write it. All of us were delighted, for considering what a man Sir William Thomson is, it is most grand that you should have staggered him so quickly, and that he should speak of your 'discovery, etc.' . . . Hurrah for the bowels of the earth and their viscosity, and for the moon and for the Heavenly bodies and for my son George (F.R.S. very soon). . . ."

Darwin was at this time thirty-two years old, and almost all of his later work lay in the development of his ideas in various directions. Prof. Brown remarks on the homogeneous nature of all his work, the aim which he had in view having been apparently to apply the tests of mathematics to theories of cosmogony. If existing mathematical methods did not suffice for his purpose he would proceed to develop new ones, as in his valuable paper on "Ellipsoid Harmonic Analysis," or failing this he would obtain arithmetical solutions by the methods of quadratures. It was largely to this singleness of aim that his success was due, for his health was always indifferent and precluded long hours of work. The second characteristic of Darwin's work mentioned by Prof. Brown is that behind all of it was the severely practical view of testing physical hypotheses and reducing his results to figures. He was an applied mathematician in the truer and older sense of the word. Of late years a newer school of applied mathematicians has arisen which is chiefly concerned with discussions of the special functions and equations which occur so frequently in all branches of mathematical physics, and with the conditions

under which various solutions are valid. Some brilliant generalisations and important results have been obtained by this school, but these are derived rather as by-products of the original investigation. The mathematician of the older school starts with a definite problem, more or less idealised so as to be susceptible of mathematical manipulation, and proceeds to attack it by such methods as seem most promising. His intuition guides him safely through difficulties which would bring the pure mathematician to a halt. Both classes of work are valuable and necessary, but it would be difficult to prove that the latter, though the less showy, has not been the more productive of results of importance.

Darwin frequently found it necessary to resort to numerical computation when further mathematical treatment became impossible, and always preferred a quantitative to a qualitative result. Although he disliked numerical calculation he would never shrink from the most arduous computations if only there seemed some possibility of throwing further light on his results. He once told Lord Moulton that he detested arithmetic and that his calculations were as tedious and painful to him as they would have been to any other man, but that he realised that they must be done and that it was impossible to train any one else to do them. In tracing one of his periodic orbits he casually remarks that he obtained for the purpose seventy-five points, each of which involved three-quarters of an hour's work. His results, which have been invaluable in theories of cosmogony, fully justified the labour expended in obtaining them and show such work to be equally necessary with that of the newer school. Let due honour be given to the men who do the necessary spade-work of science. Darwin himself aptly described his methods as "the procedure of a house-breaker who blows in a safe with dynamite instead of picking the lock"; and again, "to put at their lowest the claims of this clumsy method, which may almost excite the derision of the pure mathematician, it has served to throw light on the celebrated generalisations of Hill and Poincaré."

The present volume forms the fifth and concluding volume of Darwin's collected papers, the previous four volumes having been published during the author's lifetime and containing such of his papers as were published before 1910 as he wished to see reproduced. Many scientific reports on various subjects

have, by his own decision, not been included. Opportunity has been taken to include two biographical memoirs, the one by his brother Sir Francis Darwin, giving an account of his life and activities; the other by his friend and pupil, Prof. E. W. Brown, of Yale University, giving a valuable account of his scientific work. These memoirs are of considerable interest, more especially to the many who knew Darwin at Cambridge as a friend and a teacher.

It is impossible in this brief article to give even a résumé of Darwin's work. The long series of papers on viscous spheroids, on the effects of tidal friction, and on rotating masses of fluids were directed to the important aim of developing a consistent theory concerning the past history of planetary and satellite systems. Darwin's theory was, briefly put, that the Earth and the Moon had originally formed one body and that the origin of the Moon was to be sought in the rupture of the parent body into two parts. Darwin came to this conclusion by working backward in time from a consideration of the present state of the Earth and Moon. This theory, whilst Darwin was at work upon it, received remarkable confirmation from the brilliant work of Poincaré, who traced the process of evolution forwards by considering the process of change in a rotating liquid planet as it gradually condensed through cooling. He found that stability was ultimately transferred to a pear-shaped figure resembling that to which Darwin had been led in his researches, as the form of the parent body before the rupture had taken place. To place the whole theory on a firm basis it remained to prove that Poincaré's pear-shaped figures would remain stable through the changes necessary to fill the gap between these and Darwin's figures. Darwin was never able quite to complete this. He discussed the equilibrium of Poincaré's pear-shaped figure and thought he had established its stability; a concurrent investigation by Liapounoff pointed to the opposite conclusion. It has been left to Mr. J. H. Jeans, a pupil of Darwin, recently to settle the dispute by proving that both Darwin and Liapounoff were wrong, as their approximations were only carried to the second order, and this, he finds, leaves the question of stability indeterminate; a third order approximation, however, shows that the figure is unstable. The process of evolution then, traced forwards as by Poincaré, leads to the pear-shaped figure; there

instability ensues, a cataclysm takes place, the satellite is shot off violently from the parent body, and a new position of stability results with the two bodies in rotation at a certain distance apart, as discussed by Darwin. Thus a consistent theory has at last been obtained, in the formulation of which Darwin's work has been invaluable. Darwin himself regarded his theory as important inasmuch as it was applicable as a whole to celestial evolution. Proven it, of course, cannot be regarded as being, but the evidence in its favour is considerable. Independent support is now being obtained from the study of spectroscopic binaries, the light curves of many of which can be explained on the supposition that there are two stars rotating around one another and which are under strong tidal distortion.

The following words from Prof. E. W. Brown's memoir aptly sum up Darwin's service to cosmogony and may fitly close these brief remarks on his work :

"Perhaps Darwin's greatest service to cosmogony was the successful effort which he made to put hypotheses to the test of actual calculation. Even though the mathematical difficulties of the subject compel the placing of many limitations which can scarcely exist in nature, yet the solution of even these limited problems places the spectator on a height which he cannot hope to obtain by doubtful processes of general reasoning. If the time devoted to the framing and setting forth of cosmogonic hypotheses by various writers had been devoted to the accurate solution of some few problems, the newspapers and popular scientific magazines might have been less interesting to their readers, but we should have had more certain knowledge of our universe. Darwin himself engaged but little in speculations which were not based on observations or precise conclusions from definitely stated assumptions, and then only as suggestions for further problems to be undertaken by himself or others."

This paragraph reminds one of the note in the last issue of Science Progress entitled "Great Science and Little Science." Darwin's work did not consist of isolated observations, but is one of the monuments of Science to the making of which his life's work was directed.

The lectures on Hill's Lunar Theory, which were delivered by Darwin during his tenure at Cambridge of the Plumian pro-

fessorship, and which he had before his death desired to see published, have been included in this volume. Hill's work greatly interested Darwin, and in this course of lectures he treats it very clearly and makes the essential features easy to grasp. As an introduction to a study of Hill's work or as a means of obtaining a general knowledge of it without having to go through the full details of the original, it may confidently be recommended. Of Darwin as a teacher Prof. Brown speaks in very appreciative terms:

> "To the pupils who owed their first inspiration to him he was a constant friend. First meeting them at his courses on some geographical or astronomical subject he soon dropped the formality of the lecture room and they found themselves before long going to see him continually in the study at Newnham Grange. . . . To have spent an hour or two with him, whether in discussion on 'shop' or in general conversation, was always a lasting inspiration. And the personal attachment of his friends was strong; the gap caused by his death was felt to be far more than a loss to scientific progress. Not only the solid achievements contained in his published papers, but the spirit of his work and the example of his life will live as an enduring memorial of him."

That power of stimulating thought, which Darwin had so well, is the true function of a teacher and one which unfortunately too many University professors lack. It may well be that it will ultimately be recognised that our greatest debt to Darwin is to be found not in his own researches, important though these are, but in the researches of some of his pupils who owed their initial stimulus to him.

Royal Observatory, Greenwich.

The Origin of Igneous Rocks, by Arthur Holmes, D.I.C., A.R.C.S., B.Sc., F.G.S.: on **The Later Stages of the Evolution of the Igneous Rocks**, by N. L. Bowen. Supplement to the *Journal of Geology*, vol. xxiii. pp. 1–91, 1915.

During recent years a great deal of attention has been given by petrologists to the difficult but fascinating problem of the origin of igneous rocks. The relative claims of differentiation alone, and of differentiation combined with assimilation, have been ably expressed by Harker, Iddings, and Daly. These

authors, however, were at a considerable disadvantage in so far as they were obliged to formulate their views in the absence of adequate experimental data. Fortunately for the future stability of petrological theory, data of the kind most urgently needed have been accumulating in the geophysical laboratories of the Carnegie Institution of Washington. The physical chemistry of the cystallisation of various binary and ternary systems—notably those of the plagioclase felspars, of diopside-forsterite-silica, of anorthite-forsterite-silica, and of diopside-anorthite-albite—has been closely investigated, and the results now available are of the utmost importance. Indeed, they must become the basis of all future discussion. So brilliant is the light which these researches throw on the vexed question of differentiation, that the patient workers to whom they are due deserve not only the gratification that attends success, but also the gratitude of all students of igneous geology. Already they have cleared away some of the mists of speculation. May they be stimulated to penetrate still further the clouds that yet await dispersal !

Mr. N. L. Bowen, himself one of the foremost investigators in the Washington laboratories, presents in the paper before us a lucid statement of the experimental facts already ascertained ; and proposes, largely on their indubitable authority, a systematic petrogenic theory. As regards the relative importance of assimilation of foreign material, and differentiation of a molten magma, as rival or co-operative processes in the genesis of igneous rocks, he stands firmly in undivided enthusiasm for the latter. The unit of differentiation in a magma may, theoretically, be a molecule, a bleb of immiscible liquid, or a crystal. Bowen agrees with Harker that the Soret effect is hopelessly inefficient as a cause of concentration, but he then parts company with Harker and supports Becker in the view that diffusion is equally limited in its scope. Liquid immiscibility has been favoured by Daly, but Bowen clearly demonstrates the evidence against it (except possibly between silicates and sulphides). The most powerful factor in promoting differentiation he finds in crystallisation. " The two processes involving relative movement of crystals and liquid—the sinking of crystals and the squeezing out of residual liquid—aid each other in a general way in the production of an arrangement of the various differentiates, such that the heaviest lies

at the greatest depth, and the lighter ones at lesser depth—a gravitative adjustment."

Actual experiments with various melts have shown that the crystallisation of basaltic magma, if slow, provides a continuous variation from basalt to syenite. The early formation of olivine enriches the residual liquid in silica; similarly, the crystallisation of calcic plagioclase allows a concentration of albite, and that of enstatite an enrichment in diopside—provided, of course, that these minerals sink, and so are not resorbed at the level where they separated. If sufficient silica were liberated, the residual magma might pass to one of granodiorite or even granitic composition, and this is considered to be the explanation of the frequent intimate association of dolerite and granophyre. It is clear, however, that the early separation of olivine is inadequate to supply all the free silica required by a granite. The discrepancy is met by an appeal to biotite, a mineral which characterises by far the majority of granites, and which represents considerable proportions of the alkalies and the ferro-magnesian constituents in their least siliceous combinations. "This difference between the natural magma and the artificial melts, in which a diopsidic granite was the salic differentiate, is plainly the outcome of the fact that the artificial melts are anhydrous, whereas the magma contains various volatile constituents. The formation of hornblende and still more of the micas, with their essential content of water and often of fluorine, is the result of an increasing concentration of volatile constituents." The slow crystallisation of a basaltic magma leads to a concentration of orthoclase, albite, and water, and the effect of the latter is to bring about reactions such as:

$$KAlSi_3O_8 \rightleftarrows KAlSiO_4 + 2SiO_2$$
$$NaAlSi_3O_8 \rightleftarrows NaAlSiO_4 + 2SiO_2$$

The orthosilicates then combine with ferromagnesian silicates to form biotite, which is precipitated with free quartz. The more significant molecules that in the presence of water and other volatile fluxes become concentrated in the residual liquid are the alumino-alkaline orthosilicates, and the ferromagnesian orthosilicates (with gradual elimination of magnesium and consequent further enrichment in iron).

Bowen now goes a step further and suggests that when

mica and quartz are precipitated the residual magma contains a concentration of nepheline molecules with the volatile fluxes appropriate to the formation of cancrinite, hauyne, and sodalite. If such a liquid is squeezed out, or if the quartz and biotite sink away, a foyaite magma remains of the requisite composition demanded by nepheline-syenites and allied alkaline rocks. Intermediate stages, of course, supply alkali-syenites and granites. The following summary of the course of crystallisation followed by a basaltic magma when it cools slowly, and under such conditions that the crystals sink, or the residual liquids are drained away, indicates part of a possible, and, according to Bowen, probably the most normal line of descent. At each stage the temperature is lower, and the volatile fluxes are more concentrated, than in the preceding stage. As soon as complete crystallisation is effected the process of necessity stops, and according to the rate of cooling this may take place at any of the specified stages.

Principal crystalline products of a *slowly* cooling basalt magma.		Resulting rock types.
Felspars.	Other minerals.	
——	Olivine, spinellids.	Peridotite.[1]
Calcic plagioclase.	Hypersthene.	Hypersthene basalt or norite.
,, ,,	Diopsidic pyroxene.	Basalt or gabbro.
Medium plagioclase.	Hornblende.	Diorite.
Medium plagioclase, orthoclase.	Biotite, quartz.	Granodiorite.
Soda plagioclase, orthoclase.	Biotite, muscovite, quartz.	Granite.
Soda plagioclase orthoclase, microcline.	Soda amphiboles and pyroxenes, quartz.	Alkali-granite.
,, ,, ,,	Soda amphiboles and pyroxenes.	Alkali-syenite.
,, ,, ,,	Soda amphiboles and pyroxenes, nepheline.	Nepheline-syenite.
,, ,, ,,	Soda pyroxenes, corundum, nepheline, sodalite, etc.	Corundum-bearing nepheline-syenite.
,, ,, ,,	Soda pyroxenes, analcime.	Analcime rocks.

Bowen suggests alternative lines of descent due to decreased activity of the volatile fluxes in early stages, brought about possibly by actual deficiency of these constituents, or by relief

[1] By the separation and individual concentration of pyroxenes, or of plagioclase, pyroxenites or anorthosites may be formed respectively. By more rapid cooling, olivine, pyroxene, and plagioclase may crystallise in close association, giving an olivine basalt or gabbro.

of pressure in areas undergoing tensional faulting. One of these alternatives is the series gabbro-diorite-syenite, etc.; another is gabbro-essexite-augite-syenite, etc. In them orthoclase is able to crystallise at a moderately early period, owing to the normal delay controlled by water and other volatile fluxes being somewhat weakened.[1] In this way practically the whole field of igneous rocks is brought under survey, and their evolution is traced directly back to a primary basalt magma. While Bowen does not deny the fact of assimilation, he assigns to it only a minor rôle. Magmatic stoping and abyssal assimilation on the scale advocated by Daly are considered now to be unnecessary, since granite—the chief rock type to be explained—may be derived from basalt magma, given a sufficiently large intrusion and a sufficiently long period of cooling. The basic borders of many batholiths represent on this hypothesis a less advanced stage in the differentiation of the magma, that stage having been arrested by the completion of crystallisation around the cooler margins, while later stages—granodiorite or granite—were reached in the hotter and more slowly cooling interior.

It should be carefully noted that experimental investigation has not yet passed beyond a demonstration of the possible evolution of dioritic and syenitic magmas from basaltic magma. Two difficulties occur to the present writer that deserve discussion before complete acceptance of the further step—the evolution of all granitic rocks—becomes possible. Some basalt magmas could undoubtedly give rise to about 10 per cent. of granite, provided that they had previously differentiated in the opposite direction so as to produce a similar amount of peridotite. The gabbro, however, which would consolidate at intermediate depths, would afterwards be "dead" from the point of view of granite generation. The composition of gabbro as compared with basalt clearly shows this to be the case. The refusion of gabbro might lead to stoping and assimilation, but no appreciable supply of fresh granite could be extracted. The amount of gabbro, or potential basalt magma, in the earth is limited, for the distribution of the radioactive

[1] It is worthy of notice that Dr. J. W. Evans, in a communication to the Twelfth International Geological Congress, Toronto, expressed his opinion that in the presence of sufficient water basalt magma was enabled to produce a differentiate of granitic composition; whereas, if water were deficient, the basic portion would retain more alkalies, and on further differentiation would yield the different rocks of the alkali-series.

elements shows that if gabbro, as we know it, existed through and below a depth of 30 kilometres, the total radioactivity would be so great that the earth should be growing hotter. We are therefore restricted to a shell of granite not more than three kilometres in thickness, on an average. If, after the early Pre-Cambrian intrusions, there was less granite than this amount in existence, then some of the basaltic magma below might still be able to produce more. Otherwise refusion by stoping would be the only mechanism whereby future batholiths could be formed.

These alternatives may be tested by an appeal to the field associations of alkali rocks. Phonolites and basalts are commonly found together, but according to Bowen's scheme rhyolites ought to fall between them in the course of genesis. Yet they are rarely found in alkaline volcanic fields, unless they themselves are strongly alkaline. Why is it that the volcanic islands of the Central Pacific within the andesite border persistently fail to provide rhyolitic lavas? The reason is probably that the basalts are incapable of supplying the necessary silica. If 90 per cent. of the composition of average gabbro be subtracted from 100 per cent. of that of average basalt, the 10 per cent. difference gives a great excess of alumina, alkalies, and iron oxides over that required by granite, but a marked deficiency in silica. It would seem, then, that the salic differentiates of basalt ought to be markedly alkaline. The granite or rhyolite stage should, in general, be passed over. This is fully in accordance with the facts, and suggests an additional reason why alkali rocks seem to have become more abundant in the later periods of geological history. During early periods, basalt may have had potentialities for the differentiation of granite. Later, having lost those potentialities, it has attained a greater chance of generating alkali rocks than formerly. It is very likely that Bowen has provided the *raison d'être* of the earth's original granitic shell, but not necessarily that of later granites.

The second difficulty supplements the first. While not accepting magmatic stoping as a chief factor in the mechanism of batholithic intrusion, Bowen fails to suggest an alternative process. Where are the rocks that the batholiths have displaced? The oldest known sediments, even in their metamorphosed condition, imply a granitic origin. The oldest

known granites, which are often of gigantic size, are always intrusive into such sediments. Clearly those granites are the refused equivalents of pre-existing granite. The process of stoping, fusion, and assimilation would advance the progress of differentiation in the direction of more and more siliceous granite, and such a progressive change, at least in the regions personally known to the writer, has undoubtedly taken place. On the other hand, if basalt magma has always been the source of the later granites, these ought to be less siliceous than the earlier ones, on account of the growing exhaustion of the parent basalt magmas.

The theory emphasised by Daly, and supported by Bowen, that basalt magma is the parent magma of all igneous rocks is probably true for most rocks. The source of heat and volatile fluxes is, however, now to be sought at far greater depths than those at which basalt magma can exist. Radioactivity, isostasy, and tidal phenomena all unite in suggesting that fusion must first take place at a depth of several hundred kilometres. If, as is probable, the rocks at such a depth are peridotitic in composition, then it would appear that only a long process of differentiation, stoping, and assimilation could be adequate to provide first basalt magmas, and afterwards granite magmas. Peridotite itself is so heavy that only rarely would undifferentiated intrusions of that composition succeed in reaching the upper levels of the earth's crust.

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STRUCTURE OF COAL, by Marie C. Stopes, D.Sc., Ph.D., Fellow and Lecturer in Palæobotany, University College, London: on

(1) **The Origin of Coal**, by David White and Reinhardt Thiessen, with a chapter on the Formation of Peat by C. A. Davis. Bull. 38 Bureau of Mines, U.S.A. [8vo. Pp. 304, pls. liv. Washington. 1913.]

(2) **The Coals of South Wales, with Special Reference to the Origin and Distribution of Anthracite**, by Aubrey Strahan, M.A., Sc.D., LL.D., F.R.S., and W. Pollard, M.A., D.Sc., F.I.C., assisted by E. G. Radley. 2nd ed. Memoir Geol. Surv. England and Wales, 1915. [8vo. Pp. 91, pls. x.]

(3) **Histoire Naturelle d'un Charbon** (Russian and French), by M. D. Zalessky. Mém. Comité Géol. nouvelle sér. livr. 139. Petrograd. [4to. Pp. 74, pls. xii.]

These three papers, the work of leading men in the Geological Surveys of the United States, this country, and Russia respec-

tively, afford an interesting constellation throwing light on that most important but little known theme, the structure of coal.

The first of these papers, that by White and Thiessen, appears to have suffered shipwreck on its way to this country, for several of the chief geological libraries have not received their copies. This is particularly unfortunate, for there is no doubt that it is the most important and the most interesting paper on coal which has appeared for some time. It is remarkable in that it attempts to treat the subject of the origin of coal, not from one side only, but from every point of view. The main headings of the contents table indicate this—there the sections of the work are grouped under the following sub-titles : " Geological Relations of the Coals " ; "Analyses of the Coal Samples Studied " ; " Physiographic Conditions attending the Formation of Coal " ; " Regional Metamorphism of Coal " ; " Origin and Formation of Peat " ; " Microscopic Study of Coal." The *contents table alone* occupies three and a half pages under the last heading—more than is allotted to the whole subject by most writers on coal. In the preparation of this monograph the work of section cutting and preparatory technique occupied several years, and the Bureau of Mines appointed Dr. Thiessen specially for the microscopic examination of the coals. The results, though far from exhausting the subject, indicate what might be done with adequate facilities. One result of Dr. Thiessen's work, a result of permanent value, is the final explosion of the myth of " algal coals," which were supposed to be formed by a special organism " *Pila*." Different species of this supposed peculiar organism have been described from boghead coals, oil shales, etc., and their algal origin was widely believed in. This myth got a good start by being backed by the two distinguished French palæobotanists, Bertrand and Renault. It went the round of the profession, and is still accepted by most geologists as orthodox. A few years ago Prof. Jeffrey of Harvard brought forward strong evidence against it, and though at that time David White accepted the view, the work he inaugurated with Thiessen converted him to the opinion which will certainly in future be the orthodox one—*viz.* that these so-called algæ, with their peculiar coal-forming habits, were not algæ at all, but were partly decomposed Lycopodinean macrospores.

The Americans are particularly fortunate in having in their country not only extensive coal-basins of Carboniferous age, but also coals of Mesozoic and Tertiary ages; so that their outlook on the whole subject of coal formation should be much better balanced than that of those whose data are restricted. The value of this wider experience should be particularly evident in connection with that interesting puzzle—the cause of the anthracitisation of coals. The old-fashioned idea that anthracites are essentially palæozoic is, of course, immediately demolished by a study of the Cretaceous and other Anthracites in America and Asia; and so one very much wishes that the sections of White and Thiessen's paper touching on the subject were much fuller and more detailed than they are. One theme which is brought out clearly by these authors, is, to quote the words of their short conclusion, "The degree of decomposition, elimination, maceration, chemical reduction, and constitution, in any one stage [of coal-formation] depends on the species, kinds of parts, organs and products contributing to the deposit originally; the efficiency and duration of action, chiefly of the biochemical agencies, during the peat stages; and the efficiency and duration of action of the dynamochemical agencies during the coal stages."

The plates show many interesting structures in coal sections; but suggestive and exhaustive in some respects though the work be, it leaves one continually asking for more data, more details, more illustration. The fact that over three hundred pages and fifty plates leave one feeling that the whole subject is merely indicated rather than presented completely, shows the extent of the problems involved.

The paper on the South Wales coalfield is a great contrast to the American work. Those themes on which it enters it deals with more exhaustively, and there are many pages of details and analyses which will be invaluable to specialists. The interpretation of the true meaning of the chemical analyses of coals is a very difficult subject, which is well handled with many useful figures, in this paper. The detailed charts of the distribution of the anthracitic zones, prepared with such care, bring out clearly the relative positions of the anthracitic and bituminous portions of the seams, and must be of service not only to science, but to practical miners. These charts illustrate strikingly the fact that the isoanthracitic lines congregate

round a definite centre, with minor variations, and that they do not depend on the main tectonic movements which have determined the present lie of the beds. It thus comes about that in some places in that part of the coalfield where the measures are thickest the seams are bituminous and not anthracitic, as they should be were the anthracitisation simply a tectonic result, changing to anthracite coals which had been originally bituminous. Dr. Strahan finds that "serious objections present themselves" to the three explanations by previous workers of the change in the character of the same seam in different parts of the Welsh coalfield. But then suddenly, in the most disappointing way, the work comes to an end! The paper closes just when, to my palæontological mind, it had illuminated one side of the problem and prepared the ground for a detailed microscopic examination of the coals. The abrupt conclusion "that the differences between the anthracitic and bituminous coals in South Wales are mainly due to original differences in composition" is discussed for a few pages, but no evidence whatever of a positive, palæontological nature is brought forward in its support. Until much detailed microscopic work has been done on the subject it is impossible to speak dogmatically, but I may remark that the general distribution of the Welsh anthracites, coupled with what is known of the American and other anthracites, makes it very difficult for a palæobotanist to accept the conclusions of this paper in the simple way they stand. The interpretation of the meaning of ash analyses in the various kinds of coal can conceivably lead in a very different direction from that taken in the present paper, and is an entrancing theme, too extensive to be dealt with here.

The Russian paper differs from the two preceding monographs in being less a consideration of coals in general than a description of a single kind of coal, and this of a type new to science. The description is essentially palæontological and is accompanied by some superb micro-photographs of the new fungi and other micro-organisms found in between the layers of the larger alga which forms the coal. This alga is described as so closely resembling the recent *Himanthalia* that the fossil, coal-forming species, is named *Himanthaliopsis Sniatkovi* sp. nov. This new coal is unfortunately represented only by hand specimens, and has not been found *in situ*, so that many

most interesting questions it raises cannot as yet be answered. While White and Thiessen have effectually demolished the old "algal theory" of coal, which postulated the existence of peculiar, microscopic, jelly-like algæ, this new coal raises a new algal theory. Have we here a quite other type of "algal" coal, a coal formed from fucoid sea-weeds? So far as the specimen goes it certainly looks like it. But regarding it I will risk a prediction: when, if ever, this coal is found *in situ* it will not be an extensive coal, but will be very restricted in quantity. Extremely interesting observations are made by Prof. Zalessky on the ash percentage of this coal in relation to the ash percentage of the plants from which it was formed; the coal containing only about one-fourth of the ash present in the living plants closely allied to the species which composed it. This is not the place to elaborate ideas on the subject, but Zalessky's paper should be read in connection with the above work on anthracites. The conjunction will be found to be very suggestive.

A GREAT MEDICAL REFORMER, by ***: on **John Shaw Billings: a Memoir,** by FIELDING H. GARRISON, M.D. [Pp. x + 432, with Illustrations.] (New York and London: G. P. Putnam's Sons, The Knickerbocker Press, 1915. Price 10*s.* 6*d.* net.)

THIS is the record of the life and work of a man who was unique in the history of his profession. Except in his official capacity as an army surgeon in the War of the Rebellion, John Shaw Billings never practised his profession in the common acceptation of the term. Yet he did more to reform it, to remake it, than any other individual worker either in his own land or on this side of the Atlantic.

Details of his early life are scanty. He was born in 1838 in Cotton Township, Indiana; he was educated in Miami University, where he took his Bachelor's degree in 1857; and in 1895 he returned to his *alma mater* to deliver a suggestive address on "Waste" to the students of a new generation. In the autumn of 1858 he matriculated at the Medical College of Ohio with which the Miami Medical College had just been combined, and in 1860 he took his medical degree. His graduation thesis was entitled "The Surgical Treatment of Epilepsy," and even in this, his first essay in medical literature,

he gave a foretaste of the humour and common sense always manifested in his later writings.

Billings was Demonstrator of Anatomy at the Ohio Medical College, was beginning to get into touch with surgical practice and on the eve of becoming assistant to Prof. Blackman, when the Civil War broke out. He was appointed first lieutenant and assistant surgeon on April 16, 1862, and this appointment decided the course of his whole after career.

His first charge was the Cliffburne Hospital, which he found "in an extremely filthy and dilapidated condition." At once he instituted the necessary sanitary reforms. This was the beginning of what was to be one of the principal departments of his life's work. During the two years of office as army surgeon, Billings kept a detailed diary of daily work and incidents. No newspaper correspondent could have given a brighter or more vivid account of the campaign, as it entered into matters of an interest wider than the mere medical and surgical aspect. This diary is largely supplemented by letters of a similarly descriptive nature, written to his wife, to whom he was married in 1862.

In the summer of 1864 Billings was relieved of active duty in the field and assigned to the Washington branch of the office of the Medical Director of the Army of the Potomac, and charged with the duty of analysing and arranging its field reports, afterwards embodied in the *Medical and Surgical History of the War*. Six months later he was transferred to the Surgeon-General's office in the War Department. As an army surgeon he had become an expert in operative surgery, but all active exercise of this branch of his profession was now at an end, and he settled down to the dull routine of official life which largely consisted in financial duties connected with the disbanding of medical officers and the discontinuance of military hospitals. His leisure time was devoted to the study of microscopy, in which he became an expert. He devoted special attention to the fungi and their supposed connection with the origin of infectious diseases, especially malaria. His valuable collection of fungi he presented at a much later date (1902) to the New York Botanical Garden.

About the year 1870, the sphere of his activities was widened in other directions. He was placed in charge of the

library of the Surgeon-General's office, then a mere handful of books compared with the vast collection of the present day; but enough to start him on his gigantic development of medical bibliography. Simultaneously he was called upon to inspect and report on marine hospital service throughout the country. This necessitated personal visits of inspection to all the ports of the country. He found the service in a disorganised condition, but he re-established it upon a military basis till it gradually reached its present high stage of efficiency.

Billings made two important contributions to military medicine between 1870 and 1875. The first was the report to the Surgeon on Barracks and Hospitals, and the second on the Hygiene of the United States Army. The report on Barracks and Hospitals was the joint work of several army officers, edited by himself, containing a sweeping criticism from his own pen of the hospital management and construction then in existence, quoting Tenon's report made to the French Academy of Sciences in 1788 as a model for future scientific hospital construction, and making his own suggestions for reform in this direction. His recommendations were to bear fruit in subsequent years in a most remarkable fashion in his connection with the Johns Hopkins Hospital. The report on Hygiene in the United States Army strongly insisted upon the care of the soldier as a unit, by the encouragement of personal cleanliness and the ventilation of his surroundings.

In 1897 Billings was appointed Vice-Chairman of the National Board of Health, a body which, though comparatively short-lived, did good work, much of which was at his instigation and under his guidance.

The Centenary of American Independence let loose a flood of self-congratulatory reminiscences on the part of Americans generally. Billings's centennial survey of the literature of his profession in *A Century of American Medicine, 1776–1876*, stood out in sharp contrast to the smooth sayings of others. The writings of the colonial period he entirely discounts, and he is severely critical of the whole output to date. This established his reputation as a medical historian in the eyes of the more enlightened members of his profession abroad, though not, it is to be feared, among his confrères in the United States.

Memphis, one of the centres of cotton distribution in the

Southern States, was visited with a severe epidemic of yellow fever in 1879. Acting under the authority of the National Board of Health, Billings made a thorough sanitary survey of the city and made his report in the winter of that year. Although the agent of transmission of yellow fever was then unknown—later to be revealed by a future brilliant pupil of his, Walter Reed—the recommendations made in this report as to means of prevention were all on the right lines.

It will be news perhaps to some that the beginnings of the Johns Hopkins Hospital date back more than forty years. Its founder died in the winter of 1873, bequeathing the remainder of his estate to the foundation of the hospital, and University of Baltimore—the hospital to be called after his own name. In 1875 a circular letter was sent to five experts in hospital construction, Billings among the number, inviting them to send in plans. Billings's plan was preferred to those of his competitors, and almost immediately the building of the hospital was begun. Its well-nigh unique career as a teaching centre and its success in the treatment of disease need not be enlarged upon here, but it is common knowledge that its remarkable success in these and other directions was due principally, if not wholly, to his initiative. His abrupt interview with Dr. (now Prof. Sir William) Osler, whose lifework until he came to England was bound up with the Johns Hopkins, was characteristic of the manner of the man, but showed his wonderful instinct for making the right selection. Billings's *Description of the Johns Hopkins Hospital* published in 1890 is a model of its kind, and became an accepted textbook on the subject of hospital construction and ventilation. He always considered that the most difficult thing in forming the hospital consisted not in the planning of the buildings nor in their heating and ventilation, but in finding the "proper and suitable person to be the soul and motive power of the institution."

The year 1887 saw the opening of the Army Medical Museum, the administrative and sanitary arrangements of which had been planned by Billings himself. Its inception had dated back to the early 'sixties, at the time of the war, but its collections had been steadily accumulating for twenty years. Dr. Garrison describes it as "an interesting general collection," numbering among its treasures five specimens of the work of

His of Leipzig and Cunningham of Dublin. In 1888 Billings delivered a striking historical address on "Medical Museums" in which, speaking of his own collection at Washington, he said that the "medical museum hinted at matters which lay outside the scope of known physical and chemical laws."

Events and doings crowd the canvas of the picture of Billings's later life so closely that it is scarcely possible to cast more than a fleeting glance at them. He visited this country many times, and he formed many warm attachments and close friendships, but the two most famous visits were the occasions when in 1881 he delivered the address on "Medical Literature" before the London meeting of the International Medical Congress, and in 1886 the address in Medicine before the British Medical Association on "Medicine in the United States." The temporary celebrity which the subject and manner of delivery of this frank exposition of the contemporary state of the profession in the States secured him on this side of the Atlantic was somewhat clouded by the widening of the rift already existing between himself and prominent members of the American Medical Association. It is needless to say that the publication of this address meant to Billings the severance of lifelong friendships, an event suffered by him with his usual equanimity.

His professorship of hygiene at the University of Pennsylvania, his gift to medical literature of the *Index Catalogue of the Surgeon-General's Library* with which he was actively connected up to 1895, his directorship of the New York Public Library, his writings and lectures on medical and vital statistics, his connection in later years with the Carnegie Institute which secured the continuation of the *Index Medicus*, are events for a knowledge of which readers must refer to the memoir itself. His undertakings would have taxed the mind and strength of a man whose health never failed him, but from middle life Billings suffered from a malady which necessitated not only medical but repeated surgical relief.

The collection of the material for this memoir must have been a work of great labour, but the effect of its careful perusal has been to imprint on the memory a due sequence of events in a wonderful career. There are three portraits of Billings in early, middle, and later life respectively, a detailed bibliography of his many writings, and a genealogy of his

family tracing back the local connections of his English ancestors.

We cannot do better than conclude this notice with a quotation from Dr. Garrison's valuable contribution to medical history:

"Some of the finest tributes to Billings after his death came from England. One of the best is from Mr. J. Y. W. MacAlister, Secretary and Librarian of the Royal Society of Medicine:

"In Billings has passed away the kind of man who makes epochs. He was a *great* man in every sense of the word. Big in body, big in mind, and almost superman in his power of work, he impressed all with whom he came in contact with the conviction that, whatever walk in life he chose, he would be easily first. He undertook tasks and carried them through, which ordinary men attempt only by means of committees, institutions, societies, co-operations, and a vast amount of fuss and noise. His plan was simplicity itself. If the thing was worth doing, he simply did it. I saw him once 'resting' in the evening after a long and arduous official day. He was lying on a couch, almost hidden by two mountains of medical periodicals in every language, one on either side of him. He was slowly, but without pause, steadily working through the mountain on his right, marking the items to be indexed, and transferring each journal, as finished, to the mountain on his left. This was when he was, almost singlehanded, producing month by month the *Index Medicus* and the still greater task of the *Surgeon-General's Catalogue*—two pieces of work without which the rapid advance of medicine in the last thirty years would have been impossible.

"I remember his saying to me once when I said something in praise of what he was doing, 'I'll let you into the secret—there's nothing really difficult if you only *begin*—some people contemplate a task until it looms so big, it seems impossible, but I *just begin* and it gets done somehow. There would be no coral islands if the first bug sat down and began to wonder how the job was to be done.'

"He had done a big life's work when he was called to plan and administer the great New York Public Library, and he tackled it on his own principle—without fuss or unnecessary publicity; he just 'began,' and each day's herculean 'chore' saw him miles on his way to triumphant success.

"He was quite simply and sincerely modest, although this did not prevent an amused but quite magnanimous contempt for mere talkers. As an illustration of his modesty and sim-

plicity (at the risk of appearing vain) I recall that when he was planning the New York Public Library, he sent me a copy of the plans with a detailed memorandum on the specification, with a request that I would 'help me' (!) with criticisms or advice. I am prouder that *he* asked me this, and prouder still that he thanked me for and adopted some humble suggestions than if I had been consulted by a Government.

"His interests were as broad as his mind. One happy day I went with him and his lifelong friend, Justin Winsor, to Stratford-on-Avon. With us went Sam Timmins, the creator of the Memorial, and while Timmins did the honours of the place in his own inimitable way, Billings showed us—introduced us—to the *man* Shakespeare. They might have been schoolmates, so vivid was the living imagination with which his slow, almost solemn periods discoursed of the living Shakespeare and his immortal creations.

"Take him for all in all, Billings was a *man*, and *we* are not likely to look upon his like again."[1]

THE HISTORY OF THE FIGHT AGAINST VENEREAL DISEASE, by E. B. TURNER, F.R.C.S. (Chairman of the Representative Body, British Medical Association): on the **Final Report of the Royal Commission on Venereal Diseases.** [Pp. 191.] (London: H.M. Stationery Office, 1916. Price 1*s.* 11*d.*)

THE final report of the Royal Commission on Venereal Diseases has been published, thus closing the first act of the tragedy of the fight against this plague, an act which has taken twenty years to play to its conclusion. It was in 1896 that the first steps were taken, and the first efforts made, to obtain that inquiry into the extent and prevalence of these diseases which was so absolutely essential if any effectual scheme for their prevention and treatment was to be elaborated and set forth. The incident which actually started the agitation for this Government inquiry was one which, unfortunately, was by no means of rare occurrence. A physician being consulted by the personal attendant of five small children, all under eight years of age, on examination found her suffering from syphilis in its most infectious form. Beyond warning her that she was dangerous to any person with whom she might come in contact, and giving directions for the treatment of her disease, he was unable to take any other steps which might save the innocent babes under her care from the infection of this foul disease.

[1] *Brit. Med. Journ.* London, 1913, i. 642.

On discussing this case with a lady much interested in rescue work, a visitor at the Lock Hospital, she suggested that it might be possible to induce the Government to institute an inquiry as a prelude to more energetic action against the widespread mischief caused by Venereal Diseases, provided ample evidence of the necessity for such a course could be submitted, evidence both from members of the medical profession and from men and women interested in social work, who knew and thoroughly appreciated the vital importance to the national health of some such action. As a result of this conversation she was given an introduction to Mr. (now Sir) Victor Horsley, and in conjunction with him, in 1897, started the work which finally led to the appointment of the late Royal Commission.

After a good deal of preliminary spade work, carried out under the advice of various persons interested in the matter, among whom the following ladies and gentlemen were prominent—Adeline, Duchess of Bedford, Lady St. Helier, the late Lady F. Brudenell Bruce, Mrs. H. Bonham Carter, Mrs. R. P. Wethered, Mr. (now Sir) Victor Horsley, Dr. Mott, the late Col. Long, M.P., the late Rev. A. Brinckman, Mr. (now Sir C.) Loch, Mr. Donaldson Rawlins, K.C., and Mr. Archibald Allen—a memorial, the object of which was to obtain the appointment of a Royal Commission to inquire into the Prevalence and Effect of Venereal Diseases in this country, was drawn up.

A meeting of representatives of Ladies' Rescue Associations, of Workhouse Girls' Aid Committees, and of single workers was held on January 19, 1898. At this meeting the following resolution, among others, was proposed and carried—"That this meeting, consisting of representative rescue workers, lady guardians, matrons of homes and hospitals, and others, believes the national danger mentioned in the Memorial to be real and grave, and is strongly of opinion that a Royal Commission should be appointed to inquire into the subject." This Memorial was signed by representatives of eight Rescue Associations, ninety-six women engaged in social work, seventy-two men, representative of all classes, and fifty-two members of the medical profession holding official positions, and was presented to the late Lord Salisbury, who was then Prime Minister, in 1899. He, however, took no action upon it, because he considered that public opinion was not sufficiently informed and enlightened on this important matter.

In coming to this decision he was without doubt influenced to a certain extent by the opinions of those who were opposed to any inquiry whatever being instituted. The reasons which were given for this opposition were: the belief (1) that this disease was diminishing, not increasing; (2) that the Commission would be composed of men in favour of the old Contagious Diseases Acts, and that the evidence brought forward would produce a scare, with the result of the reintroduction of these Acts applied to the whole country. They also held that as this disease has its root in sin, it must only be fought on moral and religious grounds, and that, sad as it was to see the innocent suffer, and possibly our race deteriorate, better so than to do anything which might tend to make vice safer and consequently easier. They said, therefore, a Royal Commission would do harm, and any legislation which might be the result of an inquiry would do greater harm.

In carrying on this work it became very evident that those workers who were in favour of an investigation were those who, by reason of the character of their work, were more closely brought into personal touch with the sufferers, while those who opposed it were, while engaged in philanthropic or public work, less closely in contact with the afflicted. Much energy was expended and great efforts made to convert these "conscientious objectors," and it was repeatedly pointed out, by letters, speeches, and personal interviews, that the only object aimed at was "Inquiry," and that most, if not all, of those wishing for a Royal Commission were as much opposed to the reintroduction of the Contagious Diseases Acts as any of those who preferred to remain in darkness, and that it was wise to combat the threatened degeneration of the race by practical measures, if found necessary and possible, as well as by moral precepts. The process of enlightenment, however, was slow and unsatisfactory, and though doubtless a certain number of conversions were made, still many remained "of the same opinion still," and were a stumbling-block and obstruction to the end.

The aspirations of those promoting the request for full inquiry were as follows: (1) Inclusion of men as well as women and children in the scheme, thus making it just, and giving hopes of its being efficacious; (2) There was no suggestion of registration or anything like the licensing of vice; (3) The aim

of the old Contagious Diseases Acts was to put women in such a state of health that they should be authorised to return to an evil life; (4) The aim of this scheme was to protect the innocent from contagion, and check the spread of this awful disease. The full inquiry asked for was to include a report whether some form of notification of these ailments, as in the case of other infectious diseases, was possible or not. This is the scheme alluded to above.

In September 1899 the Brussels International Medical Congress passed strong resolutions, proposed by the British Medical Association, calling for full inquiry into the causes and prevalence of these diseases.

It might be well here to recall the "Suggested Terms of Reference for Inquiry re Venereal Diseases" as drawn up by Col. Long in 1898: (*a*) To ascertain how great the evil is now, irrespective of its temporary increase or decrease; (*b*) To collect evidence as to the prevalence of the various forms of Venereal Disease among patients attending the hospitals and dispensaries throughout the country; (*c*) To collect information as to existing arrangements for the treatment of Venereal Disease, the distribution of hospitals and dispensaries, and the number of beds available in different places, and to make suggestions as to more efficient provision for the treatment of the disease; (*d*) To collect suggestions and express opinions as to any means that can be devised for preventing or limiting the spread of Venereal Disease among the population of this country; (*e*) To consider the possibility of so altering the law of libel that while medical men should still be liable if they improperly divulged private information obtained in the course of their practice, they should yet be in a position to take action which would render interference possible by some sanitary authority, with a view to ensuring proper precautions being taken against danger to the community, and especially to innocent persons.

The next forward step in this movement took place in 1903, when a meeting was held in May at which the Bishop of Stepney (now Archbishop of York) was in the chair. Resolutions were carried reaffirming the necessity for full inquiry, and urging that public bodies connected with the health of the people should take action. To further the object of these resolutions a very representative committee of men, under the chairmanship of the late Col. Long, M.P., was set up. In that year also

an Inter-Departmental Committee, appointed to inquire into the physical deterioration of certain classes of the population, recommended " the appointment of a Commission of Inquiry into the prevalence and effects of syphilis, having special regard to the possibility of making the disease notifiable, and to the adequacy of hospital accommodation for its treatment."

In 1905 arrangements were made to present a Memorial on the old lines to Mr. Balfour, who had then succeeded Lord Salisbury as Prime Minister, but he went out of office a few days before the date fixed for the deputation, and the subsequent political situation made it impossible to proceed further at that time.

The next effort was made in 1911. At that time the Royal Commission on the Poor Laws in its report had recommended that the law giving power to workhouse authorities to detain inmates suffering from contagious diseases should be extended so as to apply to persons suffering from Venereal Disease in the infectious stage. In view of this recommendation Col. Long's Committee decided to approach the President of the Local Government Board (Mr. John Burns) and urge that his Department should take action. A Memorial was therefore prepared, and signed by large numbers of men and women representatives of associations, committees, homes, etc., by numerous medical men holding official positions, and by men representing all professions, religions, and shades of thought. A large number of Boards of Guardians, City and Borough Councils, and other bodies passed resolutions in support of it, and it was duly presented to the President of the Local Government Board. Mr. John Burns was most sympathetic in his reception of the deputation, and promised to do all in his power to promote the cure and stay the progress of Venereal Disease by administrative action, and the Memorial was duly pigeonholed.

In 1913 Sir Malcolm Morris and several other individuals feeling strongly on the matter, in view of the International Congress due to meet in London in August, came to the conclusion that to approach politicians and heads of Departments was merely so much labour lost, and that the only way to get a chance of obtaining a full inquiry was to startle and impress the " man in the street," in order that public pressure should be brought to bear on the Government so that, yielding to panic and clamour, that should be granted which was denied

to reason, evidence, and argument. These tactics succeeded to perfection. A plain unvarnished statement of facts with regard to these diseases was drawn up and signed by practically every man in the medical profession who at that time held any official position, as well as by many of those whose names were well known to the public. The *Morning Post* took its courage in both hands, and published this manifesto, with its signatures, in the latter half of July.

The effect was magical. " The Hidden Plague " blossomed forth in every headline. The whole press was full of alarm and indignation. Many of those who had strenuously opposed any idea of inquiry were now foremost in demanding one. The covering plaster being dragged from the sore, it lay open in the full light of day, and the same evidence in public did that which, when tendered in private, it had been impotent to achieve.

The International Congress meeting a few days later passed very strong resolutions as to the necessity for action, and before Parliament rose the Prime Minister intimated that a Royal Commission would be set up, and announced the names of those who were appointed to serve on it.

This Commission has just reported, and it is a matter for congratulation that even in the din of the war of nations the suggestions and recommendations which it makes as to the conduct of this war against a disease which has slain its thousands in comparison with the hundreds who perish on the field of battle are making themselves so clearly heard.

The report of the Royal Commission is good, of great value, excellent in parts, though perhaps it does not go quite so far in some respects as many of those whose life is spent in combating the disastrous results and effects of neglected syphilis and gonorrhœa would like or approve; it is a great step, the greatest ever taken, in the right direction. It is but the first act in the play. The rest must follow in due course, and the greater the success which may be expected to crown the adoption of many of the recommendations in this report, so much greater will be the enlightenment of the people with regard to these matters, and so much the more will it become possible to take further measures in the near future, which will even more effectually curb and remedy the ravages of this dread scourge.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By Philip E. B. Jourdain, M.A. Cambridge.

The three parts of the *Jahrbuch über die Fortschritte der Mathematik* for 1912 were published in 1914 and 1915, and form a volume of 1,208 pages. The volume for 1913 has not yet begun to appear. The *Jahrbuch* is usually published about two years after the date of the year it reviews, and so this delay is much greater than usual. We must not forget the immense service that German mathematicians have done, and are doing, to the world by such work as this; and it seems a pity that such works should be advertised on the cover of the *Jahrbuch* as *Wie England eine Verständigung mit Deutschland verhinderte*.

The *Jahrbuch* is conscientiously written by an international committee of sixty-three mathematicians: two are from Poland, one from Russia, two from Holland, two from Switzerland, one from Sweden, two from Italy, one from Belgium, two from Austria, one from Bohemia, one from Denmark, two from Great Britain (of whom one has ceased to contribute), and forty-six from Germany. Even in times of peace the account of mathematical work appears too long after the original work for some purposes, but now Prof. Lampe, of Berlin, the editor-in-chief, writes that the printing of the volume for 1913 is partly done, but it has had to stop on account of difficulties caused by the war.

Another periodical having partly the same function is the Dutch *Revue semestrielle des publications mathématiques*, published by the Mathematical Society of Amsterdam. The chief advantage of the *Revue* over the *Jahrbuch* is that it appears within a fairly short time of the articles it reviews. A short account of each article is given, and no attempt is made at criticism. The list of periodicals which are regularly abstracted in the two half-yearly numbers of the *Revue* is apparently quite complete. Another point in which the *Revue* has a very decided advantage over the *Jahrbuch* is that in the *Revue* references

are given to all the reviews of mathematical books which appear in current literature : occasionally there are valuable remarks in reviews, and so this is a good feature. It is of interest to mention the nationalities of the collaborators of the *Revue* : twenty-eight are Dutch, three are Polish, five are Russian, one is Italian, two are Hungarian, one is Bohemian, one is German, one is Japanese, one is Rumanian, one is Serbian, and two are American. One of these Americans has ceased to contribute, and the vacant place has now been taken by one Briton. These figures, especially when they are taken in conjunction with those given for the *Jahrbuch*, are very illuminating. Few things could bring home to us more clearly the small part which many of the nations, and particularly our own, are taking in the very necessary work of the organisation of mathematical research. It must be remembered that many of the men mentioned above are working for both the *Jahrbuch* and the *Revue* : thus in Great Britain one man alone is working in this particular field of organisation. This disgraceful state of things is of course common to many branches of science ; and in this connection we may refer to a long letter from Sir Ronald Ross in *Nature* (1916, **96**, 536).

A few words are necessary on the relation of our accounts of recent progress in mathematics to the account which is given by the *Revue*. In the accounts in this quarterly, some attempt is made at selection and criticism. Of course that necessarily implies that the views of a particular man come into consideration, and, though the accounts published in the *Revue* have the merit of being as nearly as possible unprejudiced and uncritical, yet for certain purposes it may be valuable to view things from a unitary standpoint. In particular, it will be noticed that in this quarterly attention to geometrical work is confined to those papers which are of interest from the point of view of method ; and particular attention is paid to work on the history and logical side of mathematics, and to analysis.

On February 11 Richard Dedekind (1831—1916) died at Brunswick. He was one of the chief contributors to the theory of algebraic numbers, but his best-known works were his tracts on continuity and irrational numbers (1872) and on the nature and meaning of numbers (1888). These tracts are of fundamental importance to the principles of mathematics, and the later tract contains an independent elaboration of that part of

the logic of relations (of De Morgan, C. S. Peirce, and Schröder) which is necessary for founding arithmetic, and occupies an important place in the writings of Frege and Russell. English translations of both of these *Essays* have been published, and they hold an important place in that series of writings which is associated with the names of Weierstrass, Dedekind, Cantor, Frege, Peano, and Russell. Prof. G. B. Mathews (*Nature*, 1916, **97**, 103) gives a short and good account of the mathematical work of Dedekind.

A biography of Benjamin Williamson (1827—1916) is given in *Nature* (1916, **96**, 541); and biographies and lists of the works of Luciano Orlando (1877–1915) and Ruggiero Torelli (1884–1915) are given in the *Bollettino di bibliografia e storia delle scienze matematiche* (1916, **18**, 1 and 11).

March 16, 1916, was the seventieth birthday of the great Swedish mathematician, Prof. G. Mittag-Leffler, and he and his wife celebrated the occasion by bequeathing their entire fortune to the foundation of a new international institute for pure mathematics.

History.—The work of Isaac Barrow is, it has become increasingly recognised of late years, very important for the early history of the infinitesimal calculus. J. M. Child (*Monist*, 1916, **26**, 251) gives a short account of the *Lectiones Geometricæ* of 1670, and his translation of the work will before very long be published by the Open Court Publishing Company of Chicago and London.

We must here notice some very able and learned reviews by Prof. R. C. Archibald of books dealing with Napier's *Descriptio* and *Constructio* (*Bull. Amer. Math. Soc.* 1916, **22**, 182), mathematical quotation books (*ibid.* 188), and the history of the construction of the regular polygon of seventeen sides (*ibid.* 239).

J. H. Graf (*Boll. di bibl. e st. delle sci. mat.* 1916, **18**, 21) gives the first part of an article on the correspondence between Ludwig Schläfli and the Italian mathematicians. This part contains some letters of Casorati.

The influence of mathematical conceptions on Berkeley's philosophy, as shown by his attempts to apply mathematical conceptions and methods to problems of nature and morality, is dealt with by G. A. Johnston (*Mind*, 1916, **25**, 177). Berkeley's own important contributions to the logic of the infinitesimal

calculus are not reviewed in this paper, though the author will shortly deal with them in a paper to be published elsewhere.

Logic and Principles of Mathematics.—The question as to what Zeno really meant to prove or disprove by his arguments on motion has excited some interest in modern times on account of the enormously important logical questions about infinity and continuity which were first raised by these arguments. Philip E. B. Jourdain (*Mind*, 1916, **25**, 42) attempts to give a connected account of Zeno's purpose, which account is somewhat different from the well-known interpretation of Paul Tannery; and it is interesting to read how the modern theory of aggregates permits us to disprove a plausible "proof" that a flying arrow does not move even in a continuous space.

The strange views of some eminent mathematicians, such as Poincaré, Schoenflies, and many others, in their treatment of the logical difficulties which underlie questions of fundamental importance in mathematics are reviewed, among many other developments and views of Russell's philosophy, by Jourdain (*Monist*, 1916, **26**, 24). At bottom these views simply reduce to the method of not mentioning the difficulties, and we thus have an instance of a curious logical weakness of very many mathematicians.

The views of the late Julius König of Budapest on these questions were fairly fully given to the world in his *Neue Grundlagen der Logik, Arithmetik und Mengenlehre* (Leipzig, 1914), and an account of this book is given by G. Vivanti (*Boll. di bibl. e st. delle sci. mat.* 1916, **18**, 37).

Frege's treatment of psychological logic in his *Grundgesetze* is translated in the *Monist* (1916, **26**, 182). It is a necessary preliminary to his logical views, which are of such deep importance to the principles of mathematics. An example of how mathematicians are led nowadays to logic is afforded by the perception by reflecting ones among them that *verbs* express generalised *functions*. Such glimpses of the entities with which thought deals is not only of great logical and philosophical importance; it has even shown itself to be of importance in the technical development of mathematics.

There have been recently published two papers dealing with the formulation of branches of mathematics by axioms. In the first place, Dr. R. L. Moore (*Bull. Amer. Math. Soc.* 1916, **22**, 225) shows that a certain non-metrical pseudo-Archimedean

axiom plays a part in geometry which is to a certain extent analogous to that played by the metrical axiom of Archimedes. In the second place, Dr. B. A. Bernstein (*Trans. Amer. Math. Soc.* 1916, **17**, 50) improved on Sheffer's (1913) set of five postulates for Boole's logic by choosing the same primitive ideas (*class* and *operation*) as Sheffer, and deducing Sheffer's five postulates from four independent ones. This is the last chapter so far in the history of reduction in the number of postulates for the algebra of logic: in 1904 Huntington had got down as far as nine.

Theory of Numbers and Algebra.—G. H. Hardy (*Proc. Lond. Math. Soc.* 1916, **15**, 1) attacks a new aspect of the problem he calls "Dirichlet's divisor problem." Dirichlet gave a formula for the sum of terms $d(n)$ (the number of divisors of n, unity and n itself included) when n varies from 1 to x, and the problem in question is to determine as precisely as possible the maximum order of the error term in this formula.

G. A. Miller (*Proc. Nat. Acad. Sci.* Washington, D.C., 1916, **2**, No. 1) finds that the degree of transitivity of a substitution-group of degree n which does not include the alternating group of this degree is always less than $5/2\sqrt{n}-1$.

In continuation of some former researches of his, O. C. Hazlett (*Amer. Journ. Math.* 1916, **38**, 109) deals with the classification and invariantive characterisation of nilpotent algebras, and completely solves his problem for algebras in a small number of units, if the commutative and associative laws are assumed.

Here we may mention two papers dealing with vital statistics and the application of the theory of probability to pathometry. Prof. Karl Pearson, in a paper read to the Royal Society on February 24, gave the nineteenth of his mathematical contributions to the theory of evolution. Of the other paper we will attempt a somewhat fuller account. In investigating the mathematical theory of epidemics, we may begin with observed statistics, try to fit analytical laws to them, and so work backwards to the underlying cause, as in the work of Farr, Evans, and Brownlee; or we may follow an *a priori* method and assume a knowledge of the causes, construct our different equations on that supposition, follow up the logical consequences, and finally test the calculated results by comparing them with the observed statistics. This latter

method seems to have been first used by Sir Ronald Ross in previous papers, and a more advanced and general development of this previous work is now given by him (*Proc. Roy. Soc.* 1916, A, **92**, 204), in which the curves are theoretically obtained when we suppose that the infectivity-ratio remains constant or proportional to the number of individuals already affected, while simultaneously some of these are constantly losing immunity, and also both the affected and the unaffected groups are subject to special rates of birth, death, immigration, and emigration. There are no great mathematical difficulties, and the differential equations are all solved by known methods, but the application is an exceedingly interesting and important one, both theoretically and practically, of the theory of probability. Cf. also an article by M. Greenwood in *Nature* (1916, **97**, 243).

Analysis.—Prof. W. H. Young (*Proc. Lond. Math. Soc.* 1916, **15**, 35) continues his investigations of 1910, 1912, and 1913 on derivates of the integral of a summable function.

A controversy between Profs. M. Fréchet and J. Pierpont (*Bull. Amer. Math. Soc.* 1916, **22**, 295 and 298) on the new definition of Lebesgue integrals given by Pierpont in the second volume of his *Theory of Functions of Real Variables* is of some interest.

G. H. Hardy (*Proc. Lond. Math. Soc.* 1916, **15**, 72) proves at length a theorem of consistency for summable series enunciated without proof in *The General Theory of Dirichlet's Series*, by Hardy and Marcel Riesz, reviewed in this quarterly for January 1916, p. 500.

Motoji Kuniyeda (*Proc. Lond. Math. Soc.* 1916, **15**, 128) proves a theorem for a general series of orthogonal functions which is analogous to a theorem proved by Hardy and Littlewood in 1913, and is connected with Lebesgue's extension (1905) of Féjer's theorem of 1904 about the summability of Fourier's series. Kuniyeda used the method which Hobson used in a paper referred to in this quarterly (1916, **10**, 619) for proving a theorem of Weyl; but the theorem of Weyl and Hobson is not used, so that it can be deduced as a corollary of Kuniyeda's theorem.

O. D. Kellogg (*Amer. Journ. Math.* 1916, **38**, 1), in a paper on the oscillation of functions of an orthogonal set, finds the condition that a linear function of *n* orthogonal functions can

be found which will coincide with a given function $f(x)$ at any $n + 1$ interior points of the interval.

Prof. W. F. Osgood (*Trans. Amer. Math. Soc.* 1916, **17**, 1) decides four open questions in the theory of analytic functions of several complex variables by means of simple examples.

Goursat pointed out in 1891 that Lagrange's classification of the integrals of a partial differential equation of the first order into three groups is incomplete, and since then Forsyth has repeatedly drawn attention to the fact. L. L. Steimley (*Amer. Journ. Math.* 1915, **37**, 359) gives a new and complete classification of all the integrals of the linear non-homogeneous equation, and a means is developed whereby all the elusive "special" integrals can be readily determined as soon as the Lagrange general integral is known.

With respect to difference equations, Tomlinson Fort (*ibid.* 43) shows how a method developed by Liapounoff in 1902 for the linear differential equation of the second order can be extended to the difference equation in which the independent variable is restricted to integral values; C. E. Love (*ibid.* 1916, **38**, 57) adapts the methods of Dini (1899, 1900) for the integration of linear differential equations for large values of the independent variable and the parallel investigation of Ford (1907) for linear difference equations, to the study of somewhat more general classes of equations; and R. D. Carmichael (*ibid.* 185) gives a new method for investigating the solutions of linear homogeneous difference equations.

According to R. B. Robbins (*ibid.* 1915, **37**, 367) Lagrange's analytical formulation of the calculus of variations probably prevented the early recognition of a close connection between its problem and the ordinary problem of maxima and minima. In Robbins's paper the algebraic problem of minimising a sum is compared with the transcendental problem of minimising a definite integral, with interesting results.

Cauchy and others treated familiar functions by their functional equations, and E. B. Van Vleck and F. H'Doubler (*Trans Amer. Math. Soc.* 1916, **17**, 9) study analogously certain more complicated functional equations for the Theta functions.

When we consider, for example, the bending of an elastic bar under first increasing and then decreasing weights, we arrive at the conclusion that the future states of a body do not depend merely on the present state and the immediately

preceding states, but also on the *whole* set of preceding states. This has led to the investigation of what has been called "hereditary mechanics," and the only mathematical instrument for dealing with it is the modern functional calculus: integral equations and integro-differential equations allow us to solve the problems of hereditary mechanics and physics. The best treatment of the subject is contained in Volterra's *Fonctions de lignes* and *Leçons sur les équations intégrales et intégro-différentielles*. M. Winter (*Rev. de Métaphys. et de Morale*, 1916, **23**, 268) gives an easily understood account of these things, together with some philosophical considerations.

In some researches on the kinetic theory of ions in gases, F. B. Pidduck (*Proc. Lond. Math. Soc.* 1916, **15**, 89) started from Hilbert's (1912) rigorous treatment of Boltzmann's equation, in which treatment of a certain integral was reduced to a form resembling the left-hand side of an integral equation. In one case Pidduck found an integral equation and solved it numerically, apparently the first instance of numerical solution of an integral equation as distinguished from a theoretical solution. The chief use made of Hilbert's transformation is, however, to simplify calculations which might otherwise have been inextricable.

Mandelstam showed in 1912 that a certain problem in optics leads to an integral equation, and pointed out, with Lord Rayleigh, that the solution of the problem depends on the evaluation of an integral whose integrand contains a Bessel's function. G. Steić (*Amer. Journ. Math.* 1916, **38**, 97) evaluates this integral.

Geometry.—In an interesting address on the historical development and the future prospects of the differential geometry of plane curves, Prof. E. J. Wilczynski (*Bull. Amer. Math. Soc.* 1916, **22**, 317) concludes that the notions *osculant* and *penosculant* are the fundamental concepts of differential geometry. The systematic investigation of the magnitudes, loci, and envelopes determined by the various classes of osculants and penosculants, and the relations which exist between them, makes up the whole subject-matter of differential geometry. Differential properties of a general curve are merely integral properties of its osculants and penosculants.

In a similar way to his generalisation of the notion of angle (1906), G. A. Bliss (*Amer. Journ. Math.* 1915, **37**, 1) generalises

the notion of the curvature of a curve in the plane or the geodesic curvature of a curve on a surface.

J. R. Conner (*ibid.* 29) uses beautiful and suggestive hyperspatial methods in a discussion of the rational sextic curve and the Cayley symmetroid.

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

Stellar Spectroscopy.—An important series of investigations in stellar spectroscopy by W. S. Adams of the Mount Wilson Solar Observatory has recently been presented to the National Academy of Sciences, Washington, and printed in their *Proceedings*, ii. pp. 143, 147, 152 and 157, March 1916. To make these investigations intelligible to the general reader it is necessary to explain that stars have been classified by astronomers into different types, according to certain spectral characteristics; this classification can, alternatively, be made by measuring the *colour* of the star, using as a measure of the colour the difference between the star's photographic and visual brightnesses (called the colour-index). It is supposed that the stars in their history undergo a process of gradual evolution through successive types, but two theories have been suggested as to the mode in which this evolution takes place. The older and, until recently, generally accepted theory was that a star commenced by condensing from a nebula as a hot blue star which gradually cooled, passing through the various types until ultimately it became a very red cool star and finally a dark star. The newer theory supposes that a star commences as a very diffuse mass of gas, with a spectrum of the red type, which gradually condenses, getting hotter and bluer, until a certain stage after which it gradually cools off again, at the same time becoming redder. One of the most interesting problems of astronomy to-day is to settle which of these theories is correct.

The first of the investigations referred to above provides a new method for the determination of the type of a star, which permits even of small variations in type being accurately measured. Starting with a number of stars of known type, estimates were made on a definite but arbitrary scale of the differences in intensity between various lines in their spectra due to hydrogen, and to calcium and iron; it was found that

for two given lines a smooth curve could be drawn connecting their difference of intensity with the type, and such curves were constructed for a number of pairs of lines. If, then, for a given spectrum these differences of intensity are measured the type can be read from the curves and a mean value taken. This method is an accurate, rapid, and easy method of determining the type of a star.

In the second investigation the method of studying the intensities of spectral lines is applied to the determination of stellar parallaxes or distances. There are certain spectral lines which are peculiarly sensitive in their behaviour to the physical condition of the gases which give rise to them; now two stars of the same type may differ greatly in size, mass, and intrinsic brightness and therefore presumably in the atmospheres in which the spectral lines find their origin. Such differences should be evidenced in the behaviour of certain spectral lines. This is found to be the case, and by a comparison of intensities with other lines for stars of known parallax (π) and hence known absolute magnitude (M), linear relations are derived connecting the difference of intensity and magnitude for stars of different types. Thus, for any star of known type by measuring the intensity difference, M may be derived and—m being its apparent brightness—π can be deduced from the relation

$$5 \log \pi = M - m - 5.$$

The third investigation tests this method by comparing the parallaxes of stars so calculated with measured values, and the agreement in almost all cases is so close as to indicate that a method has been discovered which may revolutionise our knowledge of stellar parallaxes, an account of the gradual development of which has been given in these notes, SCIENCE PROGRESS, x. p. 121, July 1915. The method is particularly important in that it will provide good determinations of the parallaxes of bright stars of small proper motion of which comparatively few direct measures have been made.

The curves mentioned in the first investigation were based on stars of low luminosity and large proper motion. In the fourth investigation, an account is given of the application of this method for determining type to late type stars of high luminosity: it was found that the hydrogen lines in such cases were abnormally intense, and regarded by themselves

would place the star in a much earlier type. This leads to a division of the late type stars into two distinct classes, respectively of high and low luminosity. The usual method of classification failed to make this distinction. This division is required by the new theory of stellar evolution above referred to, and is a further strong argument in its favour. Another investigation pointing in the same direction was mentioned in these notes, SCIENCE PROGRESS, vol. x. p. 439, January 1916. It is interesting to note that one of the strongest arguments against the new theory was that of the spectroscopists who could not believe that a very condensed cold star could show the same type of spectrum as a very diffuse hot gaseous star. It now appears that there are differences in the spectra which, however, were not immediately apparent.

The Absorption of Light in Space.—In these notes in the last number of SCIENCE PROGRESS, p. 620, it was mentioned that recent estimates of the amount of absorption or scattering of light in space led to the surprising result that the total mass of dust to which this is due must be about 300,000 times that of the lucid stars. An important paper by Harlow Shapley in *Proc. Nat. Acad. Sci.* ii. p. 12, January 1916, may lead to a revision of this conclusion. It is obvious that, *ceteris paribus,* the reliability of a determination of this scattering is proportional to the distances of the stars upon which it is based. The estimates referred to were based upon stars which are comparatively near the Sun. Shapley has approached the matter in a different way; he has measured the colour-indices of the stars in the cluster M 13 and compared their distribution with those of stars of similar type near the Sun. Now the actual distance of this cluster is not known, but it is certain that it must be at least 5,000 light-years and probably much greater, and the estimates previously referred to would require the smallest colour indices to be at least 2·5 magnitudes, whereas many are negative and there are none greater than 2. There seems to be no escape from his conclusion that the absorption of light in space must be inappreciable. It may be that there is cosmic dust scattered in space comparatively near the centre of our Universe, but that away from the centre its density rapidly decreases. This would reconcile the results deduced from the near and the distant stars. It remains for further investigations to settle this point. In any case, the difficulty

as to the enormous total mass of non-luminous matter in space will be removed. It is perhaps necessary to recall that Barnard's observations of dark patches in the sky seem conclusively to prove the existence of large masses of obscuring matter in space, but we have no knowledge as to the distance to which such matter extends.

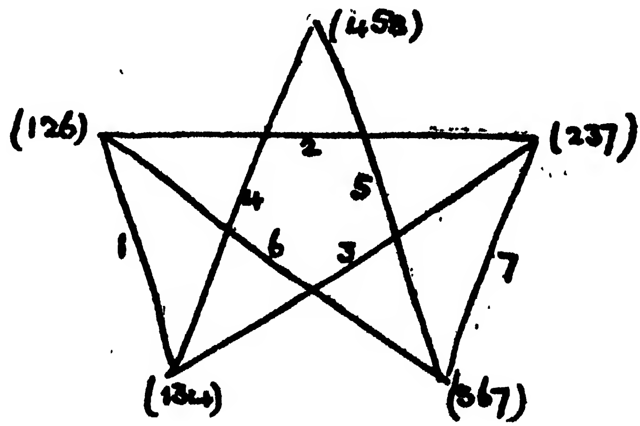
The Planets.—Some remarkable results in relation to the orbital planes of the major planets are given by Prof. Plummer in *Monthly Notices, R.A.S.*, lxxvi. p. 387, March 1916. The three *empirical* laws which he has deduced are:

(1) The poles of the orbital planes lie three by three on five lines. Otherwise, the planes are concurrent, three by three.

(2) The pole of each orbit, with the exception of Neptune, lies on two of these five lines.

(3) Each line contains the orbital poles of two adjacent major planets.

These relationships are governed by a curious law:



The figure shows a star pentagon, with two pairs of corners joined. "The sides of the inner pentagon are numbered alternately clockwise from 2 to 6; otherwise expressed, the sides of the star pentagon are numbered consecutively. The numbers 1 and 7 are placed on the additional sides to the left and right alternately." Now let the numbers 1 to 8 inclusive represent the planets Mercury, Venus, the Earth, Mars, Jupiter, Saturn, Uranus, Neptune in order of distance from the Sun. Then the corners of the figure indicate which sets of three orbital planes are concurrent.

It is remarkable that these relations, which are surprisingly accurate, have so long escaped notice. It seems hardly possible that they can also be merely chance arrangements valid only at the present time, yet there seems no room for them in planetary theory. It remains to be seen what further light investigation can throw upon these relations.

PHYSICS. By J. Rice, Lecturer in Physics, Liverpool University.

In the *Proc. Roy. Soc.* for January and February, Prof. Joly describes a very simple and elegant method by which distances of ships from shore or from each other may be estimated in fog or thick weather, and so the danger of collision considerably minimised. The method depends on the kinematic fact that if the bearing of one ship to another has the same direction as the velocity of the second ship *relative* to the first, then there will be a collision ultimately if the two ships maintain their present courses and speeds unchanged. Dr. Joly proposes that each ship should be fitted with wireless, by which it could in foggy weather periodically send out code signals giving its course and speed. In the second place it should also have some means of emitting crisp, and sufficiently loud, sounds in air, which can be timed to coincide in moment of emission with wireless signals, and as long distance wireless signals are not required, the wireless installation need not be of great power or excessively costly. An officer on a ship would receive from another ship in his neighbourhood two signals, one practically received at the moment of emission and the other received some few seconds after the first; this interval and the known velocity of sound in air determines the distance apart of the boats at the moment. With a more elaborate installation, one might use submarine signals as well. Suppose the signals are repeated at a stated interval, say two minutes, later, the officer can again determine the distance away, course and speed of the ship he wishes to avoid. It is now a comparatively simple exercise in the drawing of "vector diagrams" to determine from the double set of signals and from the course and speed of his own ship, both the bearing of the second ship to his own and also his *relative* velocity to the other vessel. If the directions of these two vector quantities are not the same, there will be no risk of collision; but if they are alike or differ by

an angle less than the possible error of the method in practice, the officer knows that there is risk of a collision, and can also predict the instant of its occurrence if speeds and courses are unchanged. Of course it is assumed that both ships are utilising the method, and that both are forewarned of the probability of a collision, and have ample time to take measures against its occurrence. Prof. Joly claims that the degree of skill or intelligence called for in timing the receipt of the signal is not so great as to limit the timing of the arrival of the signals to the more highly educated seaman; and indeed most people are aware that a very little practice with a stop watch will give all the requisite skill. If the times of the signals and the communications by wireless are then given to the navigating officer, the graphical constructions which he has to carry out are not in any way more troublesome than many with which he is familiar in laying the course of his ship. Specially ruled and prepared paper will considerably simplify the operations, and enable him to reach a conclusion in a few seconds. Indeed Prof. Joly describes a little instrument consisting of a protractor and three suitably graduated scales, which renders all the operations exceedingly simple, and enables a judgment of the risk to be formed, and the time of a possible disaster predicted, with great ease and promptitude without recourse to drawing instruments at all. The limits of error will involve a certain latitude in each determination of distance apart; but the possible error in this observation will be known, and at all events each observation is independent of its predecessor as a determination of distance, and so the errors will not accumulate. Experience with the instrument would reveal how far its predictions that another vessel is going to pass clear of his own boat could be trusted by an officer. The possibility of making five or six *independent* judgments in the space of ten minutes or so would obviously narrow the errors to close limits. The speed of the operations involved is such as to permit the navigator to deal with several vessels in his vicinity.

In the *Phil. Mag.* for March Prof. Barkla and Miss J. G. Dunlop describe some experiments recently carried out by them on the scattering of X rays, and certain conclusions concerning atomic structure which they seem to warrant. Experiments carried out by Prof. Barkla some twelve or thirteen years ago showed that when Röntgen radiation is incident on a

substance containing only light elements, the radiation proceeding from this substance is almost purely a scattered radiation, and no characteristic radiation is excited ; further, the scattering particles are not the ions or atoms, but the constituent electrons, and the intensity of the scattered radiation is proportional to the quantity and independent of the quality of the matter traversed by the primary radiation. The experimental results led to the conclusion that for the light elements, say from hydrogen to sulphur, the number of electrons per atom is almost exactly half the atomic weight. When elements of higher atomic weight were subjected to Röntgen radiation this conclusion appeared to be invalidated as far as they were concerned ; for copper was found to scatter about twice as much, and silver about six times as much, as an equal mass of one of the light elements. But it must be borne in mind that the scattering can only be taken as proportional to the number of scattering electrons when these act independently of one another, and this condition is approximately satisfied for light atoms traversed by Röntgen radiation of any wave-length within wide limits. In the heavier atoms the electrons are more closely packed, and are held by stronger retaining forces, and this independence of action is no longer possible in general. However, such independence will be more closely approached the shorter the wave-length of the primary radiation. With such a condition it might be expected that the intensity of the scattered radiation would give a similar indication of the number of electrons in an atom as that obtained for the light atoms. This expectation has been put to the test by Prof. Barkla and Miss Dunlop in the paper referred to, and the results justify the anticipation. They show that when the wave-length of the primary radiation is reduced to the order $\cdot 3 \times 10^{-8}$ cm., there is little deviation even among the heavier elements from the law of proportionality of number of electrons per atom and atomic weight ; further, there are indications that with waves still shorter, the mass scattering coefficients would become practically constant for all elements, showing the number of electrons per atom in the heavier elements to be of the order of half the atomic weight, as in the case of the lighter elements.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Disperse systems. Colloids.—Although the method of preparing colloidal solutions of metals in water and other media by sparking the metal under the surface of the liquid has been a familiar one for many years past, it nevertheless offers some problems in regard to the actual mechanism of the process which are by no means cleared up. The general consensus of opinion is that the first stage of the process, namely the disintegration of the metal, is brought about by purely thermo-mechanical means. This is, however, only a small part of the problem. The resulting system may be either stable or unstable, that is the metal may remain more or less permanently in suspension in the finely divided state, or it may rapidly agglomerate and deposit itself from the medium. We shall only possess a satisfactory view of the sparking method of producing colloids when we have solved the problem of resulting stability. Since the existence of colloidal solutions depends upon the realisation of these stable conditions it is natural to expect that the problem has been the subject of considerable investigation. One of the most interesting contributions is that of Beans and Eastlack (*J. Amer. Chem. Soc.* **37**, 2667, 1915). They approach the subject by means of the electrical conductivity exhibited by the colloidal solutions prepared by sparking. A preliminary calculation based upon the known size, charge, and mobility of the particles in a colloidal solution of gold in water showed that the specific conductivity of the colloid itself should be of the order 10^{-10} mhos. This is only 0·01 per cent. of the conductivity of the water itself, so that if the colloidal solution simply consists of the finely divided metal in suspension in unchanged water, the conductivity of the solution should not be distinguishable from that of the water. This, however, is not the case. The solution exhibits a larger conductivity than that of the water. Further, there is a marked difference in the enhanced conductivity according to the metal employed. Thus, in the case of colloidal platinum the increase in specific conductivity is eight times that observed in the case of gold. Beans and Eastlack connect this larger conductivity with the fact that colloidal platinum is more stable than colloidal gold. The idea is, that the greater stability of the platinum particles is due to the presence of small amounts

of electrolyte formed by the oxidation of the metal at the high temperature reached locally in the sparking process. The presence of chloride, bromide, iodide, and hydroxyl ions has a marked stabilising effect upon these colloidal metals even when the electrolyte is at extreme dilution (0·0001 normal), and this observation affords a convenient method for preparing such colloids. Even when no electrolyte has been added to the medium beforehand, it is well known that these colloidal metals possess a negative charge and this charge is essential to their stability. The inference to be drawn from the above experiments is, that even in those cases in which the medium is quite pure, the stability of the colloid produced is dependent upon its power of adsorbing or combining with some anion—probably a complex anion containing the metal itself. If this be accepted, it follows that the most minute trace of metallic anion is sufficient to stabilise the colloidal metal, for all attempts to detect gold in the filtrate after the coagulation of a colloidal gold solution gave negative results. It must not be thought, however, that all anions are equally effective. Thus fluorides, nitrates, chlorates, and sulphates appear to be inactive. The problem therefore appears to have slightly shifted ground, but is by no means satisfactorily answered as yet. Granted that traces of electrolytic ions, combined somehow with the colloidal particles, are the source of the charge exhibited by the particles and likewise of their stability, we are still faced with the cause of the selective effects manifested by different colloids in respect of certain anions. The investigation has reached a very interesting stage.

In connection with the preparative side of colloidal solutions a problem of some importance is the production of colloidal carbon. Bredig does not mention carbon among the colloids in water prepared by the electrical method, but Svedberg mentions the production of colloidal carbon in certain liquid organic media, the colloid being evidently produced by the action of the spark upon the medium itself. More recently a stable colloidal carbon has been obtained in dilute aqueous sodium hydroxide (but not in pure water) by Thomae, whilst Sabbatani has succeeded in producing colloidal carbon by the action of sulphuric acid upon sugar, the resulting product being poured into water. The production of colloidal carbon derives its chief technical importance at the present time on account

of the part it plays in the Acheson carbon used as a lubricant. The preparation of colloidal carbon has been recently investigated by Thorne (*Trans. Chem. Soc.* **109**, 202, 1916), who employed Sabbatani's method. Thorne concludes that although some colloidal carbon is produced by this method, its properties are obscured by more complex products of decomposition, the presence of which is obvious from the odour of burnt sugar. Further, these products do not act as stabilisers for the true colloidal form. The conclusion reached is that in pure water, stable colloidal carbon cannot be produced. The presence of alkali aids the stabilisation. Certain organic substances also assist the stable production. In the Acheson process, tannic acid is the stabilising agent, but its precise action has not yet been determined.

Whilst considering disperse systems it may not be out of place to refer to an important technical application of the property possessed by fine particles, namely their electric charge. This is exhibited not only by colloidal metals and non-metals in liquid media, but also by dust particles and fumes in air. By making use of this fact, the precipitation of smoke, fume, and dust can be successfully carried out on a considerable scale. Details will be found in two papers, viz. L. Bradley (*Met. Chem. Eng.* **13**, 911, 1915), and F. G. Cottrell (*Eng. and Min. Journ.* **101**, 385, 1916).

Continuity of state.—A valuable service has been rendered by F. H. McDougall (*J. Amer. Chem. Soc.* **38**, 528, 1916) to the study of the continuity of state by his exposition of Dieterici's equation of state. Up to the present time practically only one equation, that of van der Waals, has been subjected to close and searching examination, with the result that almost too much stress has been laid upon this particular form of representing the behaviour of liquids and vapours. As a matter of fact Dieterici's exponential expression (or rather both of his equations, for he has proposed two forms) reproduce certain experimental relations with much greater fidelity than does van der Waals' expression. This is particularly true of the ratio RT_c/P_cV_c where the suffix c denotes critical values. On the other hand the Dieterici relation $v_c = 2b$, is hard to reconcile with experiment, being even farther from the truth than van der Waals' $v_c = 3b$. The new examination of Dieterici's equation deals largely with the significance of the term b, which is

recognised to be a variable, altering with the temperature. The precise form of this variation has not been fully determined. No one can read Mr. McDougall's contribution, however, without being struck by the very extensive general applicability of Dieterici's expression.

Allotropy.—Some years ago the attention of chemists was called to a remarkable chemical paradox in the behaviour of ammonium chloride vapour when moist and when absolutely dry as observed by Johnson. That there was an abnormality at all in the behaviour of this vapour was first pointed out by Abegg. The point was, that the vapour pressure exerted by the ammonium chloride was the same whether the vapour was moist or dry, although in the first case there was large dissociation into ammonia and hydrochloric acid gas, in the second case no dissociation whatsoever. To get over the difficulty, Wegscheider suggested that ammonium chloride existed in two allotropic forms. Whilst this was shown to be sufficient to account for the anomaly, it was, nevertheless, an hypothesis of a rather *ad hoc* nature. Considerable interest attaches, therefore, to some recent measurements by F. E. C. Scheffer (*Proc. Akad. Wetenschap. Amsterdam*, **18**, 446, 1915) which go to show that two allotropic forms of ammonium chloride actually exist. From heating and cooling curves it was inferred that a transition point between the two forms exists somewhere between 174 and 187° C. Using a catalyst (*e.g.* glycerine) the point was fixed with greater precision in the region of 184·5° C., a value which is probably accurate to a few tenths of a degree. This work naturally affords considerable evidence in favour of Wegscheider's explanation of the anomalous behaviour of the vapour.

INORGANIC CHEMISTRY. By C. Scott Garrett.

Colour and Reactivity.—A series of papers on materials for experimental dispersoidology has recently been published by Weimarn in the *Journal of the Russian Physical Chemistry Society*, which in the light of recent theories of constitution and reactivity are of some considerable importance. Most chemists who have followed the developments which have resulted from the application of spectroscopic methods to the phenomenon of colour—using the word in its broadest sense—in chemical substances will be familiar with the theory recently

advanced by Baly on the subject. Baly's theory, in part at least, associates the delicate changes of colour experienced by many organic substances when the nature or concentration of the solvent or some other variant is slightly altered, with a process of opening up of the solute, in the sense that the chemical forces within the molecule as a whole are no longer so strong as originally in some particular horizon, which horizon depends on the nature of the variant impressed. The change is not one which affects what we might call the stereotyped configuration of the molecule or system, but is one in which a reactivity of some definite character, hitherto more or less latent, is developed. On these lines it is possible reasonably to explain many facts in organic reactivity which have been long looked upon as empyrical results. Whilst Baly's theory has been developed mainly in connection with organic chemistry, it is nevertheless equally applicable to inorganic chemistry. Inorganic chemistry, however, presents fewer examples of the type met with in organic chemistry, possibly because the opened-up phases have such temporary existence, or possibly because these phases are evidenced by absorption spectrum changes in an inaccessible region. The recent observations of Goldstein on the production of colour in solid salts containing infinitesimal impurities by bombardment with cathode rays, are susceptible of explanation on the lines of this theory, and the same is the case with the observations of Weimarn on coloured solutions of sulphur to which we have referred. Weimarn finds that sulphur dissolves with an indigo or blue coloration in water, ethyl, propyl, isobutyl, and amyl alcohols, acetone, glycerol, and ethylene glycol if these solvents are rendered alkaline. In a few cases the trace of alkalinity derived from the glass of the containing vessel is sufficient to induce the blue colour. In neutral or acid solutions, however, no coloration is developed—the explanation of which, on the lines of the above theory, is that the sulphur molecule complex possesses an acidic nature, and is consequently opened up and made reactive by basic or basified solvents. When the solvents possess a neutral character *per se*, or are acidified, the tendency will be for the sulphur molecule to remain unaffected or become closed, and consequently colour will be absent. Further, Weimarn has noted that any solution of a polysulphide becomes blue on heating, provided the solvent is not acid in character,

and does not exert a definite "coarse" chemical action on the polysulphide.

Again, by suitable arrangement of these two variants, temperature and concentration, it is possible to obtain colours comprising the whole range of the visible spectrum. This characteristic blue colour is probably the same as is obtained in fusions of sulphur and salts, such as potassium chloride, and solutions of sulphur in ammonia or sulphur trioxide.

Undoubtedly the sulphur molecule is a very reactive one, and since its various reactive states are evidenced by light absorption in visible and accessible regions, a study of the absorption characteristics of organic and inorganic sulphur compounds ought to prove well worthy of intensive investigation.

Weimarn's observations are confirmatory of some observations already published. By their recently developed method of preparation of pure alkaline polysulphides from alkaline hydrogen sulphides, Rule and Thomas have shown that in the pure solid state these compounds possess a clear gradation of colour, whilst in solution almost any colour can be produced by suitable variations of solvent, concentration, and temperature.

Technical.—Sulphuric acid can perhaps be said to be our most important chemical, and consequently many attempts have been made to obtain it by cheaper methods than by the two standard methods involving the oxidation of sulphur dioxide to sulphur trioxide. Bresciani in a recent publication (*Ann. chem. applicata,* **4**, 343, 1915) gives results of an attempt to prepare this acid by the action of ozone and steam on hydrogen sulphide at 120°. The hydrogen sulphide and the ozonised oxygen were present in the relative proportions of 1 to 11, but the addition of the steam reduced the percentage of the sulphide in the gaseous mixture to 0·02 per cent. After condensing the gases and separating off unchanged hydrogen sulphide with cadmium chloride, and the excess of cadmium chloride with potassium hydronide, it was found, however, that only 5 per cent. of the sulphide had been transformed into sulphuric acid.

Constitution.—An interesting paper on alumina and aluminates has been published by Martin (*Mon. sci.* **5**, 225, 1915), which marks some progress in the chemistry of this amphoteric element. Martin shows that there is a distinct difference,

suggestive of distinctive constitutions, in the hydrate formed by the auto-decomposition of the alkaline aluminates and that of the hydrate obtained by the precipitation of an aluminium salt with ammonia.

The former, dried in the air, is not hygroscopic, and possesses a constant composition of $Al_2O_3 \times 3H_2O$. Its dehydration sequence shows that a molecule of water is lost between 160° and 225° C., a second between 225° and 235° C., whilst complete dehydration is not obtained until a temperature of 1000° C. is reached. On the other hand, the ammonia precipitate is gelatinous with a composition depending on the conditions of precipitation. The dehydration sequence indicates a continuous loss of water, so that it is improbable that this form is a well-defined hydrate, and for these reasons the hydroxide formula—

$$Al \begin{cases} OH \\ OH \\ OH \end{cases}$$

is assigned to the former type.

The same investigator has isolated several series of alkaline and alkaline earth aluminates in which the proportion of basic oxide to alumina varies from 1 to 3, but in no case is this proportion less than 1.

Further, by heating mixtures of alumina, barium, or calcium sulphate and carbon in molecular proportions, he obtained thioaluminates of barium and calcium. These new salts, whose isolation throws fresh light on the acidic nature of aluminium, are soluble in water, but very soon hydrolyse and evolve hydrogen sulphide.

Analytical.—In gas analysis one of the ever-recurrent difficulties is the accurate estimation of the individual combustible gases when several of these are present in a gaseous mixture. One of the methods employed with varying success is that known as fractional combustion, and consequently the data contained in a paper by Terres and Mauquin (*J. Gasbelaucht*, **58**, 8, 1915) are of value.

These authors find that hydrogen is oxidised completely on passing over copper oxide at 250° to 300° C., whilst 6–10 per cent. of carbon monoxide still escapes complete combustion even at 305° C. Acetylene, ethylene, and benzene do not burn quantitatively at 300°, but deposit some carbon. Methane begins

to combust at 310°, and is completely oxidised at a red heat. As a general rule, admixture of hydrogen with any of these gases lowers the combustion point. Mixtures of methane and carbon monoxide cannot be separated by fractional combustion, but separation is possible when hydrogen, carbon monoxide, and methane are all present, provided the volume of the last is 50 per cent. of the mixture.

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., St. Mary's Hospital Medical School.

IN the last report reference was made to a paper by Ostromisslenski on the classification and identification of Caoutchoucs. The same author has now published a series of papers (*J. Russ. Phys. Chem. Soc.* 1915, **47**, 1885–2014) dealing with the synthesis, structure, and vulcanisation of caoutchouc. It is found that if isoprene is heated with barium peroxide and sodium at 60–70° it produces an abnormal caoutchouc, but if isoprene is first cautiously heated alone at 80–90° it is converted into a hydrocarbon containing three double bonds to which the name of β myrcene is given. The constitution assigned to this substance is represented by the formula

$$CH_2 = CH - CCH_3 = CH - CH_2 - CH_2 - CCH_3 = CH_2$$

If this latter compound is heated as above with barium peroxide and sodium it is converted quantitatively into normal isoprene caoutchouc. From these observations it is suggested that plants also synthesise rubber from β myrcene or some similar hydrocarbon rather than from isoprene, since a number of substances known to occur in plants, as for example geraniol, linalool, nerol, etc., should readily yield myrcene by dehydration. The author has also studied the question of the vulcanisation of rubber and has found that the process may be accelerated or the temperature of vulcanisation may be lowered by carrying out the reaction in the presence of an aliphatic primary or secondary amine and a metallic oxide. This may be regarded as a two-fold catalytic action in which the sulphur under the catalytic influence of the metallic oxide combines with the amine to form an unstable thiozonide according to the equation

$$2RNH_2 + 4S = RNH.S.S.S.NHR + H_2S$$

and this latter compound then acts as a catalyst in conveying the sulphur to the caoutchouc. The fact that the corresponding

thiozonides of aromatic amines are relatively stable substances which would therefore not act readily as sulphur carriers explains the observation that aromatic amines cannot be employed in place of aliphatic amines. The practical importance of these observations lies in the fact that by lowering the temperature at which vulcanisation can be effected it becomes possible to incorporate with the rubber various organic colouring matters which would not stand the ordinary vulcanising temperature without decomposing. Experiments have also been carried out on the vulcanisation of rubber by means of other substances such as the halogens or ozone in place of sulphur. In the former case a halogenide of the rubber is formed in the first instance and this substance is then adsorbed by the unchanged rubber which thereby undergoes vulcanisation. With regard to oxygen it has been shown that if rubber is exposed to moist air at the ordinary temperature it becomes covered with a film of less elastic material. If such rubber is then passed through rollers in order to make it as homogeneous as possible and is subsequently heated it becomes vulcanised, whereas, if the less elastic outer layer is removed before heating no vulcanisation ensues. It follows from this that the vulcanising agent in this case is the material formed by the action of the oxygen upon the rubber. From the widely different chemical nature of the various substances which can be employed for vulcanising it may be concluded that the change is a purely physical one, depending on the formation of a colloidal substance with the rubber which is then able to bring about the transformation. The process may in fact be compared with the conversion of cellulose esters into collodion by the addition of camphor which takes the part of the vulcanising agent.

The change undergone by rubber on exposure to moist air is compared by the author to the so-called drying of certain fatty oils. It is of course a well-known fact that highly unsaturated oils on exposure to air become converted into viscid liquids or varnish-like solids, for which reason such oils are known as drying oils. That the change was in some way connected with the absorption of oxygen was proved some time ago, quantitative measurements having shown that an oil may absorb as much as 24 per cent. of its weight of oxygen. Several authors have drawn attention to the fact that volatile products are given off during the process, but the nature of these sub-

stances has not been definitely established. Salway (*Trans. Chem. Soc.* 1916, **109**, 138), who has taken up this question again recently, finds that when linseed oil or the free acids isolated from this substance or linolenic acid are shaken with oxygen in a bulb tube heated to 100° C. acrolein is given off. The amount obtained is only small, but it confirms the observation of previous workers who had noticed the odour of acrolein about drying linseed oil. These authors, however, attributed its presence to the action of the oxygen upon the glycerol in the oil, whereas the present author has been able to obtain the substance from the free acids in the absence of any glycerol whatever. In order to explain its formation from linolenic acid he is inclined to reject the hitherto accepted formula for this acid. It is further suggested that the dry oil contains polymerised acrolein and glyoxal, but further research will be necessary before these points can be regarded as established.

With a view to determining the source of fluorine in the animal body, Gautier and Clausmann (*Compt. Rend.* 1916, **162**, 105) have determined the percentages of fluorine and phosphorus in a number of different plants which serve as food for human beings and herbivorous animals. The analyses do not point to the existence of any particular group of plants in which there is an abundance of fluorine. The leaves are generally the part of the plant richest in fluorine and in phosphorus, and the stem, wood, and bark are poorest. Although there is no evidence of any definite ratio of fluorine to phosphorus in the various parts of a plant, the two elements appear to increase and decrease together.

Two recent patents emanating from the firm of Meister, Lucius & Brüning (D.R.P. 286596 from *J. Soc. Chem. Ind.* 1915, **34**, 1168 and Eng. Pat. 1915, 1288, from *J. Soc. Chem. Ind.* 1916, **35**, 141) deal with reactions of a somewhat novel character. The first one describes the preparation of aluminium ethoxide which is obtained by treating dry ethyl alcohol with aluminium and a very small quantity of mercuric chloride as a catalyst in presence or absence of alkyl haloids or iodine; distillation under reduced pressure yields a distillate free from mercury. According to the second patent an 85 per cent. yield of ethyl acetate may be obtained by gradually adding 400 parts of acetic aldehyde to a filtered solution of 16 to 20 parts of aluminium ethoxide in 45 parts of dry solvent naphtha or other

organic solvent of high boiling point, such as nitrobenzene or xylene, the temperature being kept between 0° and 15°. After twenty-four hours the mixture is distilled; the first runnings consist of unchanged aldehyde and then follows ethyl acetate.

The preparation of urea from ammonium carbonate has also recently formed the subject of two patents brought out by the Badische Anilin- und Soda-Fabrik (Eng. Pats. 1914, 23939 and 24117). According to the latter ammonium carbonate is treated in an autoclave and the product is then heated in a vacuum at about 75°; unchanged ammonium compounds distil out as carbon dioxide and ammonia and recombine in the cooled receiver, while urea and water remain behind in the still. The water is removed by further distillation in vacuo.

The estimation of urea by urease has again received attention in papers by van Slyke and Cullen (*J. Biol. Chem.* 1916, **24**, 117) and Ibañez (*Anal. Fis. Quim.* 1916, **14**, 28); on the other hand Mom (*Chem. Weekblad,* 1916, **13**, 72) describes a method involving the use of *Urobacillus Pasteurii* which converts urea into ammonium carbonate which can be estimated volumetrically.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

General and Stratigraphical Geology.—The first volume of the geological results of the Shackleton Antarctic Expedition of 1907–9 is now to hand. As might be expected, more than half the volume, which is written by Prof. T. W. E. David and R. E. Priestley, deals with the manifold ice-forms and ice-work of Antarctica. Many proofs are adduced that the glaciation of the continent is now in a waning phase. The Ross Barrier, for example, has shrunk vertically about 1,000 feet, and horizontally at least 200 miles, since the period of maximum glaciation. An instructive parallel is drawn between the Ross Barrier and the ice sheet which filled the North Sea during the Pleistocene glaciation. An ascent of Mount Erebus has added much to knowledge of the vulcanism of the Ross Sea area. Including the present, Erebus has had no less than four distinct craters. The stratigraphical geology of Victoria Land is dealt with at length. The succession includes Pre-Cambrian gneisses and limestones, a Cambrian formation containing Archæocyathinæ, and sandstones referred to Gondwana sedimentation. The igneous rocks include

older and newer granites, great quartz-dolerite sills similar to those of Tasmania, and the Kainozoic lavas and tuffs of Erebus and other centres.

In a paper on the Annan Red Sandstone Series of Dumfriesshire (*Trans. Geol. Soc. Glasgow*, 1916, **15**, Part III. 374), Prof. J. W. Gregory and Dr. J. Horne indicate the succession and lithological differences of two sets of sandstones. The Annan Series, an aqueous deposit, is probably of Bunter age; the Dumfries sandstone, an unmistakeable desert formation, is correlated with the Penrith Sandstone as Permian, a conclusion supported by the evidence of the fossil reptilian footprints.

The intricate Carboniferous stratigraphy of the Midland Valley of Scotland is further elucidated by P. Macnair, in a paper on "The Hurlet Sequence in North Lanarkshire" (*ibid.* 387).

W. N. Benson has continued his studies on the geology and petrology of the Great Serpentine Belt of New South Wales by the publication of a paper on the geology of the Tamworth district (*Proc. Linn. Soc. N.S.W.* 1915, **40**, 540). There is a fine development of Devonian rocks in this area, including the radiolarian group made classic by Prof. T. W. E. David, and a great series of pillow-lavas and associated intrusions.

Glacial Geology.—In a paper on the age of Loch Long, the most typical fiord in south-western Scotland, and on its relation to the valley system of southern Scotland (*Trans. Geol. Soc. Glasgow*, 1916, **15**, 297) Prof. J. W. Gregory concludes that the basin of Loch Long was in existence before the Glacial Period, and that it has been deepened by glacial excavation only to a very slight extent.

From further observation of the "Arctic beds" at Ponder's End, S. H. Warren concludes (*Quart. Journ. Geol. Soc.* 1916, **71**, 164) that they form an integral part of the Low Terrace River-drift, and are not isolated patches of some earlier deposit.

The late A. M. Peach describes a very perfect pre-glacial platform which forms a coastal shelf around the mountainous island of Prince Charles Foreland, Spitzbergen (*Trans. Edinburgh Geol. Soc.*, 1916, **10**, 289). This platform terminates inland at a height of 150 ft. above sea level, and is backed by lofty cliffs or precipitous slopes, which indicate origin by marine erosion. The platform also occurs along the walls of the fiord system

of the island. It is compared with rock-platforms of the same height on the coasts of Norway and Western Scotland.

Glacial deposits of Eocene age are described from south-western Colorado by W. W. Atwood (*Professional Paper* 95—*B*, *United States Geological Survey*, 1915). A typical boulder clay, with a subordinate pebbly till, rests unconformably upon a Cretaceous shale, and is unconformably overlain by an Eocene conglomerate. This discovery leaves only the Ordovician, Silurian, Jurassic, Oligocene, and Pliocene at present without some record of glacial action.

Petrology.—The Carboniferous igneous rocks of parts of the Clyde plateau have been dealt with in papers on the geology of the Kilsyth Hills, by J. V. Harrison (*Trans. Geol. Soc. Glasgow*, 1916, **15**, 315), and on the igneous rocks of Bute, by W. R. Smellie (*ibid.* 334). Mr. Harrison describes the usual types of basalt lavas from this plateau, along with bostonite and kerato-phyre dykes. Mr. Smellie describes the probably Arenig epidiorite and serpentine of Scalpsie Bay, but the bulk of his work deals with an interesting series of Carboniferous igneous rocks. Several trachyte (or bostonite) lavas, rich in albite, are intercalated with the ordinary plateau basalts and mugearites. A minor intrusion in South Bute carries numerous xeno-liths of fresh basic and ultrabasic plutonic rocks, including gabbro, anorthosite, augite-peridotite, diallage-rock, hyper-sthene-rock, and serpentine, which may represent samples of the unknown buried plutonic products of Carboniferous igneous activity in Scotland.

The petrology of the Permian necks and associated in-trusions of the Fife coast between Largo and St. Monans is described by Mrs. Wallace (*Trans. Geol. Soc. Edinburgh*, 1916, **10**, 348). Some of the rocks are nepheline-basalts; others are described as analcite-basalts and monchiquites. Large xeno-crysts of hornblende, augite, biotite, and anorthoclase, occur in the tuffs and dykes, also olivine and hornblende nodules. These necks and their contents are similar in most respects to the Ayrshire vents of the same age.

The igneous rocks of the Berwyn Hills (North Wales) are described in a posthumous paper by T. H. Cope, whose work has been edited by C. B. Travis (*Proc. Liverpool Geol. Soc.*, Cope Memorial Volume, 1915, pp. 115). The rocks are of Bala age, and consist of rhyolites and andesites with their respective

tuffs. Intermediate and basic intrusions are also described. A new type, *hirnantite*, is described, but the analysis given is at variance with the mineralogical description, and the rock appears to be an albite-keratophyre.

W. N. Benson has described the petrology of the Devonian igneous rocks of the Nundle district, New South Wales (*Proc. Linn. Soc. N.S.W.* 1915, **40**, 121). The principal group is an albite-rich keratophyre-spilite-dolerite series, similar to certain pillow-lava series of Britain, and like them containing fine pillow-form masses believed to have been formed by intrusion into soft muds. A type called *magnetite-keratophyre* is believed to be due to magmatic differentiation, assisted by pneumatolysis. The albite in these rocks is regarded as of primary origin.

A great series of pillow-lavas associated with thick chert beds is described from the Kenai Peninsula, Alaska, by G. C. Martin *et alia* (*Bulletin* 587, *United States Geological Survey*, 1915). They are probably of Triassic age.

Curious pyroxene-felspar dykes, consisting of orthoclase and diopside, are found at Copper Mountain, Alaska (*Professional Paper* 87, *United States Geological Survey*, 1915, 40). These vary widely in composition, ranging from 90 per cent. orthoclase and 10 per cent. pyroxene, to 30 per cent. orthoclase and 70 per cent. pyroxene. One of the orthoclase-rich varieties, on analysis, is shown to have over 12 per cent. of potash, with 62 per cent. of silica, and falls into a new subrang, *hettose*, of the American Quantitative Classification.

Economic Geology.—The pressure of the war in increasing the demand for certain minerals of economic importance has led the Geological Survey to commence the publication of special reports on the Mineral Resources of Great Britain. The first three volumes are now to hand, dealing respectively with Tungsten and Manganese Ores, Barytes and Witherite, Gypsum and Anhydrite. The treatment is the same in each volume: there is first an introduction dealing with the character, sources, and uses of the minerals, followed by a detailed account of the British occurrences and mines.

Per Geijer in a paper on problems in iron-ore geology in Sweden and America (*Economic Geology*, 1915, **10**, 299) makes an instructive comparison between the metallogenetic provinces of the United States and Sweden in which banded magnetite-

quartz ores occur. Geijer upholds the theory of sedimentary origin for ores of this type. The great iron-ore bodies of Lapland are regarded as due to magmatic differentiation, and are compared with those of Iron Mountain and Pilot Knob, Missouri.

The Oriskany (Silurian) iron ores of Virginia are described by C. M. Weld (*ibid.* 399) as having originated as syngenetic bog-iron deposits in the Oriskany Sandstone. Subsequent solution and replacement by meteoric waters has concentrated and enriched the original deposits.

The same author discusses the ancient sedimentary iron ores of British India (*ibid.* 435). The universal character of this series is the presence of banded hæmatite-magnetite-quartz rocks, in which the pure ores range in size from mere films to great tabular lenses. They are interbedded with ancient unfossiliferous sediments (Dharwar) corresponding in stratigraphical position with the Huronian of America. They are regarded as slightly metamorphosed sedimentary ores, and are strikingly like the Pre-Cambrian *itabirite* ores of Brazil.

ZOOLOGY. By CHARLES H. O'DONOGHUE, D.Sc., F.Z.S., University College, London.

Protozoa.—To the work commenced by Hickson and Wadsworth, Lapage and Wadsworth have added *Dendrocometes paradoxus* (Stein), Part II. Reproduction (Bud-formation) (*Quart. Jour. Micro. Sci.* vol. lxi. March 1916). This form of reproduction takes place by internal budding, and only one bud is produced at a time. The accompanying nuclear changes are dealt with. In an investigation of "The Gregarines of *Glycera siphonostoma*" (*Quart. Jour. Micro. Sci.* vol. lxi. March 1916) Pixell-Goodrich has found four distinct species, including one Gonosporan. This species, *Gonospora glyceræ*, sp. n., whose spores are more complicated than is generally the case in the genus, is surrounded by a layer of host epithelium during most of its life, and association is accomplished by a characteristic dove-tailing. The nuclear and cytological changes that accompany the phenomenon known as endomixis form the subject of a paper, "The Periodic Reorganisation Process in *Paramœcium caudatum*" (*Jour. of Exper. Zool.* vol. xx. February 1916), by Erdmann and Woodruff. Nearly fifty years ago bottles of soil were stored at Rothamsted Laboratory, and two new species of Amœba

and three species of Flagellates, one new, have been found in it by Goodey: "Observations on the Cytology of Flagellates and Amœbæ obtained from old stored soil" (*Proc. Zool. Soc.* March 1916).[1]

Invertebrata.—A "Preliminary Notice of some Irish Sponges" is given by Stephens (*Ann. and Mag. Nat. Hist.* No. 99, March 1916), and contains an account of some monaxonellids of the sub-order Sigmatomonaxonellida.

Hargitt continues his studies on the Hydromedusæ by "Germ Cells of Cœlenterates: II. *Clava leptostyla*" (*Journ. Morph.* vol. xxvii. March 1916).

The "Organs of Special Sense in *Prorhynchus applanatus*, Kennel" (*Journ. of Morph.* vol. xxvii. March 1916), a planarian, have been described and their histology figured by Kespner and Taliaferro. Baylis contributes notes "On *Crassicauda crassicauda*," a nematode and its hosts (*Ann. and Mag. Nat. Hist.* No. 97, January 1916), and "The Nematode Genus *Tanqua*, R. Richards" (*ibid.* No. 99, March 1916). Descriptions of a number of worms, mainly Sabellidæ, but some Terebellidæ, are contained in "Notes from the Gatty Marine Laboratory, St. Andrews, No. xxxvii." (*ibid.* No. 97, January 1916) by M'Intosh. In the *Quart. Jour. Micro. Sci.* vol. lxi. March 1916, are two papers on worm development. Haswell, "On the Embryology of *Stratiodrilus Histriobdellidæ*," shows that there is no metamorphosis in this form, and that the processes resemble those in Rotifers. Gatenby describes "The Development of the Sperm Duct, Oviduct, and Spermatheca." Experimental results are recorded in "An Analysis of the Process of Regeneration in certain Microdrilous Oligochætes" (*Jour. Exper. Zool.* vol. xx. February 1916). Cummings has notes on parasitic insects in the *Ann. and Mag. Nat. Hist.*: on "New Species of Lice" (No. 97, January 1916), and a "Note on the Thorax in Anoplura and in the Genus *Nesiotinus* of the Mallophaga" (No. 98, February 1916), and in *Proc. Zool. Soc.* (February 1916) he deals with the structure and development of the various Anophura and Mallophaga obtained from animals in the Zoological Gardens. An interesting paper by Imms (*Quart. Jour. Micro. Sci.* vol. lxi. March 1916) treats of a subject indicated by its title, "Observations on the Insect Parasites of some

[1] In the case of papers from *Proc. Zool. Soc.* the date given is the date of reading before the Society and not the date of publication.

Coccidæ: I. On *Aphelinus mytilaspides*, Le Baron, a Chalcid Parasite of the Mussel Scale (*Lepidosaphes ulmi* L.)." It is the principal parasite of the mussel scale, but although it has two generations a year, its limited power of migration and low fecundity render it less effective than an insecticide. Butterflies and moths are treated in Joicey's "New Lepidoptera from Dutch New Guinea" (*Ann. and Mag. Nat. Hist.* No. 97, January 1916), and Hampson and others on a collection of moths made in Somaliland by W. Feather (*Proc. Zool. Soc.* February 1916). Turner continues his "Notes on Fossorial Hymenoptera: XIX. On new Species from Australia" (*Ann. and Mag. Nat. Hist.* No. 97, January 1916), XX. "On some Larrinæ in the British Museum" (*ibid.* No. 99, March 1916), and also describes "Two new Species of the Hymenopterous Genus *Megalyra* Westw." (*ibid.*). Edwards writes "On the Systematic Position of the Genus *Mycetobia* Mg." (*ibid.* No. 97, January 1916), and Jackson "On the Nomenclature and Identity of some little-known British Spiders" (*ibid.* No. 98, February 1916). New forms are recorded by Townsend, "Two New Genera of African Muscoidea" (*ibid.*), and Bagnall, "Brief Descriptions of new Thysanoptera: VII." (*ibid.* No. 99, March 1916). Evidence indicating the constancy of the chromosomes and their retention of definite peculiarities and characteristics is adduced by Browne in "A Comparative Study of the Chromosomes of Six Species of *Notonecta*" (*Jour. Morph.* vol. xxvii. March 1916), and it is shown that all contain an XY pair of chromosomes. A short discussion of Zwitterbienen, "The Eugster Gynandromorph Bees" by Morgan, will be found in *Amer. Nat.* vol. l, January 1916.

Records of new molluscs are made by Preston in "Descriptions of new Freshwater Shells from Japan" (*Ann. and Mag. Nat. Hist.* No. 98, February 1916), and "Descriptions of a new Species and Sub-species of *Ennea* from Northern Nigeria, and a "Correction in the Original Description of *E. Reesi*" (*ibid.* No. 99, March 1916). Two papers by Sakyo recount "Studies on the Geotropism of the Marine Snail, *Littorina littorea*" (*Biol. Bull. Woods Hole*, vol. xxx. January 1916) and "The Geotropism of Freshwater Snails" (*ibid.*).

Vertebrata.—A "Note on Intra-uterine Eggs of *Heterodontus* (Cestracion) *Phillipi*" is contributed by Haswell (*Quart. Jour. Micro. Sci.* vol. lxi. March 1916), and a "Description of Three

new Cyprinid Fishes from East Africa" by Boulenger (*Ann. and Mag. Nat. Hist.* No. 99, March 1916). Allis adds another paper to his work on the arterial system of Elasmobranchs by discussing "The So-called Mandibular Artery and the Persisting Remnant of the Mandibular Aortic Arch in the Adult Selachian" (*Jour. Morph.* vol. xxvii. March 1916).

Reasons for not regarding the germinal thickenings in the ovary of some amphibia as stored-up germ cells are given by Gatenby in "The Transition of Peritoneal Epithelial Cells into Germ Cells in some Amphibia Anura, especially in *Rana temporaria*" (*Quart. Jour. Micro. Sci.* vol. lxi. March 1916). Intermediate stages between peritoneal cell and germ cell are described and figured. In the *Jour. Exper. Zool.* vol. xx. February 1916, Burr gives an account of "The Effects of the Removal of the Nasal Pits in *Amblystoma embryos*," Spaeth produces "Evidence Proving the Melanophore to be a Disguised Type of Smooth Muscle Cell," and in dealing with "The Reactions of the Melanophores of Amblystoma larve—the Supposed Influence of the Pineal Organ," Laurens concludes that this body has no influence on the melanophores.

Certain tortoises and lizards examined by Detwiler possess no rods but only cones in the retina. He deals histologically with the migration of the retinal pigment when acted upon by light in these forms in "The Effect of Light on the Retina of the Tortoise and of the Lizard" (*ibid.*). A "Description of a new Snake of the Genus *Coluber* from Northern China" is given by Boulenger (*Ann. and Mag. Nat. Hist.* No. 99, March 1916).

Pearl's recent work on fowl-breeding has been criticised by Castle, and Pearl meets this criticism very fully in a note, "Fecundity in the Domestic Fowl and the Selection Problem" (*Amer. Nat.* vol. l. January 1916).

A connected account of early marsupial development, founded on the examination of a good series of well-preserved stages, forms the basis of "Studies in the Development of the Opossum *Didelphys virgiana*: I. History of the Early Cleavages; II. Formation of the Blastocyst," by Hargitt (*Jour. of Morph.* vol. xxvii. March 1916). Thomas contributes a series of papers on the smaller mammals to the *Ann. and Mag. Nat. Hist.*: "The Porcupine of Tenasserim and Southern Siam" and "On the Grouping of South American Muridæ that have been referred to *Phyllotis, Euncomys,* and *Eligmodontia*" (No. 97,

January 1916); "On the Generic Names of certain Old-world Monkeys," and "Notes on Argentine, Patagonian, and Cape Horn Muridæ" (No. 98, February 1916); and "A new Binturong from Siam," "A new Genus for *Sciurus pœnsis* and its Allies," and "Notes on Bats of the Genus *Histiotus*" (No. 99, March 1916). In the same publication are two communications by Pocock, "A new Genus of African Mongooses, with a note on *Galeriscus*" (No. 98, February 1916), and "On the Course of the Internal Carotid Artery and the Foramina connected therewith in the Skulls of the Felidæ and Viverridæ" (No. 99, March 1916). In "The Genitalia of Galeopithecus" (*Jour. Anat. and Physiol.* vol. l. January 1916) Jones gives a description of the genital organs of this form together with their probable derivation, and concludes that they show relationship with the Chiroptera. Dollman continues his previous notes "On the African Shrews belonging to the Genus *Crocidura*" (*Ann. and Mag.* No. 98, February 1916). Wodsedalek concludes that there has been no authentic case of fertility in mules in a paper on the "Causes of Sterility in the Mule" (*Biol. Bull. Woods Hole*, vol. xxx. January 1916), and gives an interesting account of the spermatogenesis. "Further Observations on the Intestinal Tract of Mammals" are made by Mitchell (*Proc. Zool. Soc.* February 1916).

General.—The late Prof. Minchin's address to the Zoological Section of the British Association on "The Evolution of the Cell" has been reprinted in two parts: Part 1, *Amer. Nat.* vol. l. January 1916, and part 2, *ibid.* February 1916. The same journal also contains a paper by Stockard and Papanicolaou entitled "A Further Analysis of the Hereditary Transmission of Degeneracy and Deformities by the Descendants of Alcoholised Mammals." The experiments are stated to show the hereditary transmission through several generations of conditions resulting from an artificially induced change in the germ cells of one generation. Experiments on "The Control of Sex by Food in Five Species of Rotifers," by Whitney (*Jour. Exper. Zool.* vol. xx. February 1916), yielded essentially similar results in all species. Poor feeding brings about the production of all or a large proportion of female-producing daughters, whereas an optimum food causes male-producing daughters to appear.

BOTANY. By F. Cavers, D.Sc., A.R.C.Sc.

Plant Physiology.—That despite the large amount of work done on the transpiration of plants we are still very much in the dark regarding this important process is emphasised by Muenscher (*Amer. Journ. Bot.* **2**), whose results are not the less valuable because negative. He finds no constant relation to hold between the amount of water lost and (1) the numbers of linear units of stomatal pore, *i.e.* number of stomata per unit of leaf surface multiplied by length of average pore, or (2) the length of the pore of one stoma, or (3) the number of stomata per unit of leaf surface. These results, based on the study of a fair range of species, should clear the way for further work, since they show that the variations in amount of transpiration in different plants cannot be accounted for by size and number of stomata, but must be explained by a complex of several factors. Trelease and Livingston (*Journ. Ecol.* **4**) give the results of a detailed study of the daily march of transpiration as indicated by the porometer and by standardised cobalt paper, and show that although the graphs obtained by the two methods appear to disagree, they are found on closer analysis to afford, taken together, extremely valuable indications of transpiring power in plants. In an elaborate paper Briggs and Shantz (*Journ. Agric. Res.* **5**) discuss the hourly transpiration rate on clear days as determined by cyclic environmental factors, after giving data of simultaneous automatic records of solar radiation intensity, depression of wet-bulb thermometer, air temperature, wind velocity, evaporation from a free-water surface, and rate of water-loss of various plants. Least-square reductions of the dependence of transpiration upon radiation and air temperature, or upon radiation and saturation deficit, do not account entirely for the observed transpiration, though a satisfactory agreement between computed and observed *evaporation* is obtained by the use of these environmental factors; that is, the plant undergoes changes during the day which modify its transpiration coefficient. These results support the conclusion of other recent workers that plants under conditions favouring high transpiration do not respond wholly as free evaporating systems, even if abundantly supplied with water and suffering no visible wilting.

The interesting problems centred around the permeability of cells to different ions have been worked at by various botanists

recently. Stiles and Jörgensen (*Ann. Bot.* **29**) find that the cells of potato tuber absorb hydrogen ions very rapidly, and according to a simple exponential relation between time and the concentration of the acid. The rate of absorption is increased about 2·2 times for a rise of 10° C. between 0° and 30° C. Apparently, since the rate of the reaction depends merely on the temperature coefficient and the concentration of the acid, the quantity of the substance (presumably the plasma membrane) with which the acid reacts remains constant, suggesting that either this substance is present in such large quantity as compared with the acid that the amount changed is relatively small, or that the substance formed as a result of the absorption is broken down again almost as soon as formed. Osterhout (*Bot. Gaz.* **61**), continuing his work on permeability in *Laminaria*, finds that suitable concentrations of anæsthetics produce a marked decrease of permeability, which may amount to 15 per cent. or even more, this condition lasting for a long time if the concentration is not too high, and being easily and quickly reversed by replacing the tissue in sea-water, while high concentrations produce irreversible increase of permeability. Osterhout suggests that anæsthesia in general may be due to decrease of permeability, this hindering the production and transmission of stimuli, which in turn depends upon movement of ions in the tissues; also that the irreversible increase with high concentration is due to the anæsthetic combining chemically with the protoplasm, the effect on permeability changing after a certain amount has combined.

Spoehr (*Plant World*, **19**) discusses and criticises the well-known formaldehyde theory of photosynthesis, and throws grave doubt on the possibility of a condensation of formaldehyde to sugar in plants. In his experiments, sugar appeared when lead hydroxide, calcium hydroxide, and potassium carbonate were used as "catalysts," but it appeared both in darkness and in light, and the main reaction in all cases was the oxidation of the formaldehyde to formic acid. Brown (*Amer. Journ. Bot.* **3**) has published the results of an elaborate research on the movements of the well-known Venus' fly-trap (*Dionæa*), the chief being that stimulation is at once followed by decrease in osmotic pressure of the cells of the concave region of the leaf, and later by growth-enlargement of these cells, and the appearance in them of a large quantity of starch, also that the mechanism

shows many points of apparent similarity to that of geotropic curvature.

Ecology.—Apart from detailed accounts of the vegetation of different areas, which are becoming somewhat too numerous to be noted here, some ecological works of general interest have recently appeared. Albert and Gabrielle Howard (*Agric. Res. Inst. Pusa*, Bull. **52**) have made a very elaborate study of soil ventilation, from observations on the growth of Indian crops, emphasising the importance of this factor in plant life, and the necessity for taking into account the different needs of different species in this respect. Stapledon (*Ann. Bot.* **30**), from a study of the weeds of farm land, lays stress on the importance of investigating these on a statistical basis, and in the light of the community as a whole; his results show that the weed communities are decidedly responsive to change in soil, are different near the altitudinal limits of cultivation from those on the same soils at lower elevations, and are influenced by the crop under which they grow—this last due largely to the husbandry associated with the various crops.

In a valuable paper on the periodicity of freshwater algæ, Trauseau (*Amer. Journ. Bot.* **3**) brings together many observations and draws interesting conclusions, some of which appear to be novel, as, for instance, that the normal length of the vegetative cycle in *Spirogyra* is an inverse function of the surface area of the cells, also that the concentration of natural waters at their maximum is so small in comparison with the concentrations of the cell-sap as to make it very doubtful whether it is of any significance in initiating reproduction.

Willis (*Phil. Trans. Roy. Soc.* **106**) calls attention to the remarkably high proportion of endemic species in the flora of Ceylon—over 800 species out of 3000, and 23 of the 324 genera represented. Six families with seven species each are entirely endemic; four families with a total of 102 species have 91 of them endemic; and 14 families containing 435 species include 255 which are endemic. Over a hundred of the endemic species are extremely rare, in some cases being represented only by about a score of individuals on a mountain top. Dr. Willis considers that the various features of occurrence of the Cingalese endemics form "an insuperable objection to the theory of natural selection and adaptation." For instance, the local species are the rarest, which should not be the case if they had

developed in response to local conditions. It is concluded that this astonishingly large number of endemics owes its origin to mutations which may be almost of generic amplitude—the endemic species do not show close similarity to their nearest relatives.

PHYSIOLOGY. By W. L. Symes, M.R.C.S., University of London.

The most interesting event of recent months is the issue of a volume of Abstracts of papers in physiology and related subjects by the Physiological Society of Great Britain and Ireland, with the co-operation of the American Physiological Society and of other societies and journals in this country and abroad. The publication should be seen by all whose work and interests are touched by physiology. It is reviewed in a later page (176) of this number.

The *Journal of Physiology* (vol. l.) presents a new feature, viz. a paper on history, in which Langley describes the progress of discovery, as regards the autonomic nervous system, during the eighteenth century. Of particular interest and value would be an extension of that history to the present time. So much of it arose in Cambridge that its collation, and presentation, by Cambridge would be peculiarly appropriate.

Two papers on muscle tonus appear in the current (vol. xxxviii.) issue of *Brain*. In the first of these Sherrington defines tonus as a posture, and contrasts the conditions during tonus with those during actual shortening of muscle. In the second paper Langelaan defining tonus in terms of the change produced by increasing the muscular load, discusses the rôle of "elasticity" and of "plasticity" in this state. His definition of elasticity is unfortunate, and must be kept clearly in mind if the paper is to be understood. Van Rijnberk (*Arch. Néerland,* vol. iii. B) also contributes an interesting paper on muscle tonus. He has investigated the paradoxical lingual phenomenon of Vulpian, together with the pseudo-motor reaction (Rogowics) of the dog's lip, and concludes that these represent varying tonus of striped muscle and not merely vaso-motor changes. All these papers dilate on the dual contractility of muscle already described, or implied, by previous writers (notably by Botazzi, by Boeke, by de Boer, and by Graham Brown), in connection with the dual efferent innervation by cerebro-spinal and by autonomic nerves respectively.

Macht, Herman, and Levy have studied (*J. Pharmacol.* vol. viii.) quantitatively the analgesia produced by the alkaloids of opium when given singly and in combination to normal men. They find the order of analgesic power to be morphine, papaverine, codeine, narcotine, narcein, thebaine. Combinations of morphine with the other alkaloids were much more effective than morphine alone. Narcotine and morphine proved a particularly efficacious mixture.

O'Connor (*Proc. Roy. Soc.* vol. lxxxix. B) gives an interesting account of observations on the regulation of body-temperature during anæsthesia. Anæsthetic animals consume oxygen at a rate proportional to their body temperature, except when shivering. Shivering depends on fall of temperature in the brain and leads to increased consumption of oxygen. This increment is proportional to the difference between the skin temperature of the moment and the brain temperature at which shivering begins.

The measurement of blood pressure on man is discussed (*Amer. Journ. Physiol.* vols. xxxix. and xl.) by Erlanger, and by Brooks and Luckhardt. The latter workers find that the criteria usually accepted for systolic, and for diastolic, pressures give readings which are too high. With normal arteries this error is small, but with increasing rigidity of the vessel wall it may become considerable. Erlanger discusses the interpretation of the maximum pressure oscillations, and of the sound of Korotkoff.

Ainley Walker (*Proc. Roy. Soc.* vol. lxxxix. B) finds body-length (stem-length) throughout the period of growth (in man) to fit the formula $l = kW^n$. In males, n = ·33 and k averages 23·33; in females, n = ·32 and k averages 25·58. If the stem-length of an individual differs by 17 per cent. from its calculated value, the individual is "certainly abnormal."

Three papers by Hekma (*Biochem. Zeitschr.* vols. lxxiii. and lxxiv.) discuss the formation and separation of fibrin. Hekma discards the term fibrinogen, describing fibrin as pre-existent in blood (and other solutions which yield it) as an alkali-hydrosol. When coagulation takes place, this fibrin-alkali-hydrosol becomes a simpler fibrin-hydrosol changing from an emulsoid (dispersed phase liquid) to a suspensoid (dispersed phase semi-solid or solid) in which the initially ultra-microscopic particles pass gradually into the characteristic threads. He regards the separation of fibrin as being independent of enzyme action,

Lenk (*Biochem. Zeitschr.* vol. lxxiii.) has made interesting comparisons of the effects of various saline solutions on the swelling of gelatine with those on beans and on fish. The results of the comparison vary somewhat with the concentration of the salts, but, in the main, those solutions which produce greatest swelling in the gelatine are the most toxic to life. Moreover, a salt which antagonises the toxicity of another salt also diminishes the effect of the same salt on the swelling of the gel. Schryver (*Proc. Roy. Soc.* vol. lxxxix. B) has also compared the behaviour of cells and of gels. This observer had previously found that a sodium cholate solution readily sets to a gel when heated to 50° in the presence of a calcium salt, and that this gelation is inhibited by various substances (roughly) in proportion to their toxicity. The present paper deals with the erosive effects of various agents on the gel. These are not quite parallel with the inhibitory effects of the same substances on gelation, but agree more closely with the narcotic and cytolytic action of the bodies in question. Saline antagonisms are also noted which agree with those described by Lenk. Both these writers refer to the work of Loeb on the mutual antagonism of salts, but neither to that of Ringer.

W. E. Burge (*Amer. Journ. Physiol.* vol. xxxix.) has made observations on the effect of ultra-violet radiation on living cells. He found the most effective waves had a wave-length of between 254 $\mu\mu$ and 302 $\mu\mu$. These waves caused cataract in fish when (and only when) there was a sufficiency of calcium salts. Abnormal quantities of calcium salts (and sodium silicate) in the eyelids increase this effect of ultra-violet light.

A possible relation between the pituitary and adrenal glands is suggested by the work of Shamoff (*Amer. Journ. Physiol.* vol. xxxix.) and of Hoskins (*J. Amer. Med. Assoc.* vol. lxvi.), considered in conjunction with that of Watanabe and Crawford (*J. Pharmacol.* vol. viii.). The two former writers find, independently, that some pituitary extracts cause relaxation of the intestine, comparable with that produced by adrenaline, instead of the normal effect, viz. contraction. Watanabe and Crawford state that the colour reactions of adrenaline are yielded by some samples of pituitary extract, and that such extracts produce physiological effects like those of mixtures of adrenaline and normal pituitary extracts. They have not, however, in spite

of careful trial, succeeded in isolating adrenaline from the extracts in question. That adrenaline-like effects are not restricted to adrenal extracts is further shown by Fulk and MacLeod (*Amer. Journ. Physiol.* vol. xxxix.), who find such effects yielded by acid extracts of the retro-peritoneal chromaphil tissue of man and other animals.

Jansen (*Arch. Néerland,* vol. iii. B) has investigated the formation of urea from amino-acids by the liver. He finds that the reaction is not reversed even when the concentration of urea in the blood is ten times the normal.

Synergy between spleen and liver is pointed out by L. Asher (*Biochem. Zeitschr.* vol. lxxii.). It is found that extracts of spleen which are, themselves, almost devoid of hæmolytic action markedly increase the hæmolytic effects of liver extracts. The constituent responsible for this is destroyed by boiling. Splenic extracts also increase the hæmoglobin-spitting effects of liver extracts by means of a thermo-stable substance, which is not a lipoid. Hæmoglobin was broken down beyond the hæmatin stage, but not so far as to yield un-masked iron.

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc.

WHATEVER may be the faults of Germany and Austria, those countries have no superiors, and few equals, in the laborious pursuit of science. It was reported in the English newspapers about a year ago that it had occurred to Austrian anthropologists that they possessed in the luckless Russian prisoners of war interesting and abundant material for anthropometric investigations. During the great Russian retreats in Galicia and Poland last summer, it was inevitable that a considerable number of prisoners should be captured by the Austro-Hungarian Army, and as these men hailed from many different parts of the Tsar's Empire, they were of much interest to anthropologists. The investigations have been in the hands of leading scholars of the Anthropologische Gesellschaft of Vienna, and a fairly long summary of the results is now published in the *Mitteilungen* of that society for November 1915 (Band 45, Heft 6). The examinations followed the lines of the elaborate scheme set forth in the new text-book by the Swiss scholar, Martin, and the men whose physical features were thus minutely described came, as already stated, from many distant regions, Vilna, Orenburg, Perm, Tomsk, Tobolsk, Turkestan, and many

other places. The chief prisoners' camps visited appear to have been those at Eger and Reichenberg, and the report is writted by Prof. R. Pöch. The research is not such as admits of being summarised briefly, nor such as leads directly to conclusions of any general theoretical interest, but consists merely of that detailed spade-work which is the foundation of all scientific progress.

The journal *Man* for the first three months of 1916 consists as usual of miscellaneous scraps of information. This publication is no doubt to be regarded as the chief anthropological magazine in the British Empire, but it cannot be said that it is very worthy of that position. Even from the point of view of the professed anthropologist, *Man* might without difficulty be made more interesting than it usually is; but in addition to that, it must be remembered that every science needs the interest and support of a certain proportion of the general educated public, and it is therefore the duty of anthropologists to attempt to inform the ordinary reader of the interest and importance of the science of man. This is a function which such a journal as *Man* might well perform, but which in fact it makes but little attempt to carry out. The public are not interested in the technical details of any science, nor is it necessary that they should be so. But the wider conclusions and general principles of a science can be made interesting through the medium of semi-popular articles, and there is no better method of obtaining for science more adequate recognition and appreciation. The notorious neglect of science in Britain is partly the fault of British scientists. The January and February numbers of *Man* contain articles describing some "Excavations in Malta," by T. Ashby, T. Zammit, and G. Despott.

The April number of that admirable periodical the *American Naturalist* contains an article by Prof. T. T. Waterman, of the University of California, on the "Evolution of the Chin." The human chin has attracted an extraordinary amount of attention in recent years, and there exists quite a considerable literature on the subject. Prof. Waterman criticises the theory put forward on more than one occasion by Dr. Louis Robinson that the chin prominence has been evolved through the development of articulate speech, and adopts the hypothesis (which as it happens the present writer advocated in SCIENCE PROGRESS

as long ago as October 1913) that the prominence has arisen by the rapid reduction of the size of the teeth and alveolar portion of the mandible, which has left the lower part of the jaw disproportionately large. Dr. Waterman cites the parallel case of the elephants, which possess a very conspicuous chin, and whose alveolar surface is known to have been reduced in recent geological times. The author appears, however, to have missed the interesting point that his own argument leads straight to the conclusion that *Homo sapiens* is immediately descended from a being with larger teeth than the extinct species *Homo heidelbergensis*.

On March 28 a joint meeting of the Royal Anthropological Institute and the Prehistoric Society of East Anglia was held at the headquarters of the former society in London. Several interesting, if not always very convincing, papers were read. Mr. A. E. Peake, the President of the East Anglian Society, gave a lecture on the " Recent Excavations at Grime's Graves," and put forward the opinion that the human relics found there were of Middle Paleolithic age, although apart from the supposed peculiarities of the flints not a particle of evidence was produced in favour of a Pleistocene antiquity, and the fauna found, both mammalian and molluscan, was admittedly post-Pleistocene in character. Mr. Reginald Smith supported Mr. Peake, but left the absence of Pleistocene animals unexplained. Other speakers criticised this view, however, and supported the conclusion which I expressed in SCIENCE PROGRESS in January that these interesting relics are early Neolithic. Mr. A. S. Kennard also read a highly controversial paper on the " Pleistocene Succession in England," in which he stated that he held that there had been only one glacial period in Britain during the Pleistocene, not four cold phases, as most geologists now believe. Even if the alternation of warmth and cold is not so clearly traceable in England as one could wish, it is obviously almost incredible that Britain should not have shared in the climatic changes which have been so thoroughly demonstrated on the Continent. Mr. Kennard ignored this point. The Prehistoric Society of East Anglia displays admirable energy, but it is becoming associated with opinions which most students of prehistoric anthropology regard as somewhat untenable. However, in science heretics are necessary and valuable people.

CORRESPONDENCE

MACH'S MECHANICS

To the Editor of "Science Progress"

Sir,—I am very much obliged for the kind things "C." has said of my edition of the *Supplement* to Mach's *Mechanics* on p. 679 of the current number of Science Progress, but I beg to be allowed to make a short reply to his remarks on the plan adopted of publishing the supplement separately from the *Mechanics*. This plan was my own suggestion, and I recommended it to the publishers simply because, if a new edition of the *Mechanics* were made, the student would have had to buy yet another expensive book if he wished to know the rather extensive alterations made by Mach in the last edition issued before his death. With regard to the careful preservation of what Mach wrote and the indication of when he wrote it, which are given in the English but not the German editions, I of course agree with "C." that this is of no importance and even harmful to a student: it is, however, useful to many who are concerned with the development of science to know how time and new tendencies affected Mach's thought.

Under the present system a comparatively small outlay will permit those who already possess the 1907 English edition to bring the whole up to date. The saving of about 5*s*. 6*d*. may be a consideration at the present time.

I am, Sir, your obedient servant,

Philip E. B. Jourdain.

NOTES

Petition

Spirit who dwell'st on the high hills of Thought
Far in thine eyrie of an ice-cold north ;
Sister of Eagles, whose wings bear them forth
To find—whose eyes can see what they have sought :
Dwell not for ever in reverie overwrought,
With stars and ice and visions of high worth,
Silent and still for ever above the Earth,
On the frost-firèd Pinnacles of Thought :
Descend—for the world sickens. Come, be swift !
Fall like a falchion through this dreadful gloom,
Here where we lie, and die, and throw God's gift
To murderous idols ; save us from our doom :
Great Science, such for thou dar'st doubt the truth ;
Sister of Stars and Mountains ; for that makes thee Truth.
(Dedicated to Sir William Ramsay.)

Our Duty

To-day Duty is the only living word in the whole world ; but do not let us define it too meanly. It is our duty, not only to do what we have to do, but to do it well : those who have to work must work, who have to teach must teach, and, above all, who have to think must think. And the greatest obligation lies especially upon the men who can think, and not least upon men of science and those who have been trained in her school to think the truth. We have the right to commend our methods ; for it was almost exclusively the men of science, the philosophers, the poets, and the men of great arts who constructed the superb edifice of modern civilisation—based, not upon the doings of kings and soldiers and politicians, but upon the incessant thoughts and labours of individuals often unknown or forgotten. This is so ; but it is exactly here that our duty now intervenes. It is our duty certainly to continue our enquiries into the laws of nature ; but at this moment we have

another duty, greater still, which is to impress upon humanity the fact that the same qualities of mind which have given to science such wonderful success in the past must also be used by mankind in general if they wish for a continuance of the civilisation which science has helped to make.

The war has produced an immense revolution in the mind of every man who has one—silent to-day, but which will speak to-morrow. Two years ago mankind was like a youth walking in the sunshine in a beautiful world, confident in himself, in his management of the world, and in what he thought were essential parts of him, the intelligence and virtues which made him akin to angels. To-day this great vision is broken, and he finds that there are also within him the innate infamy and stupidity of the brute—the heart of the tiger and the brain of the baboon.

One fact about the war stands out predominantly, unquestionably, and written in letters of fire upon the night of the present. Before the war there were probably not a dozen persons in the whole world who consciously and prepensely desired any war at all, and if there were twelve such persons they lived only in Germany. Many of the wise foresaw the coming struggle, and urged the world to prepare for it; but this does not mean that they desired it. In France perhaps there were some who knew better than others the ultimate designs of the German preparations; but was there a man in France who was such a fool as to wish for war for itself? We can say definitely that the whole of the British Empire, certainly, contained not a single man or woman who had ever even conceived so villainous a crime. At the same time every one capable of reasoning saw what such a war would entail; and who would not have been willing to sacrifice much of his wealth or leisure to prevent it? Yet, in spite of this attitude of the innumerable millions of humanity, a small gang of unspeakable scoundrels have been able to impose this monstrous calamity upon their fellow-men—not for any ultimate benefit of humanity, not for any cause or dogma which they rightly or wrongly considered to be true or great, but entirely for their own personal glorification. For this purpose they have torn to pieces millions of their own countrymen and of the neighbouring nations; they have left millions of mourners; they have destroyed cities and ancient buildings; they have crushed the weak; they have

torn up treaties and their plighted troth; they have been unnecessarily cruel; and perhaps worst of all they have shown mankind to be capable of the most foul and systematic lying that has ever been heard upon the earth. But this does not complete the story; for on many sides mankind has witnessed follies almost as iniquitous; neglect of the warnings of the wisest men; lack of courage and duty in imposing the necessary measures at the outset of the war—vacillation, want of forethought, want of judgment, and, we fear, preference of personal interests to those of country. In short, mankind has not only seen into his own heart, but also into the heart and the mind, or no mind, of his rulers. In both he has often found crime, falsehood, or incompetence.

How then has this monstrous thing happened? To be brief, because the whole of humanity lies to itself. We are not willing to use the reason God has given to us. We go about seeking the vain phantoms of our own imagination. We worship murderous molochs; we throw them our children; we listen to the voice of the paid teachers of untruth; we set up dogmas like idols on our hearth; and do those things which the prophets of old warned the Israelites against doing. The Lie always clothes herself in the white garments of Truth; and we mistake her for Truth because we have not the courage to test her. Worse than this, we are indifferent to Truth, even in our daily walks of life, and therefore much more so in the great affairs of the world. Our politics, our sects, our fads, are nothing but the monstrous diseases inflicted upon us by the evil spirit whom we have been worshipping and whom we have taken to be our guardian and physician. She has hideously deformed us, covered us with sores, bent us with pains; and we now die for her.

Read between these lines then. There is but one Truth—that which always holds the balance between the arguments. If we are men we must use the best faculty of man. Otherwise surely we shall return to the brute.

In a recent and admirable philosophical book [1] touching upon the war it is argued in detail that the instincts of herds of various kinds of animals still exist among nations of men to-day—that the Germans exhibit the instincts of the wolf-pack and that the British possess rather those of the hive. This is

[1] *Instincts of the Herd in Peace and War* (see "Books Received," p. 188).

true; but the argument carried to its conclusion leads further, and precisely to our point. Men are not wolves and are not bees; they are men. By this time we have acquired intelligence far superior to that of wolves and of bees—why then do we not use this intelligence for our own government? Cannot men who made the great ships that move upon the waters and in the air, the great engines that labour on land, and the great instruments that enable the mind to measure the heavens—can we not use these powers to prevent such disasters as we are now suffering from, by constructing an equally efficient engine of government? Truly we can. But only by one method. By using the calculation, judgment, forethought and invention which men of science and inventors have employed in the cases mentioned. To do this, however, we must first recognise that we are men, and neither angels nor animals. Let us cast behind us our sects, our party politics, our dogmas, and our lies, and fling our broken idols on the rubbish heap. Both England and Germany must do this, for both have worshipped false gods. If they do not do it, they shall again receive, as they are receiving to-day, the punishment which Jehovah gave to the Israelites who disobeyed His prophets—and Him.

One of our highest thinkers has suggested that civilisation waxes and wanes every fifteen hundred years or so—rises to the highest point of prosperity, and then sinks down again to the lowest depth of the opposite. And we have had immense examples before us—Greece and Rome, China, India, and certain nations of modern Europe. There is behind this some great and undiscovered law of nature. Let us beware of it. Let science try to discover it. Otherwise, it may be that this war is only a beginning, and that the noble structure which our fathers made will become in the time of our sons a jumble of ruined columns overgrown with thistles, among which the wolves howl by moonlight and there lives no great thing.

Let the point be clearly understood. There is moral crime and there is intellectual crime; and the intellectual crime is perhaps the worst of all. Those who remain ignorant when they should learn, thoughtless when they should think, and sunk in superstitions when they should reason, are defaulters before God; and this war was in the first place due to the

intellectual crimes of humanity, and is now become their punishment.

Homer, Dante, Shakespeare, and Cervantes

These great men of science were, each in his own epoch and country, the first to commence the exposition of a branch of natural knowledge which, though it is of prime importance for humanity, has not yet even received a name. The sub-science of this department of knowledge aims at collecting, classifying, and cataloguing the infinite varieties of character and circumstance found among men and in human life; the theory of the science attempts to extract from the facts an explanation of human action; and the final great synthesis endeavours to give us a logical rule of virtue and conduct. The only manner in which such a science can be taught to men is by way of narratives of events which, though they may not actually have occurred as described, are occurring over and over again in history and in our lives—just as Euclid's book was the first to crystallise geometry in sets of definite propositions with figures which are never actually found in nature. Similarly, the constructions of the men of science referred to have to be idealised, partly for brevity and partly for fixing the attention of the public; and with them, as with Euclid, this necessity demands crystallisation in the best possible form—a thing which is known by the name of art. Fundamentally however all these works are works of science, and the art is found only in their presentment.

The great histories and biographies, as well as other epics and novels, belong to the same class of work. The ultimate object of all the writers is to instruct, to warn, and to encourage—quite a different object to that of a lower type of writings distinct from this type, the object of which is merely to amuse or to impress readers with the cleverness of the author. In the result, these great books have become the prime educators of mankind, each doing more in this way than all our schoolmasters put together can do. At every issue our conduct is ruled, tacitly or admittedly, by some picture from these books remaining somewhere in our minds. We do not murder because we remember Cain and Macbeth; we do not lie because we remember Ananias and Iago; we do not wish to be stupid and greedy because we remember Sancho Panza; nor to be unduly

idealistic because we remember Don Quixote; and in the greatest of Books we have the final instance of what human conduct should be, and also the final tragedy of what virtue may come to, for ever set before us to warn us lest we think that merit may have any other reward but itself. Here in the great cathedral which is man's spirit each of these books is a chapel by itself; beautifully adorned, lit by wonderful lights from heaven, and existing for ever in a sacred silence which is never forgotten by those who have once experienced it.

At first the unthinking may not be quite willing to attach such attributes to some of these books and may imagine that they were written (like the other books mentioned above) merely to amuse. But the difference becomes clear when we search for the soul of the writer within his own words and deeds—for every scene which he presents to us is a deed of his life performed with infinite pain and labour. It is the didactic intention which makes one great difference. Consider carefully the intention behind the books of Homer. The *Iliad* is a picture of the genius of Force, set out before us to warn us of its evils and to instruct us as to how nature often compels us. In order to picture Force properly, the work begins by showing its great failing, namely unreasonable wrath—the whole world made to suffer; and then Force returns to his proper function in consequence of a noble but equally unreasonable grief. This is in fact a primordial lesson to humanity when it exists principally under the government and influence of Force. But there is more in the allegory than this, because the figure of Achilles is not only that of force but also of what we call genius; and is indeed almost the embodiment of the original god Dionysus before he took to wine. On the other hand, the *Odyssey* is a great allegory of high but normal human intelligence; and in order to form the picture Homer plunged Ulysses in difficulty after difficulty that by his wisdom, cunning, and virtues he might be able finally to emerge. This again was a prime lesson to humanity; and the Greek tragedies gave us apologue after apologue with similar high meanings.

In quite another wave of civilisation Dante wrote another parable, the function of which it was to impress something that the Greeks had not fully impressed, the sense of a more matured moral obligation. We see similar design in every one of the greater books of Shakespeare—in *Macbeth*, ambition;

in *Othello*, jealousy; in *Romeo and Juliet*, love; in *Lear*, ingratitude; in *Julius Cæsar*, political faddism; in *Antony and Cleopatra*, political profligacy; and in *Coriolanus*, pride. His *Hamlet* and *Timon* may almost be looked upon as being medical textbooks. *Timon* was probably the result of some form of enterosepsis, and *Hamlet* that of excessive introspection —both of which produce pathological results. Shakespeare's comedies and some of his histories each has a prime lesson to impart, and the greatest of them figures in Caliban the monstrous stupidity of the mass of humanity. The masterpiece of Cervantes is possibly by itself superior to any particular work of Shakespeare, just as perhaps Dr. Faustus may be considered. It states in immortal figures two extremes of the mind, idealism and realism—two qualities which are constantly in conflict, and which must yet always work together. There was never a book with such wisdom, with more humour, and with so much unutterable pathos—the extreme type of pathos which lies in the shattering of high ideals.

The degenerate criticism of the day affects to deride the intention and in its small way looks only upon the artistry of the surface; and indeed it is true that all these great works are immortal not only because of their didactic intention but also because of their art of presentment; but the latter is only the servant of the former and not its master—as the weaker critics think. For the authors knew men—that their brains can be reached only through their eyes and their ears. Why are not such works more common? Because the man who can unite the wisdom to see great truth with the capacity for constructing great art exists only a few times in a whole phase of civilisation. What a supreme art was the art of these men. In order to study it in the full we should do precisely what our schoolmasters warn us against, and that is, if possible, read the books in translation; for the artistry of the surface then becomes invisible and leaves us with the art of the spirit within. The supreme beauty of the statue lies in the pose, the form and the muscles, and not in the skin. So in these works the chief art is in that of the construction and not of the language—though seldom do we read eulogies of the former. It is not possible that either the *Iliad* or the *Odyssey* could have been written by more than one man, because it is inconceivable that two men or a committee of men could ever have made such

constructions; and it is indeed extremely unlikely that each of these works was written by a different man. The construction of the *Iliad* is the most magnificent thing in the whole range of art. Each of the three parts ends in a summit of excellence, and every character, event, episode, and metaphor is subordinated to the whole, is indispensable, and is the best one possible within the design. The construction of both works was the labour of the lifetime of a single mind of much greater capacity than the minds of all of us; and as the great tragedy draws to a conclusion we are justified in saying, what we are seldom justified in saying, that this work is divine. So also, and precisely so also, the art of Dante, Shakespeare, and Cervantes consists in the first place of perfect constructions. The language which they use, beautiful, original, or melodious as it may be, is of quite inferior importance. The real art lies in the choice of materials; and the genius, in the invention which gave the authors such an immense field to choose from. The dropped handkerchief in *Othello* is the thunderclap of the tragedy, and the knocking at the door in *Macbeth* comes like the voice of God in punishment. So also the judgments of Sancho Panza, which may appear to be little humorous incidents, really reveal the depth of the human tragedy; and poor Don Quixote, whose head is amongst the stars, falls to earth, as always will the god of Virtue-without-Wisdom.

Now in the midst of a vile war which brings no honour to any one except to those who are dying in it, we may pause for a moment to see these snowy peaks of human genius rising above the bloody turmoil around us. It would seem as if some high Power has reminded us of two of these great men in the midst of the battle and has said to us, "I have given you a beautiful world, an intellect capable of fathoming the stars, a capacity for godlike virtues, and men of genius to guide you; but all you do is to cut each others' throats in the mire."

R. R.

The Anti-Science Manifesto

The *Times* report[1] of the Neglect of Science meeting was meagre, and was immediately followed by a counterblast, in the shape of a manifesto from a small number of distinguished persons, which had evidently been secured beforehand, so that the bane

[1] May 4, 1916.

and the antidote might be administered together. In such a document, signed by men some of whom are unquestionably able and distinguished, we look for new arguments, if not for convincing arguments; but we fail to find either. I think it is regrettable and deplorable that able and distinguished men will sign, without reading it, anything put before them that purports to support a cause they have at heart. His Grace, and the noble, right reverend, right honourable, and other distinguished signatories of this manifesto have not read it: if they had read it, they could not have signed it.

For what does it say? It pleads that the present "classical" system of education should be continued lest higher education should become materialistic; but it does not say what is meant by a materialistic education; it does not say how the study of natural science encourages materialism, whatever this may be, nor that it does encourage materialism; nor does it say how the study of Latin and Greek discourages materialism. It appears therefore that the charge of encouraging materialism is merely vague, rhetorical, and abusive, introduced, not as argument, but to raise prejudice; and this is a measure that not one of the signatories would knowingly have taken. It is clear therefore that they did not read what they signed.

The manifesto warns us, and very properly, against early specialisation, but it does not show us that early specialisation is necessary to the teaching of science, and it omits to point out that the teaching of Greek language and literature to the young is early specialisation. It says that a training in [Greek] language, literature, and history gives width of view and flexibility of intellect; but every one of the signatories, who are all cultivated men, must know full well that the pedant steeped in classical learning has been for generations an object of scorn and derision as the very type and exemplar of narrowness and rigidity of mind. They would never have knowingly signed an assertion so notoriously false.

"We might enthrone physical science in all our schools without acquiring as a nation what we most need." Perhaps we might; but in the first place, no one has proposed to enthrone physical science in all our schools, or in any of them—all that has been proposed is that it shall have a position proportionate to its importance; in the second place, the experiment has never been tried, and therefore, although it is true

that it might not succeed, yet on the other hand it is equally true that it might; and in the third place, we have for many generations enthroned classics in all our schools without acquiring as a nation what we most need. Is this a good reason why we should be forbidden to touch the sacred structure, forbidden to try the effect of adding a new method to the old? Certainly the signatories, able men all of them, would never have put their names to such a slipshod travesty of reasoning if they had known what the document contained.

That the scientific method is no longer confined in application to natural science, and that all good work in all studies is based upon this method, I should heartily agree; but why is it called the scientific method? It is so called because it originated in the study of natural science; because it is the backbone, the very life, of natural science; because natural science cannot be profitably studied but by its means; and because it is in connection with natural science, its source and its home, that the method is most easily, most naturally, and most appropriately taught. The signatories of the manifesto are not only able men, but also honest men, and they would never knowingly make themselves parties to a disingenuous attempt to filch from natural science the credit of originating the scientific method, and to transfer the credit to the classics. They cannot, therefore, have read the manifesto they signed.

What we want, the signatories of the manifesto are made by it to say, is scientific method in all branches of an education which will develop human faculty and the power of thinking clearly to the highest possible degree; and again I am happy to express my concurrence. They "believe that in this education the study of Greece and Rome must always have a large part." Why? Because it will develop human faculty and the power of thinking clearly to the highest possible degree? Oh dear no! "Because our whole civilisation is rooted in the history of these peoples, and without knowledge of them cannot be properly understood." In order to develop the power of thinking to the highest possible degree we are enjoined to do that which it is not pretended will conduce to this result in any degree. What it will do is not to develop the power of thinking, but to enable us to understand our civilisation, which may be a very desirable thing, but is not the same thing, nor is it among

the objects that the manifesto says ought to be the objects of education. However, let us take it that the object of education is not solely to develop the faculty of thinking to the highest possible degree, an accomplishment that some of us, in spite of a classical education pushed to the highest possible degree, appear somehow to have missed, but is also to enable us to understand our civilisation ; and let us further agree with the manifesto that in order to do this we must study the history of Greece and Rome, because our whole civilisation is rooted in the history of these peoples, and without knowledge of them cannot be properly understood. Let us allow all this ; but then for the same reason it is equally necessary that we should study the history of the Village Community, of the Tribal Assembly, of the Manor, of the Roman Catholic Church, and of the Feudal System ; for in them also our civilisation is rooted, and rooted more directly and intimately, and without knowledge of them cannot be properly understood. Yet the manifesto does not advocate these studies. It is clear, therefore, that the signatories never read the manifesto, for a very moderate development of the faculty of thinking clearly, a development far less in degree than any of these able champions of classical education possesses, would have detected such a notable confusion of thought.

Lastly, if they did read it, we are forced to adopt a conclusion from which the minds revolts. "In the literature of Greece," says the manifesto, "we find models of thought and expression." We do indeed. We find models of thought and expression so excruciatingly bad that though their meaning has been debated for more than two thousand years, it is still wrapt in the densest obscurity. Of what can be understood, we find a few masterpieces, comparable with the many masterpieces of modern times, but by no means surpassing them, or even equal to them. By universal consent, Homer is placed in the very first rank of Greek poets, and is usually regarded as the supreme Greek poet ; yet Dryden, who was himself not only a great poet, but also a great critic, who certainly had no bias in favour of natural science, and in fact had no more interest in it or knowledge of it than some of the signatories of the manifesto in the *Times*—Dryden declared that it would need the addition of all Dante's greatness to raise Homer to the level of Milton. Apart from what is unintelligible, and

apart from a few masterpieces, whose excellence, though it has been much exaggerated, may be admitted, what do we find in Greek literature? We find much rambling and silly speculation; we find much that refers to local and temporary affairs, and is no longer of interest, except incidentally to the historian; we find much that is childish, much that is false, and much that is grossly immoral. If the signatories of this manifesto had indeed read it before they signed it, what should we say? I know what I should say. I should say "My Lords Cromer, Bryce, Curzon of Kedleston, and Esher, did not clear thinking, developed to a very moderate degree, warn you that you were giving occasion for a very serious *scandalum magnatum*? Sir Edward Fry, how many times have you not heard your own officer in your own Court read the Queen's Proclamation against Immorality! My Lord Archbishop of Canterbury and my Lord Bishop of Oxford, fie upon you!"

CHAS. A. MERCIER.

"Science Progress"

The tenth volume of SCIENCE PROGRESS was concluded with the previous number, and this number commences the eleventh year of its existence. We have decided to inaugurate this event by a small change of title, and are obliged to many of our most valued contributors for having made suggestions on this subject. As will be seen, the result has been that no great change was ultimately fixed upon. At the same time Mr. Murray, to whose continual support and valuable suggestions SCIENCE PROGRESS has been indebted from the first, has kindly given the short statement upon the history of this quarterly which immediately follows; and we take the opportunity also to thank the numerous men of science who have so generously assisted us with their writings and their advice. With their help, SCIENCE PROGRESS has now reached the maturity of its development; and will, we trust, continue to fulfil its functions —and its duty.

The Origin of "Science Progress" (Mr. Murray)

The rapid advance of scientific research and discovery renders it very difficult for those who are engaged in any special study of the kind to keep in touch with what is being done in

other kindred and collateral departments. Each branch has its own special organ in the press, but the need of some periodical which should as far as possible co-ordinate results and make them easily accessible was strongly felt.

With a view to carrying out the purpose SCIENCE PROGRESS was first established in 1906. It originated among some of the leading teachers and students of the London University. Their aim was to provide articles—written, as far as possible, in non-technical language, so as to be readily followed by all well-educated men, and at the same time to enable scientific men engaged in one branch of science to appreciate the full significance of the progress being made in other branches.

The first editors were Mr. N. H. Alcock, M.D., and Mr. W. G. Freeman, B.Sc., F.L.S., but in course of time the calls of professional duty away from London compelled them to abandon their editorial work, which was for a time very kindly undertaken by Prof. H. E. Armstrong, Ph.D., F.R.S., LL.D., who in July 1913 gave place to the present editor.

I should like to take the present opportunity to request readers of SCIENCE PROGRESS to send any comments they may have to make on its contents, or any suggestions for subjects for treatment, directly to the Editor, by whom all such communications will be gratefully received.

The Advancement of Science

The British Science Guild has been occupied for some time in producing a comprehensive Memorandum upon the steps which it thinks ought to be taken for advancing science in Britain and for improving the whole position of science as regards education and research in this country. Unfortunately the Memorandum cannot be printed in this issue of SCIENCE PROGRESS, but we understand that it will shortly be circulated and acted upon.

Bacterised Peat

Prof. W. B. Bottomley's reply to the letters upon this subject which we published in our April number has reached us too late for insertion in this one, but we hope to issue it in October. The suggestion that our food supply might be doubled by the use of bacterised peat was made at his lecture by another speaker and not by him (*Times*, October 19, 1915).

The Neglect-of-Science Meeting

The Committee which published in the Press of February 2 a memorandum on the teaching of science in the schools, especially with reference to the Civil Service examinations (see SCIENCE PROGRESS, April, page 666), held a meeting to discuss the subject at the Linnean Society on Wednesday, May 3, 1916. Lord Rayleigh was in the chair, and the room was too small for the large number of people who attended. The memorandum had already been accepted, not only by a number of distinguished men, but also by a number of public bodies, including University Colleges, schools, the Royal College of Physicians, the British Association, the British Science Guild, the Royal Society of Edinburgh, the Iron and Steel Institute, the Society of Engineers, and many other societies and institutions; and the meeting was unanimously in favour of the proposals of the Committee. The full report of the proceedings at the meeting has just been published by Messrs. Harrison & Sons, St. Martin's Lane, London, price 6*d.*; but unfortunately it comes too late for us to give an abstract of the speeches. We would, however, strongly recommend every scientific man to get a copy of the paper. All the speeches were excellent. Sir Ray Lankester showed good strategy in confining the proposals mainly to a single point, namely the insistence on more natural science in the competitive examinations for the Civil Service. Sir Edward Schäfer followed with an excellent summary of the position of scientific men in the matter, and was strongly supported by the Poet Laureate, the Hon. F. Huth Jackson, Lord Montagu of Beaulieu, the Earl of Portsmouth, Mr. H. G. Wells, Sir Harry Johnston, Prof. Poulton, Prof. Turner, and a number of other distinguished gentlemen. All the resolutions were carried, and the next step will be to take the required action suggested by the meeting. We regret that it is impossible to give further details in this issue. We may, however, add that the resolutions were accepted by no means only by men of science, but also by many distinguished teachers and headmasters. The reason why special stress was laid upon the Civil Service examinations was that the whole course of teaching in the schools and universities is set to the tune of those examinations, and if a reform is made in them, reform in the schools must follow as a matter of course. We subjoin the resolutions themselves:

(1) That in the opinion of this meeting it is a matter of urgency, in order to promote national efficiency in the near future, that the natural sciences should be made an integral part of the educational course in all the great schools of this country, and should form part of the entrance examination of the Universities of Oxford and Cambridge as well as of the newer universities.

(2) That it is in the highest degree desirable that the Government should exercise the large power which it possesses of encouraging the study of the natural sciences and thereby increasing the efficiency of our public servants (1) by assigning capital importance to the natural sciences in the competitive examinations for the Home and Indian Civil Service; (2) by requiring some knowledge of the natural sciences from all candidates for admission to Sandhurst.

(3) That this meeting is of the opinion that the method indicated in Resolution 2 is the only one by which it is practicable to bring about the desired change in the attitude of the schools and colleges throughout the country towards the natural sciences and to make some knowledge and understanding of those sciences general. As the results of such changes will only develop in the course of years, it is urgent that the matter should be at once taken in hand by His Majesty's Government.

(4) That the Committee are authorised to take such steps as they may consider appropriate in order to bring these views to the notice of His Majesty's Government.

In *Nature* of June 8 there also appeared a strong letter asking parents of boys at public schools to support the signatories in similar efforts by communicating with Sir Mark Collet, Bt., Kensing, Kent. The signatories of the letter were Lords Avebury, Desborough, Claud J. Hamilton, Sir John Jellicoe, and others.

Science and the State

One of the best letters that have recently appeared upon this subject is written by Sir Napier Shaw, F.R.S., in *Nature* of May 11. We have often talked about the workers in the trenches of Science, and it is interesting to note that this very phrase was used by Sir David Brewster sixty-one years ago. "The project which Brewster favoured was State support for men of science on the lines of the French Academy, and to the lack of such support Brewster attributed the neglect of the Newtonian philosophy in England, while it was being successfully developed in France by Laplace, d'Alembert, Clairaut, and others. . . . The exponents of science in this country have allowed the issues of the inevitable conflict of studies in science to be dictated everywhere from the examination point of view. That calamity—for it is nothing short of it—is more largely responsible for the apathy of the State towards science than is generally acknowledged. So far has our control by examination extended that it is not too much to say that, for the general, our education has become the art of passing examinations without having to think, and the educational profession is, in practice, the only human occupation for which a general education is not required." This final epigram is a dismissal with a kick indeed!

Another excellent paper bearing on the subject is by Prof. F. G. Donnan, F.R.S., in the *School World* of May last. He says: "Science is, therefore, no less than an emancipation of the spirit from the thraldom of ignorance, obtained through the laborious investigation and direction of the conditions and circumstances of its environment. Science as thus defined may be, it is true, only a part of civilisation. There must, indeed, exist an inner light, an inner progressive unfolding of the spirit, whence come the highest aspirations of religion, art, and literature. Such questions are as old as philosophy itself. But however that may be, the important thing to perceive is that there can be no antagonism and there must be no divorce. This is the dominant—the solemn—note that peals throughout the world to-day as it pealed in the days of Galileo and Da Vinci. Science as thus rightly understood must be the heritage of every man, and not the cult of any special sect."

The fatuity of the Latin-grammar educationalists appears to lie in believing that their branch of knowledge is the only one of any consequence. On the other hand, we who demand more science in education never go beyond urging that science shall be a part of the curriculum, and only a part. Our opponents put into our mouths things that we never said and never dreamed of saying. We maintain that, as Prof. Donnan says, science must be the heritage of every man, and we think so because we are quite convinced, from our experience of the world, that a man is not complete without some knowledge of natural phenomena and the explanations of them which have been gathered by mankind during the last two thousand years or more.

Experts and the Colonial Office

On March 12, 1915, the British Science Guild addressed a circular letter to Government Departments and municipal and other public authorities, asking

whether "it would not be proper and advisable" for all such bodies "to make it their invariable rule and practice to pay scientific experts of all kinds for assistance rendered by them, either at committees, or by letter, or in any other way, such payment to include, not only refund for travelling expenses, or other out-of-pocket expenses, or maintenance, but also a proper fee for the professional assistance rendered." There is only one right, and one wrong, in the world; and obviously such payment is the proper and honourable thing for all public bodies to give. Nearly all the Government Departments and other bodies accepted the suggestion more or less definitely, and only the Colonial Office and the London County Council refused to do so. With regard to the former, Mr. Harcourt said that the arrangements already in force in his office "have been found to work well in the past, and Mr. Harcourt sees no sufficient grounds for modifying them." The business was now interrupted by the war; but a little while ago a medical member of the Guild (whose name need not be mentioned here) resigned, in consequence of Mr. Harcourt's reply, the membership of two unpaid committees of the Colonial Office, and pointed out that he, at all events, did not consider Mr. Harcourt's arrangements to be satisfactory. Following upon this, on May 23, Mr. W. H. Cowan, M.P., asked the Secretary of State for the Colonies whether he would appoint a committee to consider and report upon the proposals of the British Science Guild. To this question, Mr. Bonar Law (now Colonial Secretary) replied, "I agree with my predecessor in thinking that there is no sufficient ground for modifying existing arrangements," and refused to appoint the committee. Upon this, the medical member of the Guild resigned his membership of a third Colonial Office committee of which he was a member.

We hope that all scientific men will take note of this occurrence. The meaning of it is that the Colonial Office intends to continue acquiring expert advice for nothing, and really places itself in the position of a wealthy patient who makes a practice of visiting the consulting rooms of doctors and of omitting to pay their fees. There is not only an indescribable meanness in this attitude, but an equally indescribable want of wisdom. Originally, the Colonial Office may have pleaded that its attention was not drawn to the impropriety; but after the action of the Guild, of Mr. Cowan, and of the medical man referred to, this plea cannot be advanced, and the Office has therefore put itself in a very false position.

It should be added here that although the British colonies have benefited immensely by the researches and practical efforts of a few private medical men, neither the colonies nor the Colonial Office have ever made the smallest effort to pay properly for the professional benefits received from these workers;[1] and now Mr. Bonar Law attempts to dispose of all these obligations in a single curt reply which, in Sir Ray Lankester's phrase, is both "contemptuous and contemptible." Of course the matter has nothing to do with the war.

Maleducation and Malpronunciation

One would think that, after our excellent schoolmasters have been teaching us Latin and Greek for some centuries, we should by now possess some elementary knowledge of what these languages probably sounded like when they were spoken. Of course as the ancients published no works on phonetics, we cannot be sure of their pronunciation; but we may at least infer with a very high degree of probability that Latin and ancient Greek sounded more like modern Italian and

[1] See SCIENCE PROGRESS, January 1914. Also this issue, p. 179.

modern Greek than like Cockney English. For example, the vowels have much the same sounds in all the romance languages but quite different sounds in modern English. Reforms are now certainly being made, but we still hear *dominus* pronounced with an English *u* at the end and *amo*, *amas*, *amat* given an appalling pronunciation. Yet our public schools and universities even award high marks for writing verse in these languages. What are we to think of the following doggerel which makes *patria* rhyme with *away*?

> Qui procul hinc, the legend's writ—
> The frontier-grave is far away—
> Qui ante diem periit:
> Sed miles, sed pro patria.

Shelley, who certainly ought to have known better, makes *Urania* rhyme with *lay*, and our diphthongs are impossible. We once heard a Greek play at a university, but our pleasure in it was spoilt by the shouts of laughter indulged in by some Greeks who were sitting behind us. The Greek of Oxford-atte-Bowe appears to belong to that exclusive university; for when a modern Greek reads a bit of Homer one can really quite appreciate the rhythm! But if this matter is mentioned to a classical scholar he shudders with horror and demonstrates (what the modern Greeks deny) that the old accentuation was entirely wrong. However we had better not plunge any further into this thorny wood.

It is time also to make a strong protest against the pronunciation of English blank verse on the stage. Evidently in the age of Marlowe and Shakespeare, the rhythm was given its full force, with the swing intended by the authors; but our actors love to jumble all the words just as people do in ordinary conversation round a tea-table—so that one does not know whether the character is intended to be talking verse or not. It reminds us of the clown's remark after a pathetic piece of music, "Say, are you singing a song or lodging a complaint." Probably poor Poetry is as dead as mutton in the England of to-day, and for the same reason that poor Science lives in the kitchen like Cinderella—while their prosperous sisters, Maleducation and Impolitics, go to all the balls and enjoy a place in the sun.

Trade after the War

It would be well if many or all economical affairs could be treated as open scientific questions instead of political ones as they are now treated; and the question of trade with Germany after the war is a case in point. Should we or should we not build up tariffs against her? Speaking on March 30 on behalf of Messrs. Lever Bros, Sir William Lever, whose immense experience (not only in commerce but also in art) always commands the closest attention, thinks not. He said, "The question arose as to what should be their national policy when the war was over in order to reap the full advantage of victory that would surely be ours in the field. Some are nervous that we shall lose in commerce the advantage that our brave soldiers had won in the field. What they all desired was to reap fully and completely for our Allies and ourselves the full reward of victory. They were all agreed upon this, but differed vastly in the methods by which they thought they would best achieve this. . . . The policy of tariffs and restrictions could only stimulate a false Imperialism, founded upon injustice and disregard of the rights of others. Then we were to insist upon Germany paying huge indemnities to Belgium, France, Russia, and Serbia. There was not cash in all the world to pay these indemnities. Germany could only pay in goods, and pay she must.

She would want raw materials, and for these she must come to the British Empire, every nook and corner of which would be stimulated. If we refused this trade after the war, or raised special tariffs, the trade would go to neutral countries—Scandinavia and the United States. This would deal a blow at our shipping by stimulating neutral and enemy shipping. Next to our Army and Navy, we shall owe victory in this war to our mercantile marine, and the adoption of tariffs, in addition to being a cowardly policy, would strike a blow at our shipping interests, which were our vitals."

The Shakespeare Tercentenary

When the Royal Society held its meetings in 1912 to celebrate its 250th anniversary, several distinguished foreigners who were present remarked upon the fact that the principal speakers at most of the functions were not the distinguished men of science of Britain, but the politicians, the lawyers, and the parsons. Apparently mere persons who affect science or literature and do not belong to these denominations are scarcely worthy of hearing, even when their own subjects are under discussion or commemoration. So also in connection with the great Tercentenary of Shakespeare held on the 1st of May. Some of us were present at a meeting at the Mansion House at which the Lord Mayor presided, and presided well. We have no fault to find with the conduct of the meeting, except that there was scarcely enough enthusiasm to impress the foreigners who had come to England in order to do honour to one of the greatest of Englishmen; but here again we noted the same defect. One would naturally have expected that our competent Poet Laureate would have given the principal address, and that he would have been followed by all our most distinguished poets and writers. But, as usual, the addresses were delivered by party politicians, an archbishop, colonial politicians, and other gentlemen of the same category. In this case, however, the principal politician was certainly also a poet of some distinction, but he only took the place which should have been occupied by another politician who is nothing but a lawyer and, many think, about as poor a statesman as ever lived. The literary foreigners who attended were allowed to say a few words towards the end of the meeting; and the only literary contribution was the speech of Prof. Gollancz in which he handed over his *Book of Homage* to the Lord Mayor—this book consisting of writings by a large number of eminent men, extracts from which he read. So the world goes. What would Shakespeare have said? It rather reminded one of the play so admirably performed at the end of the *Midsummer Night's Dream*, and Bottom the Weaver, who could do everything, was evidently on the spot! This is the net result of our system of education which, although Greek is taught more than science, merely leads to politics and dogma, and pushes science, literature, and learning together into the background. Many people think that if Britain wishes to hold its place in the world (which it is now losing) it must change its attitude towards the very much higher forms of intellectual life. At any rate, let us make a change when we ask foreigners to attend our commemorations. Otherwise we become merely contemptible in the eyes of the outer world.

On the other hand, it is with the warmest feelings that we welcome the honour which the King has given to Sir Frank Benson—the actor who by his genius and energy has done more than any one else to keep the works of our great poet before the mass of people in Britain. For his ideal he has given up much, including a larger popularity which he might easily have won by the performance

of a lower drama. As it is, the education which he has given to the people, and especially to our children, has probably exceeded in extent that of the combined efforts of thousands of our politicians, lawyers, and schoolmasters.

The "Athenæum" Subject Index

In the April number of SCIENCE PROGRESS the scope of this publication was outlined and a review given of one number. Part VIII, issued March 1916, deals with the subjects of Theology and Philosophy. Its entries, drawn from 140 journals, cover all forms of religion, even the most primitive being included. Students of Divinity and Philosophy, and even Church workers, will find such a reference of great value.

"Scientia"

The February number of this Italian periodical publishes five articles in English and French in addition to its own language, German being now conspicuous by its absence. The English article, "Iranian Migrations before History," by Mrs. Walter Maunder, tries to prove by the internal meteorological evidence of the ancient Persian writings that the Iranians must have had two great migrations: firstly, their migration southward from within the Arctic Circle, and secondly, at a much later date, from the Panjab. The contribution of M. Charles Gide, of the Paris Sorbonne, entitled "Les Dépenses de la Guerre et leurs Conséquences Économiques," is quite in accordance with the prevailing interests of the moment. He shows how useless it is to predict that the war must soon come to an end because of the present financial strain on the Allies for the simple reason that this strain has not yet begun to be felt, the circulation of paper money having rendered payments fictitious. Few individuals, he says, have as yet felt any pinch at all, with the exception of the English, to whom he pays a warm tribute of praise for sacrificing so much in the way of direct taxation. He estimates that the waste of production caused by the war is certainly not greater than that caused in peace times by the production of luxuries. "To sum up," he says, "one might define the war, speaking economically, as a manufacture of luxury, the only luxury at this present time allowed to the belligerents." He finally proceeds to outline various methods by which France can recover financially after the war. Other articles are contributed by Th. Moreux, F. Bottazzi, and A. Loria.

"The Scientific Australian"

The March number is of more general interest than previous publications. The first article contains a description of the remodelling of the Zoological Gardens at Sydney, which seems to be on the lines of Hagenbeck's Zoo at Hamburg. Modern military needs are catered for in two articles: "Optical Military Instruments," by H. H. Baker, F.S.M.C., No. 1, Range Finders; and "Modern Munitions of War," No. 1, Guns and Propellants; while there is in addition much interesting readihg. The introduction of a table of contents might be serviceable.

The British Immigration League of Australia

The Tenth Annual Report of this League for the year 1915 gives a complete résumé of its objects and makes a strong appeal to its own countrymen to wake up to the fact of Australia's dire need of immigrants. It points out that although the fringe of towns on the coast are overpopulated, labour is required on the waste

land of the country, which can never be opened up unless British labour of a desirable quality is introduced. It says, "A patchwork system of State-assisted immigration has not succeeded in the past: it cannot possibly meet with success the much greater demands of the future. It is a matter in regard to which the eccentric activities of the irresponsible politician should be as far as possible restricted. A certain percentage of the immigrants to all States during the past years has been made up of undesirables. The scheme ahead must provide for the exclusion of undesirables. We need for the population of Australia men of our own race and our own civilisation, our own traditions and our own points of view."

"Les Annales des Nationalités"

This periodical discusses from every point of view the questions involved in the relationship of one nation to another, and deals largely with the smaller countries that have been devastated by the war. It includes articles on "France and the Oppressed Nationalities," by C. R.; "A Fault of the Allies," by Georges Bienaimé, in which he displays with much sarcasm the great discrepancy between the boasts of some of the Allies that they are championing the cause of the weak and the actual treatment meted out to such when they come within their power; and "Conference of the Nations and the Work of its Permanent Commission," by J. Gabrys, which is to be continued in following numbers. It also sets out in brief, in a section entitled Chronicle of the Nationalities, the aims and projects of Bohemia, Bulgaria, Lithuania, Poland, and Serbia; but the article on Serbia consists chiefly of a tribute to her heroism.

"The Southern Slav Bulletin"

The Jugoslavs, by publishing this little English pamphlet, are endeavouring to keep their aims before the mind of the British public, aims which they hope will be realised for them in the final terms of peace, on the supposition, of course, that victory will perch on the banners of the Allies. They look forward to the time when they can gather under their own rule all of their brethren now condemned to live under the sovereignty of enemy countries. Amongst other matter the number dated May 4 contains articles on the Crown Prince Alexander's visit to London, the Jugoslav National Holidays, and the Cause of Serbia.

ESSAYS

Examination by the Civil Service Commissioners for an Appointment in the Imperial Service

We have been favoured by Dr. Charles Mercier with the following, which purports to be a shorthand note of an interview with the Civil Service Commissioners. We are bound to say that the only evidence of its authenticity that Dr. Mercier gives is the internal evidence, but it will be seen that this is quite conclusive.

Examiner: Mr. Jones, the post at the disposal of the Secretary of State is that of Administrator of a very extensive district, much of which is still unexplored, in tropical Africa. Will you tell the Examiners what qualifications for the post you possess?

Candidate: I have some knowledge of geology, sufficient to judge in what formations coal, metallic ores, mineral oil, and other important minerals are likely to be found. I can recognise by their appearance the chief metallic ores, can test them for the metals they may contain, and make a rough assay of the quantity. I know the climate and soil suitable for the cultivation of cereals, maize, hemp, cotton, rubber, tea, coffee, camphor, dates, and different kinds of timber. I know something of the breeds of the horse, ox, sheep, pig, goat, and camel, and what breeds are suitable for different purposes and different localities. I am acquainted with the principles of irrigation, and know for what land it is suitable, and for what land it is practicable. In a suitable locality I can show how hundreds of thousands of acres of desert can be converted into a fertile corn-field. I know the causes of most tropical diseases, the precautions to be taken against them, and the mode of treating them; in fact, I have taken a degree in tropical medicine. I have learnt the language chiefly spoken in this district, and can converse in it fluently. I know the social organisation of the people, their customs, their system of land tenure, their modes of cultivation, and their staple products and industries. I have studied——

Examiner: That will do, Mr. Jones. To put it briefly, you have gone in for stinks. I may tell you at once that such an account as you have given of yourself would have completely and finally disqualified you for any appointment made by this Office before the war. In the present state of affairs we have to put up with what we can get; but even now, the Secretary of State is determined that this post shall be given to no one but a highly educated man. What evidence can you give that you have had a liberal education?

Candidate (astonished): Well, really, sir, I——

Examiner: Surely, Mr. Jones, you understand me. I ask you if you have had a liberal education. In other words, can you read Greek? If you were to receive this appointment it is very unlikely that you would have any spare time; but the Secretary of State must be assured that if you had any spare time, you would be able to occupy it in reading Plato in the original Greek. If you like to fritter away your time with cultivating cotton or rubber, or breeding camels, or studying tropical diseases, I dare say the Office will not interfere, unless, indeed, your studies are likely to prove useful; but the consideration that will decide the elec-

tion is whether, if you had any spare time, you could fill it by reading Plato in the original Greek.

Candidate: Really, sir, I am not interested in philosophy, and if I had any spare time, I should not spend it in reading philosophy, either in Greek or English.

Examiner: Mr. Jones, you are trifling with the Examiners. Of course you would not read Greek. No one reads Greek, except to prepare himself or some one else for examination. You were not asked whether you would read Greek: you were asked whether you would be able to read some difficult Greek writer, such as Plato, in case you had the leisure, and wished to read him.

Candidate: I might be able to spell it out with the help of a dictionary, but I should not understand it.

Examiner: Is it possible, Mr. Jones, that you are so ignorant as to suppose that any one can understand Plato? Do you not know that he has been studied for more than two thousand years, and that his meaning is more hotly debated than ever? I am afraid it will be quite impossible for us to recommend the appointment of a candidate who is so completely uneducated. Pray, Mr. Jones, have you any other qualification to put forward for the consideration of the Examiners?

Candidate: I am afraid not, sir. I wish now that I had spent on Greek the time I devoted to football.

Examiner: Football, Mr. Jones? Did you say football? Had you any success at football?

Candidate: I won my blue, sir, and played for the 'Varsity.

Examiner: Dear me, Mr. Jones, why did you not say so before? This is most important. We had no idea—really, Mr. Jones, modesty is very becoming in a young man, at any rate before he enters the Service, but even then it may be pushed too far. We shall have much pleasure in submitting your name to the Secretary of State for this important post, and we wish you every success in your career, which we are sure will be most distinguished. But do cultivate the ability to read Plato. You need never read him, you know. No one reads him who is not compelled to do so; but unless you could read him if you wished to, there is something wanting. We never consider a man who cannot read Greek quite—quite—you know what I mean. No one wants you to read Greek, and no one in his senses, except professors and people of that kind, ever does read Greek; but if you *can* read Greek, it gives you that air of superciliousness which is so highly valued in the Civil Service, and is, in fact, the stamp of the higher ranks of the Service. We never *say* that a man who cannot read Greek cannot be a gentleman, but every one will admit that the man who can is intellectually, morally, and above all, socially, superior to the man who cannot. And take my advice, and give up hunting after metals, and breeding sheep and goats. It may never be useful, but there is a taint of utility about it that the Office does not like. If you must breed something, breed hyænas, or crocodiles, or some other good sporting animals; but if you want to get on, beware of utility.

Candidate (*sotto voce, as he retires*): Well, I'm damned!

Chalk Flints and the Age of the Earth

Standing by the new Lighthouse at Beachy Head, in front of the grand cliff section there exposed, even the casual and ungeological observer must be struck by the curious black lines in the chalk. These are the well-known flints of the Upper Chalk. We notice how they occur here in more or less parallel lines,

following the stratification of the chalk. Also that they are some three or four feet apart. They present a geological problem of considerable difficulty. So far as the writer is aware no theory which goes into precise details as to the actual method of the formation of these *lines of flints* has yet been brought forward. Those who have written on the subject have confined themselves to generalities. They have suggested the growth in the Cretaceous ocean of crops of sponges, and other silicious organisms. To account for their repetition at approximately regular intervals, no better suggestion has been made than that of Prof. Owen long ago. According to this view they are the remains of *successive* crops of sponges which grew again and again according to *some periodic law*. Assuming this—and it seems quite the most probable explanation yet offered—we have to ask, What was the length of the period, and what was the cause of the periodicity? First as to the length of the period between the successive crops of sponges. Prof. Sollas, in his estimation of the age of the earth, takes one foot in 100 years as the average rate of rock formation. But chalk is generally held to be a rock of much slower formation than many others. One geologist, indeed, suggests one foot in 1,000 years as the rate at which it was accumulated. Let us, then, take 6 in. of chalk in a century as a rough compromise. Since the remains of the successive crops, our lines of flints, are some three or four feet apart we have an interval of from 600 to 800 years between each growth. A crop of sponges, then, flourishes for a certain time, dies, and is covered up with chalk. An interval of 600 years, or more, elapses, and a similar crop appears over the same area of the ocean. There may be even the *same species* of sponge in the new layer. But whether they are the same, or new species, the difficulty is about the same. For 600 years there have been no sponges in this particular area of the chalk ocean. Where have the reproductive spores for the new crop come from? Not from the previous one. They could not have been floating about in the ocean all those centuries. The only possible suggestion is that they migrated away from the region, propagated themselves elsewhere for 600 years, and then floated back in the form of reproductive spores to their former home. These settling down produced the new crop of sponges, the succeeding line of flints. To be driven to such a suggestion is almost to confess a *reductio ad absurdum*. There seems to be absolutely no reason why the sponges should *leave* the area, and equally none for their *coming back*. And species of sponge coming back in that way would naturally do so gradually. They would not thus form a layer of flint keeping more or less to the same level in the chalk. At the periphery, where they entered the area, they would be *lower* in the chalk, and gradually rise. The flint layer would also be thicker in one direction, and thin out gradually.

And what about the cause of the periodicity? Nature knows no periods of 600 years' duration. She celebrates no jubilee, as it were, nor does anything different from what she has been doing, at the end of such a time. To assume such periods is not geology, which is tied down to explain the past by the present. The *only periods* observed by nature effecting rock formation would appear to be *annual*. And such periods it must be supposed *would* leave their mark in the making of rocks. Thus the deposits of summer may differ from those of winter, or there may be seasonal pauses in sedimentation, which will leave their mark. It may be suggested that these marks of periodicity may often be read in the rocks themselves. If, then, the lines of flint in the chalk *are* periodical growths, must not the periods be *years*? Must not each line of flint with its covering of three or four feet of chalk be the product of one year? A growth of sponges might take place during the summer months. As winter approached numerous

free-swimming reproductive spores might be liberated, and the old sponges dying might be covered up. In the spring the reproductive bodies might settle down to produce a new crop. There is, at any rate, nothing impossible in such a course of events. But if so, we get a rate of rock formation of 300 or 400 feet in 100 years instead of one foot. Prof. Sollas's modest 26,000,000 years for earth history is reduced to 86,666 or 65,000. To most geologists, perhaps, this will appear as great a *reductio ad absurdum* as the one indicated above. Yet it seems, on the *assumption* of periodic growths, the only possible solution. And after all, there is no certainty either in Prof. Sollas's 26,000,000, or in the 100,000,000 arrived at by other lines of investigation. These figures—and, indeed, much larger—*are required* by evolutionists, but they have not been *proved* by geology.

And the case of the chalk flints does not stand alone in suggesting a quicker rate of rock formation than one foot in 100 years. Without here bringing forward details, it may be stated that every rock specially studied by the writer from this point of view seems to indicate a rate of formation *much greater* than one foot in 100 years. But the chief point it is wished to emphasise is, that every rock may be made to tell us something—quite roughly, of course, in many cases—of the time it took in the making, if carefully studied. And further, that in not a few cases the actual thickness formed in a year is made clear in the structure of the rock itself.

A reconsideration of the one foot per century measure is required.

Since writing the above my attention has been kindly called by Prof. Grenville A. J. Cole to a suggestion of his own on the subject of the origin of flint. "The rhythmic deposition of flint," he says, "may be due to some action in a limestone mass in which the silica was at first uniformly diffused."

Prof. Cole also refers to the work of Liesegang on Geological Diffusion, on which his opinion was founded. And he further points out that Liesegang himself suggested an application of his experimental results to the explanation of chalk flints. "Liesegang," says Prof. Cole, "compares the layers of flint with the zones of regular deposition that occur in cases of diffusion of one substance through another, and suggests that the silica in the chalk was formerly diffused fairly evenly, and that a progressive one-sided precipitation then took place."

The suggestion is an interesting one, and has a very important bearing on the question of the origin of chalk flints. I have not seen Liesegang's paper, nor do I know the details of Prof. Cole's suggestion, and so am not aware whether they consider this segregation of diffused silica to be sufficient in itself, *without the periodic growth of sponges*, to account for the facts. This is the important point as regards my original note. If it is put forward as the *complete* explanation of the occurrence of the flints in more or less regular layers I am afraid I cannot accept it. If it is to be considered as a *supplementary* agency I think it is *absolutely necessary*. So far as my chemical and physical knowledge carries me, it appears to me necessary to have some definite starting-points for the segregation of the diffused silica. A growth of silicious sponges would supply this. The silica of the sponge mass might start the process, and thus determine the "local habitation" of the flints. But it would require a considerable amount of diffused silica deposited on the sponges to produce a line of flints. Here, then, the suggestion of Liesegang and Prof. Cole would play its part.

And there appears to be evidence that *animal matter* may start the segregation. For in the chalk there are frequently found the tests of sea-urchins and bivalve shells converted into solid masses of flint.

I make these remarks with the reservation that there *may be* in Liesegang's work, and in Prof. Cole's suggestion, details of which I am not aware, and which might render them a more complete explanation of the *lines of flint* than they appear to be.

G. W. BULMAN, M.A., B.Sc.

The Ego-Cell—a Speculation

Unless advanced by experts who know all that is to be known in connection with their subjects, speculations in science are apt to be very useless, if not grotesque; and I mention the following one with the more diffidence since, on describing it to two eminent specialists on Nerve-Physiology, they both said that it is not altogether new and that they did not think it likely to be sound. Nevertheless, even the speculations of amateurs have sometimes led to advance, or at least have assisted advance by others; and, at the worst, may amuse readers!

The speculation is that the judgment, will, or mechanism for deciding conduct in living things which possess volition is likely to reside in a single cell—in the brain or principal nerve centre. There appear to me to be two lines of argument which converge towards this hypothesis. The first has always been borne in upon me when studying motile protozoa or discrete cells, such as the leucocytes of the blood. These objects, though they consist of a single cell, possess in themselves the rudiments of many of the capacities found more developed in the Complex Animals or Metozoa. An infusorian or flagellate, swimming rapidly about in water, is always turning from side to side, just as a dog does when running in a field. The little cell appears to have a mind to make up; and its movements are not absolutely at random. An amœba often progresses in the same direction, and when it meets with an obstacle tries either to burrow beneath it or to get round it. Animals of a little higher development, consisting of only a few cells clustered together, show the same power of decision or will. Hence I am inclined to infer that judgment and will—that is, what appears to be the principal constituent of the Ego itself—is capable of being developed to some extent in a single cell.

As regards the other line of reasoning referred to above, we may infer from introspective psychology that the Will or Judgment is an extremely unified power. At every moment of our life something decides instantly what is to be done during the next moment. The deciding power does not appear to be distributed through a large number of entities but to reside in a single one. I mean that there is the same difference here as that between the decision of a general in the field and a war council, or between the director of any enterprise and a committee. We all know, often by painful experience, that while a director can act with immediate promptitude, a council may require months of deliberation before acting. Personally, therefore, I have a feeling (let us say fancy, if we please) that my will and judgment are the results of autocratic and not of democratic decisions. For example, suppose that I have come to a cross-road and have to decide which turning to take, my Ego immediately consults some other entities in my brain where he (my Ego) knows that he has been keeping information stored for years—that is, consults his memory. He seems, so to speak, to ring up his Librarians by telephone and to ask them what facts bearing upon the question they have at hand. Perhaps one replies immediately, "You passed that house on the right years ago in company with ——, and that is therefore the road to ——." If, however, there is no such definite reply, something like a Secretary-Cell says from somewhere, "The Librarians have no record, sir; but let me remind you that you have a map in your left coat-pocket." So my Ego wires to my Left Hand to take

it out, to my Right Hand to spread it, to my Eyes to search it, and to my Surveyor to measure the distances. Alas, the map was not made by the German War Office, and therefore the cross-road is not entered upon it. What is poor Ego to do? All the Side-Cells seem to advise this or that, and he listens to all of them. But another and lower Side-Cell points out that the dinner hour is approaching and that a decision must be come to soon. Then it is the Ego-Cell which decides. He has obtained all the advice possible; but he does not take a vote amongst the Side-Cells; he makes the decision himself—just as the amœba or infusorian does The only difference is that the latter have no Librarians and Secretaries to assist them, and their judgment is therefore not likely to be so good. Then the order is given: the Engineer-Cells clutch their levers; the batteries come into play; the great motors begin to act; and away goes the whole huge mechanism down one of the roads. The work now being in the hands of competent subordinates, the Ego-Cell himself has nothing more to do but to admire the scenery.

Seriously, the question appears to be whether Decisions are made by a single entity or by a majority-vote. As Decisions seem to be possible for unicellular organisms, the former appears to be the more probable hypothesis; and we infer (for a speculation) that as organisms become more and more complex as they rise in the animal scale, their nervous mechanism acquires more and more numerous Side-Cells to do the subordinate work, but that the Prime- or Ego-Cell required for ultimate Decisions remains unique in all. Prof. Pearson in his paragraph on "the brain as a central telephone exchange," in *The Grammar of Science* (1900, p. 44), suggests interesting points in the mechanism of thought; but I do not find in this book or in works on psychology or physiological psychology any reference to the possibility that that mechanism may be developed round a single unit which still retains the ultimate autocratic power.

If the speculation be by any possibility sound, we shall be confronted by some startling corollaries; for we men ourselves—we who walk and talk so proudly under the stars and think ourselves to bulk so big in the world—must shrink, so far as our actual self is concerned, to something like one five-hundredth of an inch in diameter—to microscopical objects buried amongst a mass of similar objects in the brain and possibly quite undistinguishable from them in shape or size. The rest of us shall only be our appanages, the servants, property, and house-machinery of the insignificant molecule which is ourself! Just as the emperor is no bigger in body than his subjects, so perhaps the Ego-Cell may be nothing finer than the millions of other little masses of protoplasm which wander in our blood or live unproliferating and undying in that small box of the cranium which can yet hold infinity.

The only way in which research might be able to throw light upon the subject seems to me to be by way of studying the nervous system of the very low organisms in comparison with their habits. For example, gnat-larvæ would be very suitable, and I have often thought of commencing such work upon them. They show a very considerable degree of intelligence. Thus, when the surface tension of the water in which they live is destroyed by oil, they think that something has gone wrong with their air-tube, and spend a long time in trying to clean it up—which shows a degree of intelligence superior to that of many of our municipalities. Would carefully stained preparations of their nervous-system disclose the existence of any very exceptional cell such as is suggested by the above hypothesis? But, of course, the Ego-Cell may not be morphologically different from any of its congeners.[1]

R. R.

[1] Compare *Masefield's Sonnets and Poems*, No. 13 (Letchworth Garden City Press).

REVIEWS

Napier Tercentenary Memorial Volume. Edited by CARGILL GILSTON KNOTT. [Pp. xii + 441.] (Published for the Royal Society of Edinburgh by Longmans, Green & Co., London and New York, 1915. Price 21*s.* net.)

THIS volume is splendidly produced and provided with several fine plates, two of which are in colour, the frontispiece being a reproduction in colour from the portrait of Napier in the possession of the University of Edinburgh. It consists of the Addresses and Essays communicated to the International Congress which met at Edinburgh in July 1914 to celebrate the Tercentenary of the publication of Napier's *Descriptio*, and at the end there is an account of the Congress, list of members, and other matter relating to the Congress, and indexes.

The first thirteen contributions deal with the life and work of Napier himself, and constitute an exceedingly valuable addition to our knowledge of the history of mathematics; the remaining sixteen papers deal with subsequent developments of the logarithmic idea, and the common feature of them is progress in methods of calculation. An account of the Congress—the last International Congress held before war broke out—has been given by Dr. Knott in SCIENCE PROGRESS for October 1915, and an account of some of the papers read at the Congress was given there. Here it will only be necessary to give a list of the papers and make a few remarks on them. The Inaugural Address on "The Invention of Logarithms, its Genesis and Growth," by Lord Moulton, is perhaps the most interesting and novel of all the contributions in the volume. Lord Moulton, with his wide experience of inventions gained in his career at the Bar, has given a reconstruction of the gradual evolution of Napier's ideas, which must be taken into account by all future historians of mathematics. Prof. P. Hume Brown writes a biographical paper on "John Napier of Merchiston," and Mr. George Smith writes a descriptive article on "Merchiston Castle." The other historical papers are in order: Dr. J. W. L. Glaisher, "Logarithms and Computations"; Prof. D. E. Smith, "The Law of Exponents in the Sixteenth Century"; Prof. F. Cajori, "Algebra in Napier's Day and alleged Prior Inventions of Logarithms"; Prof. G. A. Gibson, "Napier's Logarithms and the change to Briggs's Logarithms"; Lieut. Salih Mourad, "Introduction of Logarithms into Turkey"; Prof. J. E. A. Steggall, "A Short Account of the Treatise *De Arte Logistica*"; Prof. G. Vacca, "The First Napierian Logarithm calculated before Napier"; Prof. G. Vacca, "The Theory of Napierian Logarithms explained by Pietro Mongoli (1650)"; Prof. D. M. Y. Sommerville, "Napier's Rules and Trigonometrically Equivalent Polygons"; and Prof. R. A Sampson, "Bibliography of Books Exhibited at the Napier Tercentenary Celebration, July 1914." The papers in the second group are as follows: Prof. H. Andoyer, "Fundamental Trigonometrical and Logarithmic Tables"; Prof. C. G. Knott, "Edward Sang and his Logarithmic Calculations"; Prof. J. Bauschinger, "Formulæ and Scheme of Calculation for the Development of a Function of Two Variables in Spherical Harmonics"; Prof. M. d'Ocagne, "Numerical Tables and Nomograms"; Prof. M. d'Ocagne, "On the Origin of

Machines of Direct Multiplication"; Mrs. E. Gifford, "New Table of Natural Sines"; Dr. J. R. Milne, "The Arrangement of Mathematical Tables"; Mr. T. C. Hudson, "Note on 'Critical' Tables"; Prof. J. E. A. Steggall, "On a Possible Economy of Entries in Tables of Logarithmic and other Functions"; Dr. A. Hutchinson, "The Graphical Treatment of some Crystallographic Problems"; Mr. William Schooling, "A Method of Computing Logarithms by Simple Addition"; Mr. A. K. Erlang, "How to Reduce to a Minimum the Mean Error of Tables"; Dr. W. F. Sheppard, "Extension of Accuracy of Mathematical Tables by Improvement of Differences"; Dr. Artemas Martin, "A Method of Finding without the Use of Tables the Number Corresponding to a given Natural Logarithm"; Mr. H. S. Gay, "Approximate Determination of the Functions of an Angle, and the Converse"; M. Albert Quiquet, "Life Probabilities: on a Logarithmic Criterion of Dr. Goldziher, and on its Extension."

The following remarks may be useful to a mathematician who reads through this volume with the intention of forming a sound opinion on the various historical questions that arise about the discovery of logarithms. On arithmetic and algebra in Napier's time, see p. 69 (Glaisher); on the method of "Prosthaphæresis" or the method of replacing the product of trigonometrical functions by a sum of other trigonometrical functions, referred to on p. 7 of Lord Moulton's Address, cf. p. 73 (Glaisher), pp. 99–100 (Cajori), and pp. 219–21 (Sampson); on the comparison of an arithmetical progression with a geometrical progression, cf. p. 7 (Moulton) and pp. 81–2 (Smith); and on methods of calculation with Napier and later writers, see pp. 70–71 (Glaisher). With Lord Moulton's theory should also be compared Prof. Carslaw's theory mentioned in this quarterly for January 1916 (p. 434).

The only mistake which there appears to be in this volume is that curious mistake of spelling "*Tabullen*" in the name of Bürgi's *Progress Tabulen*, which Prof. Knott has made both in his article in this quarterly referred to above and on p. vi of the volume under review. This is all the more strange as there is a facsimile of Bürgi's title-page facing p. 210.

PHILIP E. B. JOURDAIN.

A Course of Modern Analysis. An Introduction to the General Theory of Infinite Processes and of Analytical Functions, with an Account of the Principal Transcendental Functions. Second Edition, completely Revised. By E. T. WHITTAKER, D.Sc., F.R.S., Professor of Mathematics in the University of Edinburgh, and G. N. WATSON, M.A., Fellow of Trinity College, Cambridge, and Assistant Professor of Pure Mathematics at University College, London. [Pp. 560.] (Cambridge: at the University Press, 1915. Price 18*s*. net.)

IT is scarcely necessary to give a formal review merely on the occasion of a second edition of so admirable and well known a book as this one; but, as we happen to know, many have been long awaiting the second edition, and our readers may therefore be glad to be furnished with some idea as to the changes and additions made in it. In the first place, for the second edition, Prof. Whittaker's name is associated with that of Mr. G. N. Watson, who, Prof. Whittaker tells us in his Preface, is responsible for the new chapters on Riemann Integration, on Integral Equations, and on the Riemann-Zeta Function. The second edition adopts the Peano decimal system of paragraphing, much to the advantage of the work. The two Parts of the original work still stand; but the original Chapter 4 on Uniform Convergents is suppressed and partly replaced

by the new Chapters 3 and 4 on Continuous Functions and Uniform Convergents, and on the Theory of Riemann Integration. The old Chapters 3, 5, 6, 7 and 8 still stand with changed numbers. But three more new chapters have been added to the First Part, namely on Fourier's Series, Linear Differential Equations, and Integral Equations.

In the Second Part all the eight chapters of the first edition still stand, but the single chapter on Hypergeometric Functions is now replaced by two, while the three original chapters on Elliptic Functions are now put into one chapter or are otherwise distributed. The new chapters in the second edition are named, The Zeta-Function of Riemann, The Confluent Hypergeometric Function, The Mathiew Functions, The Theta Functions, and the Jacobean Elliptic Functions.

We rather miss the detailed table of contents in the first edition, which is now replaced by a single list of contents including only chapters. On the other hand, to make up for this, the new edition contains an excellent list of authors quoted and also a general index in place of the original rather scanty index of terms employed. It will thus be apparent that the changes are considerable. The work is so well done that one can usually find in it most of the matter now being often brought out separately in the form of small mathematical volumes, and is therefore indispensable, both for workers and teachers. The new edition contains a total of 560 pages against a total of 378 pages in the first edition.

A Treatise on Electricity. By F. B. PIDDUCK, Fellow of Queen's College, Oxford. [Pp. xiv + 646, with 369 illustrations.] (Cambridge: at the University Press, 1916. Price 14s. net.)

THE enormous development of the modern branches of electricity in the last decade has produced a large number of monographs and special treatises which have been written by specialists for the benefit of the advanced worker. These books are very formidable, not to say bewildering, to the student, faced as he is with the necessity of knowing something of each subject for his final examination and lacking the time and the experience necessary to enable him to pick out for himself their more important parts. Much of the matter they contain is still in a state of flux, but there is a great deal whose position and relative importance in the main structure have become quite definite, and a complete treatise incorporating such fundamental facts is considerably overdue. Mr. Pidduck has undertaken the task and his book will become henceforth an almost indispensable part of a third year's reading for Physics Honours. To cover so much ground in a single volume makes the problem of selection a very difficult one, and in this matter the author has achieved a notable success. Magnetism has perhaps been rather neglected—there is, for example, no reference to Langevin's theory and its later developments—but the conflicting claims of the theoretical and experimental sides of the subject have been balanced most admirably. There is even a chapter of sixty pages devoted to applied electricity which contains just the knowledge that ought to be in possession of the student of pure Physics, whose course too frequently omits all reference to such mundane things as dynamo characteristics and A.C. motors.

No attempt has been made to avoid the mathematics which is essential to the full comprehension of the subject, but for the most part it is put very clearly. In the discussion of the screening effect of a hollow iron sphere, as in one or two other cases, the author assumes the particular form of the potential functions which satisfy the equations of condition. This of course shortens the work considerably,

but it is most unsatisfactory from the point of view of the Physics student, and some direct hint as to the derivation of the solution is always desirable.

After an introduction dealing with a few of the more important mathematical theorems, *e.g.*, Gauss' transformation, seven chapters are given up to the older and fundamental parts of the subject. It is suggested that Whetham's *Theory of Experimental Physics* or a similar treatise would serve as a useful preparation for the more advanced treatment given here. A course of reading made up of these two books would, however, be far from complete, since necessities of space have constrained the author to leave all side issues (*e.g.* the method of electric images) severely alone. A book, such as Foster and Porter, dealing with these portions of the subject more thoroughly would be necessary to cover the ground adequately.

All through special attention has been paid to methods and apparatus used in ionisation work, and in this connection it is surprising to find only the old incomplete theory of the quadrant electrometer, although Dolezalek's latest "Binant" pattern is referred to. The chapter on electric currents is rather more elementary than any of the others. It contains an account of Eichenwald's experiments on the magnetic effect of a moving charge, a subject which generally suffers from a most undeserved neglect; but in the section on the variation of resistance with temperature there is no reference to Kamerlingh Onnes' latest experiments. Electrical instruments of all kinds are described and illustrated including the newer types of galvanometers, viz., the string galvanometer, the vibration galvanometer, and Duddell's thermo-galvanometer.

Chapters X–XIV (270 pages) are concerned with the modern branches of electricity and are intended as introductions to the special treatise. The subjects dealt with are Electrolysis, Electric Oscillations, Conduction through Gases, Radioactivity and the Electron Theory. They are uniformly excellent throughout and only a brief mention of some of their contents is possible here. The chapter on Electric Oscillations includes a short account of the Electromagnetic Theory of Light, a description of a simple form of laboratory apparatus for experiments on Hertzian waves, and a long discussion on the transmission of electric waves along wires. The latest methods of making the standard measurements are given in the chapter on the Conduction of Electricity through Gases, while such special subjects as Photoelectricity and Crystal Structure are touched on. The Electron Theory includes an account of the Lorentz-Einstein transformation and a short statement on the theory of Quanta.

Sufficient has been written to show how valuable an addition this is to the literature of electricity, and the Cambridge University Press is to be congratulated on its inclusion in their catalogue. The book contains an index which might well be amplified in the next edition.

D. ORSON WOOD.

A Treatise on Light. By R. A. HOUSTOUN, M.A., D.Sc. [Pp. ix + 478.] (London: Longmans, Green & Co., 1915. Price 7*s.* 6*d.* net.)

DESPITE the number of good text-books on Optics which have made their appearance in recent years, Dr. Houstoun's treatise is a welcome addition to the literature of the subject. The choice of material and the concise, yet thorough, treatment make it a veritable storehouse of well-supplied information for the student who has been through the purely elementary parts as treated in the first-year course. In few books will one meet with such an absence of "padding," and such economical use of space; yet, withal, there is no impression of mere cramming

or of overcrowding pages with facts. The wealth of material in the book is arranged in an orderly fashion, and each portion of it serves in its own way to illustrate the principles of the subject.

The treatment of geometrical optics, which constitutes the first part of the book, is unusually good. The theory of lens combinations is based on Helmholtz's tangent law ; and there follows a chapter on the defects on the image formed by a single lens. There is a very judicious selection from the many methods for measuring the various constants of mirrors and lenses and the optical properties of various transparent materials, together with a very satisfactory account of various optical instruments.

The chapters on Interference and Diffraction, in the section of the book which deals with Physical Optics, are thorough in the sense that the physical details are carefully expounded without any appeal to an elaborate mathematics, which makes it all the more remarkable that Dr. Houstoun should, after pointing out the insufficiency of Huygens' principle even after Fresnel's amplification of it, suddenly startle his readers with Kirchoff's formula in its baldest mathematical form, but without the slightest indication as to how it is developed, or how it removes the defects of Fresnel's more elementary treatment. It is not very difficult, after all, to explain to a student, who has grasped the full meaning of propagation with a finite velocity, the meaning of "retarded" amplitudes and potentials, nor to demonstrate that the effect of a secondary source depends on the velocity of vibration there as well as on the displacement.

The treatment of Polarisation leaves little to be desired. Perhaps a little fuller account of the Fresnel-Arago laws on interference with polarised light, together with the bearing of the third of these laws on the nature of unpolarised light, might have been given with advantage ; the treatment of interference phenomena in biaxal crystals is also somewhat too brief.

It is undoubtedly in the third part, on Spectroscopy and Photometry, that the book presents a marked advance on the customary treatment of these subjects. The account of spectroscopic work is roughly on historical lines, a chapter being devoted to earlier and a chapter to later research. There are separate chapters on the ultra-violet and on the infra-red regions, while the description of some of the most recent forms of interferometers and spectrographs is excellent in its lucidity and terseness.

The mathematical treatment of the subject is contained in the fourth and last part. The first chapter of it makes a bold innovation considering the type of student to whom the book is addressed. It essays to give some account of the "pulse" theory of light to the "second-year man," and to remove from his mind the all too prevalent notion that, because we can by a mathematical device treat any light disturbance as simultaneous streams of quasi-homogeneous light, we must therefore imagine the atom to be a collection of equally numerous harmonic vibrators. It must be admitted that the object of this chapter is to be warmly commended. It is a hopeful sign for the literature of any science when the results of the most recent thought and research are so rapidly incorporated in standard text-books. Doubtless this chapter will make a larger demand on the student's mathematical equipment than any other part of the book, but the struggle to grasp its underlying meaning will amply repay him. In the remaining chapters of this part, Dr. Houstoun wisely abandons the old elastic, solid theory, except for a brief reference to some of Fresnel's work, and applies the electromagnetic theory to the explanation of the laws of reflection, refraction, and dispersion, magneto-optics and radiation. A chapter on the relative motion of ether and

matter, with a brief reference to the recent relativity principle, concludes the volume.

An excellent feature of the book is the collection of examples at the end of each chapter; there is none of the artificiality of the average "Examples in Physics" about them. They are akin to the problems which present themselves in a laboratory, and are obviously the invention of one who has been in close touch with practical work.

We have nothing but the warmest praise for this admirable treatise, and hope that it will speedily find its way into the hands of the students at our Universities.

J. RICE.

Relativity and the Electron Theory. By E. CUNNINGHAM, M.A. [Pp. v + 96.] Monographs on Physics. (Longmans, Green & Co., 1915. Price 4*s.* net.)

THIS little book deals with a somewhat narrower subject than the author's recent work *The Principle of Relativity* which was reviewed in last July's issue of SCIENCE PROGRESS. The Relativity principle is a very general hypothesis, viz. that it is impossible by means of physical experiments to determine the *absolute* velocity of a body through space. The acceptance of this hypothesis involves a very considerable change in our notions concerning the measurement of space and time, and in a certain sense it involves a mutual dependence of those two modes of perception, which have hitherto been regarded as independent of one another. The principle itself is based on the failure of a number of now classical researches to detect and measure the velocity of the earth relative to the ether. Some of these experiments date back to the days of Fresnel and Arago, and naturally such failures were the occasion of much discussion before the advent of Einstein's proposition ten years ago. A fair amount of success had attended various efforts to account for the absence of expected effects, by the invention of hypotheses concerning the constitution of matter. Lorentz and Larmor had rendered signal service in several essays, in which they sought to found a complete theory of matter on the hypothesis that the electron was its fundamental constituent. Einstein, by a flash of insight, reversed the procedure; instead of inventing *ad hoc* hypotheses concerning material structure to account for the failure to detect the earth's motion through the ether, he affirmed that these failures were but a sample, as it were, of the failures which would await any attempt to detect absolute motion in space by whatever experimental device the attempt might be made. That postulate, he said, must form the starting point for a revision of our notions of space and time, and thereafter, for a re-discussion of such knowledge, as is summarised, for example, in the equations of the electromagnetic field.

As the experimental failures referred to above occupy a very prominent place in the older Electron theory, and in Einstein's postulate, it is not surprising that many of the results of the Electron theory, arrived at as approximations in Lorentz's analysis, appear in the Relativity theory as exact results, and the monograph under review has for its object an explanation of the relation between the two theories and a demonstration where and how the principle of Relativity is "the natural and necessary complement" of the Electron theory. The book accomplishes its purpose in the same able manner as the author's earlier work dealt with the wider aspects of the principle. It is addressed to a wider public than the earlier volume, and more especially to those experimental physicists whose training in mathematical analysis is so insufficient as to render parts of that book unreadable. The origin and development of the principle are sketched from

its earliest adumbrations in Newton's system of Dynamics to its precise formulation by Einstein. The modifications which its acceptance introduces into our ideas concerning the measurement of space and time are very clearly outlined in a succeeding chapter, and a further chapter is devoted to its application to the equations of the electro-magnetic field, and to the demonstration that the conception of the "contractile electron" to which Lorentz's researches gradually impelled him, is itself a direct application of the principle of Relativity.

The sixth chapter of the book will doubtless prove a "tough nut to crack" for the reader whose mathematical training is slender, yet without it the book would be incomplete; for he who has not grasped that Relativity is not so much a physical law as a general mode of thought which demands that all our physical laws must satisfy a certain test, has missed the entire significance of the principle. The mathematical form into which this test can be cast is most readily expressed in terms of the so-called four-dimensional space of Minkowski; and in the sixth chapter there is an explanation of Minkowski's four-dimensional vectors which is certainly as clear and simple as the nature of the matter allows. Two chapters on the relation of the principle to mechanics and to the conception of an ethereal medium conclude the monograph. As far as the writer of this notice is aware, this little book is, while being a fairly complete account, the simplest exposition in English of the principle of Relativity; and its simplicity is not of that spurious kind which, by avoiding the real difficulties, leaves the reader absolutely unenlightened.

J. R.

The Universe and the Atom. The Ether Constitution, Creation, and Structure of Atoms, Gravitation, and Electricity, Kinetically Explained. By MARION ERWIN, C.E. [Pp. vi + 314, with 58 illustrations.] (London: Constable & Co., 1915. Price 8*s*. 6*d*. net.)

THIS is not a popular exposition of modern physical research; but an account of an original vortex theory of ether structure devised by the author, who has, at the same time, and perhaps unconsciously, also invented a new system of mechanics. Two or three quotations bearing on this point will suffice to show the importance to be attached to the book. First, among the definitions on p. 150 are these: "Pressure is force, weight is force, momentum is force"; again, on p. 152, "In a general sense pressure is momentum." If some doubt is cast on this statement by the fact that, on p. 178, pressure is equated to energy, a final remark (p. 74) that "force represents energy" puts matters right by suggesting that all five terms are synonymous!

The theory is entitled "The Pan Cycle Hypothesis. Invisible Composition Light-waves, the Warp and Woof of the Ether-structure and of all Things Material." It appears to involve a longitudinal as well as a transverse component in the vibrations set up by a light wave; certainly the final conclusion violates the doctrine of the availability of energy.

The wisest sentence in the book is the first, "To acquire a knowledge of the tools to be used, and the materials to be operated upon, is the first step in the successful building of any structure." It is self-evident that if the author had only acted up to his own precept he would have gone no further. As it is there remains a strong suspicion that he has been overwrought by too much study of Ganot's *Physics* and the *Popular Science Monthly*. It should be added in conclusion that the preface is dated from the Garden City, L.I., New York.

D. ORSON WOOD.

Principles of Physical Chemistry. By EDWARD W. WASHBURN, Professor of Physical Chemistry in the University of Illinois. [Pp. xxv + 442, with 61 illustrations.] (New York and London: McGraw-Hill Book Co., 1915. Price 15*s*. net.)

PROF. WASHBURN'S reputation as a physical chemist is such that a book from his pen is likely to meet with a good reception. The present text-book is a valuable addition to the somewhat limited number of manuals of Physical Chemistry written in English and intended for English-speaking readers. The scope of the work will be evident from the chapter-headings, viz. the structure of matter and the composition of substances, the gaseous state of aggregation, the liquid state of aggregation, liquid-gas systems, the crystalline state of aggregation, crystal-gas systems, crystal-liquid systems, relations between physical properties and chemical constitution, Brownian movement and molecular magnitudes, some principles relating to energy, solutions (4 chapters), the colligative properties of solutions of electrolytes, the conduction of electricity, conductance and degree of ionisation, electrical transference, thermochemistry, heat capacity and internal energy, chemical kinetics, chemical equilibrium involving the ions of water, the phase rule, disperse systems, radioactivity, atomic structure and periodic system, and finally an appendix dealing with certain thermodynamic derivations. The author has contrived to compress all this inside 420 pages, a not inconsiderable feat, for the result is by no means a "cram" book. Both kinetic theory and thermodynamics are freely employed throughout. The greatest difficulty for the reader is to understand the arrangement of the matter dealt with. Each chapter is in itself excellent, but it is difficult in some cases to see why a given chapter occurs where it does. The author himself, as we infer from his preface, has been guided much more by teaching considerations than by an attempt to give the whole subject an atmosphere of unity. To come to some points of more detail. One of the most striking features of the book is the treatment of solutions. Solutions are divided into two great classes, namely, solutions of constant thermodynamic environment, and solutions whose environment is a function of the concentration. To the first belong ideal and dilute solutions; to the second, concentrated solutions and solutions of electrolytes. All the usual properties of solutions are discussed from this point of view. Prof. Washburn has very decided views regarding the inadmissibility of identifying osmotic pressure with gas pressure in spite of their formal identity in dilute solution. The problem of electrical conduction in solutions is fairly fully dealt with, though in the opinion of the reviewer the author has shown too great modesty in the chapter on electrical transference by not including an account of his own very accurate measurements by the reference-substance method in addition to the short table of results given on p. 232.

In a book of this size, in which at the same time certain subjects (such as solutions) are treated particularly fully, it is inevitable that some subjects are omitted. Thus photo-chemistry is dealt with in a single paragraph, electromotive-force is practically not considered, and affinity is not even mentioned. On the other hand, the recent work on atomic heats, especially the applicability of Debye's equation, is discussed very clearly, as is also von Weimarn's theory of disperse systems, the determination of the charge on the electron, whilst room is also found for a brief reference to the work of Laue and the Braggs upon X-rays and crystal structure. In short, what Prof. Washburn has undertaken to expound has been done in a masterly way. It would have been eminently desirable had

the work been considerably expanded. For its present dimensions the price seems slightly excessive.

W. C. McC. Lewis.

Catalogue of the Mesozoic Plants in the Department of Geology. The Cretaceous Flora. Part I. Bibliography, Algæ, and Fungi. [Pp. xxiii + 285, with 2 plates and 25 figures in the text.] 1913. Price 12*s.*—Part II. Lower Greensand (Aptian) Plants of Britain. [Pp. xxxvi + 360, with 32 plates and 112 figures in the text.] 1915. Price 21*s.* By Marie C. Stopes, D.Sc., Ph.D., F.L.S. (London: printed by order of the Trustees of the British Museum.)

The Cretaceous Flora is of special interest, for, as the author says, it differs fundamentally from all the older floras in the presence (and in many places the preponderance) of Dicotyledonous and Monocotyledonous plants, together with the older and "simpler" families. "The Angiosperms must undoubtedly trace their ancestors further back than the lowest Cretaceous, but for practical purposes of geology they may almost be described as appearing in the Cretaceous period."

The present Catalogue is an exceedingly welcome contribution to our knowledge of the plants of this critical stage of botanical evolution.

The first volume begins with a short sketch of the occurrence of Cretaceous floras in different parts of the world, illustrated by a useful comparative stratigraphical table. This is followed by a list of literature and a full enumeration of all recorded species of Cretaceous plants, which occupies much the greater part of the volume. The descriptive portion is short, for not many Algæ and Fungi are known from these rocks. Among the former the Calcareous Siphoneaceæ and Corallinaceæ are of chief interest, as their structure lends itself to good preservation. The Fungi are in a much less favourable position, but among them two good examples of parasitic (or possibly saprophytic) forms, referred to the Pyrenomycetes, are included—*Pleosporites Shirainus*, and *Petrosphæria japonica*—the one infesting a *Cryptomeriopsis* and the other a *Saururopsis*, from the Upper Cretaceous "coal-balls" of Hokkaido, Japan. The structure of these microscopic Fungi is well preserved.

The geological scope of the second and larger volume is more limited; it is concerned with the Lower Greensand (Aptian) plants of Britain. This volume embodies a great amount of original research, and is of the utmost value as a detailed and critical study of a Lower Cretaceous Flora.

The author finds that, contrary to what had been supposed, "the flora of the English Lower Greensand is rich, not only in species, but in interest": the petrifactions, showing admirably preserved structure, are the great feature, and the point of chief scientific interest is the presence of highly organised Angiospermous stems among the remains of the earlier Gymnospermous vegetation. The forms recorded number 45, comprising 1 Thallophyte, 2 Ferns, 9 Cycadophytes, 27 Conifers, and 5 Angiosperms.

As regards climate, the absence of Araucarians, and the occurrence of well-marked annual rings in the Coniferous woods, suggest a cooling off from the warmth of the Wealden period.

The excellent account of the strange structure of the Fern *Tempskya* is based on Kidston and Gwynne-Vaughan's description of a Russian species.

Among the Cycadophytes, the account of the classical *Bennettites Gibsonianus* includes some interesting details and original figures of the anatomy. A new species, *B. Allchini*, from Kent, is described, and the anatomy of Carruthers'

species *B. maximus*, remarkable for the small development of the wood, is illustrated.

The Conifers occupy some 200 pages of the Catalogue; among them numerous species of petrified wood of this class are described. The classification and naming of Coniferous woods is admittedly one of the most difficult subjects in palæobotany; the author recognises seven genera, and, in accordance with views now prevalent, attaches special value to the pitting of the medullary ray-cells as a means of discrimination. It is pointed out, however, that all the seven are "pseudo-genera," and are founded only for convenience of description. In favourable cases the affinity with recent genera may be clear, as, for example, between species of *Pityoxylon* and the Pines. The full and admirably illustrated description of the anatomy of so many woods, including a number of new species, appreciably widens the basis of our knowledge.

Besides the woods, the petrified twig of a *Sequoia*, *S. giganteoides*, and the beautifully preserved cones of *Pinostrobus* and *Cedrostrobus* are of special interest.

At the close of the section on Coniferales, a remarkable specimen from the Isle of Wight, *Vectia luccombensis*, is described. It consists of a well-preserved tissue, almost certainly phloëm, attaining a thickness of 26 mm. and probably more. So great a mass of phloëm is almost unprecedented. The author inclines to regard it as Coniferous, and suggests a comparison with *Cryptomeria*, *Thuja*, and other genera. To the reviewer it appears more probable that *Vectia* was the phloëm of a Cycadophyte, such as *Bennettites*, in which this tissue is known to have reached a remarkable development. The structure of *Vectia* bears a sufficiently striking resemblance to that of the phloëm of *Bennettites Gibsonianus* (cf. fig. 73 with fig. 10) to suggest affinity.

Of the five species of Angiospermous stems described, two (*Cantia arborescens* and *Hythia Elgari*) are new, while the rest have been recorded by the author in a previous publication. All show perfectly typical Dicotyledonous wood-structure, and have nothing primitive about them; they also differ widely among themselves, showing that very diverse types had already appeared at this early date. In no case can any certain conclusions as to affinities be drawn, though *Woburnia* shows agreement with Dipterocarpaceæ; the main point is the presence of highly advanced and differentiated woody Angiospermous plants, while the "Age of Gymnosperms" was still in progress. The origin of the Flowering Plants must lie very far back.

An appendix to the volume deals with some fossils, which, though found in the Potton Sands, of Lower Greensand age, were probably derived from the Wealden. They include two species referred to *Cycadeoidea*, Buckland, a genus commonly taken as synonymous with *Bennettites*, Carruthers, but which the author regards as distinct. On her view, the character distinctive of *Cycadeoidea* is the presence of two or more zones of secondary wood in the trunk, while in *Bennettites* there is a single zone. The two zones were mentioned by Buckland in his species *C. microphylla*, but unfortunately the type specimens, both of this and of his other species *C. megalophylla*, are lost. The species referred to *Cycadeoidea* in the present volume are *C. Yatesii*, Carruthers, and *C. buzzardensis*, sp. nov., with numerous zones of wood. The fructification in both is unknown.

The next plant described, *Colymbetes Edwardsi*, shows an extraordinary structure, and well deserves its rank as the type of a new genus. The specimen, of unknown origin, but probably from the "Potton Sands," consists of the inner portion of a trunk, including the large pith and a considerable thickness of wood. The pith, like that of other Cycadophytes, contains numerous gum-canals, and is

surrounded by groups of primary xylem. But the peculiarity of the fossil lies in the structure of the secondary wood, which consists of ten or more successive zones; the zones alternate, the elements of each zone running in a direction approximately at right angles to those of its neighbours. This has the curious effect that the transverse and radial sections appear just alike—every alternate zone being cut radially in transverse section, and transversely in radial section, and *vice versâ*. All the zones, in spite of the violent changes in their direction, are quite continuous, and appear to be the product of a single cambium. The remarkable structure, which seems to be without analogy (except, very remotely, with *Dracæna*), is beautifully illustrated, and the facts quite clearly established. The tracheides are scalariform. The plant was undoubtedly a Cycadophyte, but there is no clue to its closer affinities.

A much fuller review would be needed to do complete justice to this important work, dealing, as it necessarily does, with so many matters of detail. The illustrations, both plates and text-figures, are admirable, and these two volumes, which we hope to see followed by others, are a most valuable addition to the great series of catalogues published under the auspices of the British Museum.

D. H. SCOTT.

Metamorphic Geology. By C. K. LEITH and W. J. MEAD. [Pp. xxiii + 337, with 16 plates and 35 figures.] (New York: Henry Holt & Co., 1915. Price $2.50.)

IN this book "metamorphism" is taken to include all the changes--physical, chemical, and mineralogical—that a rock may undergo from the time of its original crystallisation as a primary igneous rock. As a broad basis for systematic discussion the conception of a "metamorphic cycle" is introduced. Starting with igneous rocks as the primary source and initial condition of the working materials, the gradual evolution of the various end products of *katamorphism* (weathering) is traced. The effects of cementation are then followed, and finally, the development of slates, schists, and gneisses from any of the pre-existing types is described. Changes brought about by cementation, or by dynamic and thermal metamorphism, are grouped together as *anamorphism*.

The work is divided into four parts, dealing respectively with Katamorphism, Anamorphism, General Discussion, and Laboratory Work. Graphic methods are used wherever possible, and indeed nearly all the illustrations are diagrams illustrating the chemical and mineralogical changes undergone by average or specific types of rocks. Generally these diagrams are clear and effective, but occasionally the authors so overburden them with detail that it becomes a severe mental strain to grasp their significance. Only four photo-micrographs are reproduced, and field occurrence is left unillustrated. The illustrations thus reflect both the purpose and the limitations of the book, which is almost wholly devoted to a statistical method of investigation. Nevertheless, though "office" study predominates over laboratory and field work, there is no lack of stimulating suggestion, and one feels that the book marks a forward step in the direction of a new geology which will be largely quantitative.

In Part I there is an interesting chapter on laterites and bauxites. It is held that kaolinisation is an intermediate process, and that if the resulting product be porous, so that solutions can continue to percolate, then a further leaching of silica takes place until only stable hydrated oxides are left. It is worthy of notice that while many writers have assumed the average igneous rock to be equivalent to approximately equal parts of granite and basalt, Messrs. Leith and Mead show

that the average composition of sediments demands a dual parentage composed of 65 per cent. of granite and 35 per cent. of basalt. Such rock on decomposition and sorting yields 82 per cent. of shale, 12 per cent. of sandstone, and 6 per cent. of limestone. To express the redistribution more accurately—

100	grams	igneous rock		87·8	grams	Shale
5·3	„	CO_2		12·9	„	Sandstone
2·0	„	H_2O		6·7	„	Limestone
1·0	„	O_2	yield	2·0	„	Oceanic salts, etc.
0·7	„	C				
0·3	„	SO_3		109·4 grams		
0·1	„	other substances				

Metamorphism as understood in Europe is divided into dynamic and thermal anamorphism, though it is admitted that usually the distinction is difficult to define. A chapter is devoted to textural and structural changes, and the views of Becke and Grubenmann are discussed and criticised at some length. The authors show that anamorphic changes in sediments are in the general direction of a return to igneous rock composition, but they recognise that the assemblage of minerals in schists and gneisses differs both quantitatively and qualitatively from those of igneous rocks. Unless actual fusion takes place, as in stoping or marginal assimilation, the metamorphic cycle is not known to be closed.

The various criteria employed in determining the origin of schists and gneisses are thoroughly examined, and it is shown that no one criterion can usually give a conclusive result. A welcome feature in the presentation of the subject-matter of the book is the treatment (already strongly advocated in this country by Mr. T. Crook) of ore deposits as rocks.

The authors are to be congratulated on the production of a lucid and logical account of metamorphism, for with all the limitations of their methods they have covered the whole subject in such a way that the reader cannot fail to grasp the essentials of every change and to realise its place in the pulsating evolution of rocks.

ARTHUR HOLMES.

Biology. By Prof. GARY N. CALKINS, Ph.D. [Pp. viii + 241, with 101 figures.] (New York: Henry Holt & Co., 1914. Price 7*s*. 6*d*. net.)

PROF. CALKINS in *Biology* provides the student with an introduction to biological studies, and the method he adopts is to treat a number of types of the lower animals and plants. The type system has great advantages for instructional purposes, more especially when accompanied by well-arranged laboratory exercises, but in many cases it tends to produce a course that is no longer justly described as biology but becomes rather comparative morphology and anatomy. This volume is far from doing that, for the problems are all approached from the physiological point of view, and as the author claims "animal differentiation for the performance of primary functions of protoplasm is the main theme of the entire course." It therefore forms an excellent complementary work to be read in conjunction with the ordinary biological text-books generally in use in this country.

The main types dealt with are, Yeast, Bacteria, *Amœba*, *Chilomonas*, *Paramœcium*, *Hydra*, *Lumbricus*, *Homarus*, *Tænia*, *Pleurococcus*, *Sphærella* and *Pteris*, but other forms are briefly referred to in dealing with certain points. Each type is dealt with morphologically and histologically first and then physiologically, and finally most of them are utilised to illustrate some biological

phenomena. Thus *Hydra* leads to symbiosis and by a reference to *Obelia* to polymorphism: *Tænia* is used to introduce parasitism, protective adaptations against parasites, and immunity. The first chapter is a general introduction to the vital phenomena exhibited by protoplasm and the last deals with a number of problems related to evolution.

The illustrations are throughout good, many of them taken from Sedgwick and Wilson's *General Biology*, to which the book forms a suitable companion, a number of others appearing here for the first time, and some of these, *e.g.* two of the earthworm, are very useful.

It is a well-planned work and one that can be read with profit and enjoyment by all students of biology.

C. H. O'D.

The Mechanism of Mendelian Heredity. By T. H. MORGAN, A. H. STURTEVANT, H. J. MULLER, and C. B. BRIDGES. [Pp. xiii + 262, with a frontispiece and 64 illustrations.] (London: Constable & Co., 1915. Price 12*s.* net.)

PROF. T. H. MORGAN is well known as a supporter of the biological teachings grouped together under the name of "Mendelism," and his researches with those of his co-workers have much enriched our knowledge of the phenomena of heredity. Most of this work has been carried out on the fruit-fly *Drosophila ampelophila*, which has consequently become as much a classic type as the *Œnothera* of De Vries or the peas of Mendel himself. *Drosophila* possesses a number of advantages for the purposes of investigation: it breeds freely and rapidly, is easily manipulated, and, what is extremely useful, entails the minimum of space and expense. These points made it possible for more than one hundred characters to be studied within a comparatively short period of time. It was found that these characters tended to be inherited in four groups: two of which were large and one very small. This becomes the more interesting when we find that the somatic number of chromosomes is four pairs in both male and female, and that they consist of two pairs of large, one pair of median, and one pair of quite small chromosomes. The smallness of this diploid chromosome number much facilitates the cytological study of the germ-cells. The ordinary phenomena of Mendelian inheritance are well illustrated by a number of characters in the fly.

Two of the most interesting phenomena are linkage and crossing over. To quote: "Whenever the two factors remain together in the same chromosome there will be formed equal numbers of gametes containing the two factors, and of gametes containing the normal allelomorphs of the two factors. But if pieces of homologous chromosomes are interchanged, then some of the gametes will contain one of the factors in question, and an equal number will contain the other factor. The process of interchange between chromosomes is called crossing over; the tendency of factors to stay together is called linkage." Reasons are given for supposing that "the percentage of crossing over is an expression of the distance of the factors from each other," and on these lines an interesting diagram of the relative distribution of the factors in the chromosomes is constructed.

Useful chapters on sex inheritance, the constitution and distribution of the chromosomes, multiple allelomorphs and multiple factors follow. The concluding chapter treats of the "Factorial Hypothesis" and summarises the authors' views on Mendelian factors and the part they play in heredity. Two theories prevalent among Mendelians in this country, namely, the presence and absence theory and the theory of inhibitors, are criticised strongly, and a very good case against both

of them is made out. This is, indeed, a welcome step, for the nomenclature and proportions of Mendelian results are complicated enough, and a certain amount of unnecessary complexity is removed when these two concepts are discarded.

Prof. Morgan is an out-and-out believer in his theories, and seems quite prepared to regard the Mendelian hypothesis as capable of explaining the inheritance of any or all characters. Some results are quoted as supporting certain interpretations, but to one not immersed in Mendelism it is not entirely satisfactory to find that when according to theory the expected ratio in a count of nearly 3,000 was 15 : 0 : 1, the result obtained was 22 : 3 : 1. If these extensions of Mendelian proportions are capable of mathematical statement, and are going to produce figures that are of some significance, and capable of application to other problems, one would certainly hope for a closer correspondence between predicted and actual results. One further point appears open to criticism: in parts the book is very difficult to read and follow. This may follow from joint authorship, or more probably from a laudable intention not to make the book too long and at the same time include all the necessary information.

Apart from this, however, the work is an excellent one, and one for which the reader will have much to thank the authors. It is well printed and illustrated, and contains a useful bibliography. The plea in its introduction that the study of heredity should not be confined to the expert, as it tends to be at the present time, but should form part of the working equipment of every biologist, is one that will be heartily endorsed in any school of biological thought, and this kind of book goes far to aid the production of that very desirable result. In our opinion it is perhaps the most valuable contribution that has been made to such literature in English since the translation of Mendel's original papers in 1900.

C. H. O'D.

The Embryology of the Honey Bee. By J. A. NELSON, Ph.D. [Pp. v + 282 with 95 text figures and 6 plates.] (Princeton: Princeton University Press. London: Humphrey Milford, Oxford University Press, 1915.)

MUCH has been written on the bee from many different aspects. Certain points in or parts of its embryology have been worked at by such well-known zoologists as Bütschli and Kowalevski, and more recently by Blochmann, Petrunkewitsch, Nachtsheim, and Dickel, yet only one comprehensive account has been given, namely that by Battista Grassi in 1884 in a somewhat obscure Italian publication. Good as this account was, it is quite obvious that the whole problem was ripe to be tackled again in the light of the general advances that have been made in our knowledge of insect embryology. Considerable improvement in methods of microscopical preparation enables the difficulties in technique inherent in the investigation of the embryology of an insect to be more successfully surmounted. The present volume is an attempt, and a very successful one, to remedy this deficiency in our knowledge of the bee. The various changes from the unsegmented egg onwards are carefully and tersely described and the main points dealt with in the description illustrated. The illustrations themselves call for note as they are beautifully drawn and reproduced, and show clearly that it is not necessary to rely on the ordinary half-tone process for reproducing sections.

The difficulty of interpreting many of the phenomena in the formation of the germ layers and embryonic membranes is very great, for as Weismann long ago pointed out, the ontogeny of the insecta is more distorted and cœnogenetically degenerate than perhaps in any other group of animals. Hence the value of carefully recorded and well-illustrated accounts of these occurrences such as we

find in this work. The chapter on the duration and rate of development, on the technique, a short summary, and an exhaustive bibliography form very useful additions.

The author is to be congratulated on having produced a very valuable book, which, by treating the subject in a comparative manner, not only adds to our knowledge of the actual embryological processes in the bee itself, but brings them into relationship with those in allied forms.

C. H. O'D.

The Alligator and its Allies. By A. M. REESE, Ph.D. [Pp. xi + 358, with 62 figures and 28 plates.] (London and New York: G. P. Putnam's Sons, 1915. Price 10*s.* 6*d.* net.)

IN a book of this description it is to be regretted that the author did not check and extend the observations of previous writers upon which he draws so freely. One result of this is, that a certain number of avoidable incongruities occur. For example, a map of the distribution of the Crocodilia (taken from the Cambridge Natural History) is given on p. 6, on which it does not show that crocodiles are to be found in Florida; yet on the same page, and only a few lines lower down, it is stated that they do occur there. An attempt is made to give a list of the existing species of crocodiles; the list is quoted from Ditmar's *Reptiles of the World*, and has not got added to it such interesting forms as *Tomistoma schlegelii* or *Crocodilus cataphractus*, to mention only two species. The incomplete notes on the ancestry of the Crocodilia do not give any account of the recent palæontological work on the group, and so are of little value. Throughout certain chapters of the book we find constant references to Bronn's account of one thing or another; indeed, Chapter III. is admittedly an unverified translation from Bronn's *Thierreich*, illustrated by its figures. It is misleading to call this Bronn's account, however, since the three volumes on Reptilia in Bronn's *Thierreich* were the work of Prof. C. K. Hoffmann years after Prof. Bronn's death. All this kind of thing detracts from the value of a book, and, indeed, the present volume on the whole is disappointing, for it contains little that cannot be obtained from other sources and a deal that is taken from standard text-books. We cannot think it justifies the claim on its wrapper to be an "exhaustive scientific treatise."

C. H. O'D.

The Mosquitoes of North and Central America and the West Indies. By LELAND O. HOWARD, HARRISON G. DYAR and FREDERICK KNAB. Vol. III. Systematic Description (in two parts). Part I. [Pp. vi + 523.] (Washington D.C.: Published by the Carnegie Institution of Washington, 1915.)

SINCE the appearance of the first two volumes of this monograph in 1913, its importance and value have been universally acknowledged, the authors by their masterly treatment of the general consideration of the subject, in Volume I, having rendered their work of great service to a wide circle of readers. The issue of the systematic portion (illustrated by the plates and drawings composing the second volume) will therefore provide much satisfaction to all who are interested in this group of insects, but it will naturally appeal more particularly to those concerned with the taxonomic aspect. The publication of the present volume has been awaited with considerable interest since the wide dispersal of the necessary literature and the not infrequently meagre and often inadequate descriptions supplied therein have hitherto rendered the recognition of many species a difficult matter.

According to the conclusions arrived at by the authors the mosquitoes form a sub-family of the Diptera and are divisible into two tribes only—*Sabethini* and *Culicini*. The two remaining and generally accepted tribes of *Anophelini* and *Megarhinini* are not recognised as such and are merely given group rank under the *Culicini*. In all, 382 species belonging to 25 genera are treated in this monograph, and of these 214 species included in 16 genera are dealt with in the volume under review. In spite of these comparatively large numbers, however, we are assured that many more species undoubtedly occur within the area under consideration, as but few parts have been at all adequately explored, and many large tracts, including the whole of Arctic North America, remain practically untouched. Preceding the detailed accounts of the genera and species are a few pages devoted to short statements regarding the definition of mosquitoes and their position among other insects, the characters employed in the tables and the geographical area covered by the work; an historical sketch of the classification of mosquitoes is also provided. The taxonomic portion of the book opens with the tribe *Sabethini*, in which 8 genera and 85 species are minutely considered, and the original descriptions of the unidentified genera *Goeldia* and *Isostomyia* are incorporated and remarked upon. Six of the species allocated to this tribe and described in this work are new to science. Of the tribe *Culicini*, 8 genera and 129 species receive attention and include descriptions of seven new species and a new sub-genus (*Climacura*) of *Culex*, which has been erected for the reception of the aberrant form *Culex melanurus*, Coq. This tribe is divided into four groups on general relationship—Deinoceritines with three genera, Culicines with eleven genera, Megarhinines with the single genus *Megarhinus*, and Anophelines with the genera *Anopheles* and *Cœlodiazesis*. In this volume the Deinoceritine group and the genera *Culex*, *Carollia*, *Lutzia*, *Culiseta* and *Mansonia* of the Culicines are discussed. The information relating to these forms and the members of the *Sabethini* is exhaustive and is arranged in an admirable and methodical manner. In each case the general tribal characters of the adults, larvæ and pupæ, are first enumerated and are followed by remarks on the evolution and affinities of various genera or groups and accounts of their distribution and bionomics. Separate synoptic tables of the genera according to adult characters, male genitalia, and, as far as possible, larval structure are then given. Detailed treatment of each genus ensues: complete synonymy and homonymy, with references and notification of the type species, generic diagnoses of the adult and larva, remarks upon relationships, synonymy and bionomics, and series of dichotomic tables of the species (based on the structure and coloration of the adults, the male genitalia, and the larvæ) are set forth in the above order. The various species of the genus are then dealt with on similar lines. Whenever possible descriptions of both sexes, the male genitalia, and the larvæ are given in addition to the original descriptions of the type and more important synonyms; much interesting and important information regarding the distribution, synonymy, and bionomics of each species is also included.

The production of this exhaustive treatise must have entailed a vast amount of work both in the field and the laboratory, and the authors are to be sincerely congratulated upon the results of their labours. They have produced a work which has a distinct value of its own; as such it may justly be regarded as indispensable to all who make a study of these insects, whether the forms in question be indigenous to the regions covered by this work or not.

H. F. C.

Records of the Indian Museum. (A Journal of Indian Zoology.) Vol. XII. Part II. March 1916. (Calcutta: Published by order of the Indian Museum Trustees. Price Rs. 2.)

THIS part of the Indian Museum records is devoted to the whip-scorpions and marine mollusca. The first paper, by Mr. Gravely, is on the evolution and distribution of the Indo-Australian Thelyphonidæ, with notes on the distinctive characters of various species. The systematic portion supplements the information given by Pocock in the volume on Arachnida in the *Fauna of British India* (1900), and the various papers of Kræpelin. The area dealt with by Gravely is considerably more extensive than that included in the *Fauna of India* volume.

The geographical distribution of the various species is given in detail, and there is some interesting material on specialisation of certain characters within the limits of this family. Although Mr. Gravely has added considerably to our knowledge since the publication of Pocock's volume, it is obvious that much remains to be done.

The second paper, by Mr. Preston, is on a small collection of marine mollusca dredged in shallow water in the Andaman Islands. Mr. Preston, who has quite recently contributed (March 1915) a volume on the Mollusca (Freshwater Gastropoda and Pelecypoda) to the *Fauna of British India*, here describes a number of new species of coastal marine mollusca.

J. T. J.

The Apple. A Practical Treatise Dealing with the Latest Modern Practices of Apple Culture. By ALBERT E. WILKINSON. [Pp. xii + 492, with 4 coloured plates and 195 figures.] (Boston and London: Ginn & Co., 1915. Price 8*s*. 6*d*. net.)

A VERY extensive literature has accumulated during recent years on the subject of apples and their culture. Much of this information is contained in periodicals, Agricultural Bulletins, etc., and, though embodying many experimental results of the first importance to the practical man, is too scattered to be readily accessible.

In this useful manual of modern methods of apple culture, as practised in North America, the author has epitomised and brought within the compass of a single volume the more essential facts from the large mass of available information.

Nearly every aspect of the subject is dealt with in a scientific and clear manner that will appeal alike to the fruit specialist and the amateur grower.

The major part of the text is devoted to cultural methods, and we note with pleasure the absence of dogmatism on controversial points such as sod culture versus tillage and the application of artificial manures. Several chapters are given to Insect Pests, Diseases, Spraying, Propagation, Breeding, etc., and useful information is provided on Methods of Harvesting, Marketing, and the utilisation of by-products.

The subject-matter throughout is well arranged, but it would have increased facility of reference if the somewhat full summary of contents had been incorporated in the rather meagre index.

E. J. S.

Rambles in the Vaudese Alps. By F. S. SALISBURY, M.A. [Pp. x + 154, with 8 plates.] (London: J. M. Dent & Sons, 1916. Price 2*s*. 6*d*. net.)

WRITERS too often fail to recognise the extreme complexity of the combination of factors which go to make up that intangible something wherein lies the atmosphere and charm of any district.

Not the least attractive feature of the present volume is the obvious catholicity of the author's interests and the varied aspects from which he treats his subject.

In the course of these rambles in the neighbourhood of Gryon many of the more familiar alpine plants are brought to our notice and some feature of interest, either in their folk-lore, situation, or structure, is explained.

The freedom from technicality and the occasional touches of humour render the book eminently readable, and to the lover of flowers, for whom the Vaudese Alps are something more than a mere pleasure resort, it should prove both interesting and attractive. It is unfortunate that the value of the illustrations has been minimised by the absence of any direct reference to them in the text.

E. J. S.

Physiological Abstracts, Vol. I, Nos. I and II. Issued by the Physiological Society (Great Britain and Ireland), with the co-operation of the American Physiological Society, and also of the American Society for Experimental Pathology, the American Pharmacological Society, the American Society of Biological Chemists, Biologisk Selskab of Copenhagen, the Carnegie Institute of Washington, the Chemical Society (Great Britain and Ireland), the Russian Biological Society, *Archivio di Fisiologia* (Prof. Fano), *Archives Italiennes de Physiologie* (Prof. Aducco), *Journal de Physiologie* (Prof. Dastre), Professors Arthus (Switzerland), Boldyreff (Russia), H. J. Hamburger (Holland), A. Krogh (Copenhagen), Overton (Sweden), Torup (Norway), and Zwaardemaker (Holland).—Abstractors: G. Barger, W. M. Bayliss, F. G. Benedict, T. Graham Brown, R. Brinkman, G. A. Buckmaster, F. J. J. Buytendijk, H. W. Bywaters, Mrs. G. D. Cathcart, W. J. Gies, T. Lewis, Keith Lucas, W. Mair, E. Mellanby, Mrs. E. Mellanby, O. Rosenheim, C. S. Sherrington, S. B. Schryver, W. L. Symes, S. Tait, W. H. Thompson, G. S. Walpole, Eric M. P. Widmark. Edited by W. D. HALLIBURTON, Professor of Physiology, King's College, London. [Pp. 70.] (London: H. K. Lewis, 1916. Price monthly, 3*s.* net; yearly, 25*s.* net.)

PROF. HALLIBURTON tells us, in his "Foreword" to *Physiological Abstracts*, that their scope will be wide, and that they are to appear in monthly parts of which twelve will constitute a volume.

The first (April) part comprises sixty-eight Abstracts, varying in length from a line (or less) to a couple of pages (or more), distributed over thirty-four pages comparable in dimensions with those of the *Proceedings of the Royal Society*. The second (May) issue contains fewer of the more wordy contributions. It gives almost double as many abstracts in the same number of pages. In each part the titles of foreign papers are given in English, and the cover bears a table of contents in numerical order.

As promised, the scope of the venture is really wide. For example, three consecutive abstracts deal respectively with soap, sand, and sugar. Animal (including human) physiology is the most prominent feature, but plant physiology is also represented. Pathology, too, of the lower animals as well as of men, obtains a fair share of the pages.

The individual abstracts are clear and well presented. They will be of value to all workers in biological science as a guide to, or as a substitute for, the original matter with which they deal. In the latter capacity they should appeal to a large number of those concerned with the applications of physiology—*e.g.* in

medicine, in veterinary science, and in agriculture—to whom the originals are, for one or other reason, not accessible.

A few words of hope, in anticipation! At the present rate the completed volume would contain roughly 400 pages and 1,100 abstracts. Judging from similar publications in other countries, there may be expected in an ordinary year upwards of 3,000 papers to be abstracted. This figure suggests that the bulk of the volume must inevitably be increased. It further arouses the hope that the conciseness shown in his abstracts by W. D. H. may be, in some degree, acquired by his collaborators. Failing this, the volume—if it is to maintain completeness—must become inconveniently bulky.

A feature of cardinal importance, in a volume of abstracts, is the index; for on it depends the permanence of their value. In itself, the index of authors is usually adequate. A good index of subjects is, however, uncommon; and without a good index of subjects the value of abstracts is little more than momentary.

These *Physiological Abstracts* are intrinsically valuable. We venture, therefore, to express the hope that the volumes will, from the beginning, be so indexed as to ensure that their value shall be permanent.

W. L. SYMES.

Radium, X-Rays and the Living Cell. With Physical Introduction. By HECTOR COLWELL, M.B., D.P.H., late Assistant in the Cancer Research Laboratories, Middlesex Hospital, and SIDNEY RUSS, D.Sc., Physicist to the Middlesex Hospital. [Pp. x + 324, with 2 plates and 61 figures in the text.] (London: G. Bell & Sons, 1915. Price 12*s.* 6*d.* net.)

THE publication of this book marks a new step in the progress of scientific medicine. It is the combined work of a medical man and a physicist, and in all probability it is the forerunner of other works dealing with kindred subjects. The subject-matter is dealt with in a clear and convincing manner from the physical, the chemical, and the biological point of view. It is the type of book which at the present time is needed by the medical man who, before advising his patients to undergo radiation treatment, wishes to learn something of the manner in which these agents act, and to satisfy himself that the treatment is truly scientific and not of the nature of a "placebo."

A perusal of the book shows clearly the principles upon which present-day radiation therapy is based, and indicates the possible development of the future.

The introductory chapters deal with the physics of X-rays, radium, and allied bodies. They form an excellent résumé of the investigations carried out by many workers, and should be invaluable to the practical radiologist.

The first chapter deals briefly with the Röntgen or X-rays, the historical résumé s complete, the method of production is clearly described, and the question of measurement of X-rays is discussed. The point is clearly made that up to the present no satisfactory method of measurement in clinical work has yet been produced.

The important subject of secondary X-rays is clearly dealt with in Chapter II.

The transmission of X-rays through matter is exhaustively treated, and a great deal of valuable data provided.

Radioactive substances are next discussed in practically all their aspects, and indications given of their properties.

The radiations from radioactive bodies are also dealt with fully, the three types of rays being described, and the striking differences from one another indicated.

The relative ionising and penetrating powers of the Alpha, Beta and Gamma, and X-rays are dealt with in Chapter VII.

The radioactive emanations are fully described; the changes in the radium which lead to their production, and the important changes which emanation undergoes are shown.

Part II of the book, which occupies over two-thirds of the volume, deals in succession with the chemical action of the rays, the action upon certain forms of animal life, developing forms, seeds and plants, bacteria, and finally the action upon normal and morbid tissues.

The matter dealt with in this section of the book is very important, and deals with the theories on which the rationale of treatment by radiations is based. It is also an historical survey of radiotherapy, and places in the hand of the radiologist a valuable collection of facts and data expressed in a clear and convincing manner, which reflects great credit upon the authors and upon all those associated with them in a large amount of experimental work, the record of which makes up the bulk of the book.

The action of radiation from radium and X-rays upon malignant cells is fully entered into—many photo-micrographs showing the action upon the cells illustrate these pages.

The much-discussed question of the production of malignancy in normal tissues by radiation is also fully discussed.

The chapter on idiosyncrasy and dosage provides information which will be very valuable to the radiologist.

The book concludes with a chapter on the important subject of selective and differential action of the rays.

The appearance of a work dealing with the action of radium and X-rays upon the living cell is a clear testimony to the importance now attached to radiation treatment in practical medicine—a treatment which may revolutionise many theories hitherto accepted in explanation of the actions of drugs upon the system.

The book is illustrated by a number of figures and two coloured plates. The photo-micrographs are of the highest standard. The coloured plates are reproduced from Clunet's work on *The Effects of X-Rays upon the Skin*.

The authors and publishers are to be heartily congratulated upon the production of the book.

Rural Sanitation in the Tropics. Being Notes and Observations in the Malay Archipelago, Panama, and other Lands. By MALCOLM WATSON, M.D., C.M., D.P.H. [Pp. xvi + 320, with illustrations.] (London: John Murray. Price 12*s.* net.)

DR. MALCOLM WATSON is one of the three or four British medical men who, out of the hundreds or thousands of health officers and medical officers working in the tropics, have made enthusiastic and persistent efforts to apply recent scientific discoveries regarding diseases directly to the reduction of them. He was, perhaps, the first Briton to take up the matter of malaria-prevention shortly after malaria was proved to be carried by mosquitoes—that is, about sixteen years ago. Stationed in the malarious Federated Malay States, being then Medical Officer of the town of Klang, he was sagacious enough to recognise at once what a great benefit would be conferred upon the country if the disease could by any means be reduced in it. The practical problem confronting him was by no means an easy one. The rainful is large, the country is covered with rich vegetation, and the plantations are worked by large collections of coolies, chiefly brought from India.

Under these conditions malaria-bearing mosquitoes are likely to abound, while they are able to convey the disease with the greatest facility from man to man amongst the crowded occupants of the "coolie lines" in the plantations. Though these are also the conditions in many other parts of the world, yet few countries have had the good fortune to possess local officials who have the enthusiasm and the energy to undertake new work of this nature; and it was certainly due chiefly to the spirit and the example of Dr. Watson that the authorities in the State of Selangor took up the matter. At that time the new town of Port Swettenham was being built close to Klang, and there was a great wave of malaria running through both places. The areas were, however, rapidly drained, and the result was almost immediate. Not only did the malaria fall to a very low rate, as easily proved by Watson's figures, but, owing to the general diminution of this one disease, the *total* death rate diminished within the sanitated areas—the contrast between them and the unsanitated areas outside being quite remarkable. Shortly afterwards Dr. Watson commenced to extend his work to plantations outside the principal towns. The country consists of a large area of flat land and also a large area of hilly land. In the former his success was rapid, because the mosquito which carries malaria there can easily be dealt with by drainage and removal of jungle close to habitations. But in the hilly land there dwells a much more difficult insect, the larvæ of which can live in the little side pools of the most precipitous hill torrents, and this requires an expensive form of malaria prophylaxis by means of piped drains.

Dr. Malcolm Watson's book is both a record and a text-book. It teaches in the best manner possible by the description of the author's own attempts, failures, and successes. It is well printed and beautifully illustrated, and every Health Officer in the tropics should have a copy. It contains not only an account of his own work in the Federated Malay States, but also an extremely interesting record of his visit to Panama, which he made for comparing notes regarding the prevention of malaria there and in his own country; and the comparison leads to most authoritative pronouncements on a number of moot points and practical details. The book, however, deals not only with the prevention of malaria, but also with that of yellow fever, and, as its title indicates, with most matters connected with rural sanitation in general. The reviewer knows no better book upon the subject, and in fact no book so good.

The difficulties which Dr. Watson has had to face and which he has continued to face for so many years have been extraordinarily great owing to the nature of the climate in the area dealt with by him; and it is just this point which reflects such credit upon him. We are too much given to bestowing all the praise in such cases merely to the men who have done the fundamental researches, and to ignore those who have applied these researches directly for the welfare of humanity. In the reviewer's opinion men like Dr. Watson and Colonel Gorgas are as much entitled to the world's gratitude.

As a nation we have not done a tithe of what we ought to have done as regards tropical sanitation in general and the prevention of malaria in particular. This has been due to the false ideals engendered by a preposterous education, party politics, and game-playing; by unscientific habits of thought and want of discipline in administration; and by the ease with which third-rate persons climb into the most responsible posts, often to the exclusion of genuine workers—qualities abundantly shown also in the war. In the reviewer's opinion the British have largely neglected the great duty in this respect imposed upon them by their possession of the fairest parts of the tropics.

R. R.

Sleeping Sickness. A Record of Four Years' War Against it in Principe, Portuguese West Africa. By B. F. BRUTO DA COSTA, Licentiate First Class and Chief of the Mission, J. FIRMINO SANT' ANNA, Licentiate First Class, A. CORREIA DOS SANTOS, Licentiate First Class, and M. G. DE ARAUJO ALVARES, Licentiate Second Class. Published in Portuguese in "Archivos de Hygiene e Pathalogia Exoticas," Vol. V., March 30, 1915. Translated by J. A. WYLLIE, F.R.G.S., Lieut.-Colonel, Indian Army (retired). [Pp. xii + 261, with 68 illustrations and 2 maps.] (Published for the Centro Colonial, Lisbon, by Baillière, Tindall & Cox. Price 7*s*. 6*d*. net.)

WAR against disease is a legitimate form of warfare which, like most other profitable and legitimate things, is neglected by humanity. Probably the whole sum of money ever spent by mankind to get rid of the diseases which destroy them by millions would not equal the sum spent every day by the British Empire for the purpose of killing Germans—a good purpose, it is true. Nothing convinces me that mankind in the mass has not even yet reached the evolutionary stage of a completely reasoning animal more than this fact. He imagines that he is a god already; but I suspect that when he will come to make this claim before the Throne the answer will be, "What did you do to organise for your advantage the beautiful garden in which I gave you to live?" Poor mankind before that Judge will have little to reply. Half the world is a beautiful garden, but one which is rendered almost uninhabitable and useless by diseases. Science has found the cause of most of these diseases and how to prevent them; but the curious people who rule us do not possess the intelligence to use the information which men of science have obtained. That is the truth and the entire truth.

The Portuguese Islands of San Thomé and Principe off the West Coast of Africa were known to be hotbeds of sleeping sickness since 1871, when Dr. Ferreira Ribeiro discovered it in them. The disease has remained principally in the latter island, where it has often reached alarming proportions—so much so that, according to certain political agitators, "it has been accused of being the focus of the spread of the disease to West Africa. This is something like accusing Hammersmith of being a focus for the spread of measles in London—since sleeping sickness is fairly prevalent roughly all through West Africa. The accusation may be right in one respect, since any focus of high intensity of any disease is, for mathematical reasons, likely to be a danger to areas where the disease does not abound so much. At all events, we are glad to be able to record that the enlightened Portuguese medical authorities of this book have, together with their colleagues and with the warm support of their Government, long carried out a laborious campaign against sleeping sickness in Principe; and this book is an admirable record of their doings, admirably translated by Colonel Wyllie, F.R.G.S., and most beautifully illustrated with numerous full-page photographs of the island. As every one knows, the *Glossina palpalis* which carries the disease breeds chiefly in proximity to water, and loves the shade of a luxurious vegetation, conditions which abound in Principe. I have no space to describe the scientific and sanitary work explained in the book, but these will interest not only the many medical men who have made a particular study of sleeping sickness, but also the general reader, who might like to keep this beautiful record of a good work.

The translator has interleaved a note by himself on the politics mentioned above. I have no knowledge of this matter. He attributes the attack upon the

Portuguese authorities for their alleged indifference regarding the presence of sleeping sickness in Principe to certain German influences in Great Britain. His note stated that our Foreign Office has turned a deaf ear to these machinations. Colonel Wyllie says that the "'Foreign Office White Book, Africa, No. 2, 1913,' is worth reading for the illuminating exposé it gives of their inaccuracy, defective reasoning powers, and valueless suggestions. This collection of documents dealt the *coup de grâce* to the San Thomé Slavery Lie—the trump-card of pan-Germanism in England to follow that of Congo Atrocities, already played."

R. R.

Discoveries and Inventions of the Twentieth Century. By EDWARD CRESSY. [Pp. xvi + 398, illustrated.] (London: George Routledge & Sons, Ltd. Price 7*s*. 6*d*. net.)

MANY more books of this nature should be published. They contain the real history of the world, that is of the advances made by the human intelligence from time to time, and are much to be preferred for educational purposes to the accounts, often quite untrue, of political and military events and of the doings of kings, generals, and various kinds of impostors who pretend that they are advancing the cause of civilisation. It would be good if our youth could learn to distinguish between genuine efforts for human advancement and the bogus efforts. Slums, disease, vice, cheating, crimes, bad laws, and wars will assuredly continue until the mass of men are able to appreciate the difference. Mr. Edward Cressy's book will be of interest to many others besides school-boys. It is in the nature of a sequel to Robert Routledge's *Discoveries and Inventions of the Nineteenth Century*, but deals with engineering inventions rather than discovery—it would of course have been quite impossible to include the latter in any detail in a book of this size. The chapters include histories, recent advances in connection with steam power, the generation and transmission of electricity and electric lighting, the electric furnace, the artificial production of cold, soils and crops, railways, motor cars, ships, the conquest of the air, wireless telegraphy, ships of war, photography, radium, and the constitution of matter—altogether a useful mass of information. The writing is generally good, but at times the author has not taken quite sufficient pains, we think, to explain details in a manner free from ambiguity. Of course many of the chapters may have been greatly amplified, especially the one upon aeroplanes. The book was written before the war, but this does not greatly detract from its value.

Aircraft in Warfare. The Dawn of the Fourth Arm. By F. W. LANCHESTER, M.Inst.C.E., M.Inst.A.E., with an Introductory Preface by Major-General Sir DAVID HENDERSON, K.C.B., Director-General of Military Aeronautics. [Pp. xx + 222, with 14 plates and 21 figures in the text.] (London: Constable & Co., 1916. Price 12*s*. 6*d*. net.)

THE author in his "Note" states "that the present work may be said to date from its contribution as a series of articles to *Engineering*, covering a period from September to December 1914 . . . the last two chapters, however, include new matter." This reason is given for reprinting and revising—"that articles in a technical journal, whatever its standing may be, can never appeal to so wide a circle as a publication in book form." We agree entirely.

The work is a serious and well-reasoned contribution to military and naval aeronautics and appears at a very opportune time. If it finds Sir David Henderson

—to use his own words—"strongly entrenched behind a barricade of military prejudice," it may be that Mr. Lanchester will prove an efficient bomber in a supervision trench. On the other hand, if that section of the public and newspaper press, who see in the occasional nightly raids of the Zeppelin, reasons for advocating an impossible host of aircraft defenders of all types along our eastern, south-eastern and south coasts would read this book, they would be able to place the dirigible in its proper place, and speed up, instead of harassing, those in authority, on whom a very heavy burden has been laid, by the murder of our civilians and the wanton destruction of private property by the aircraft of a criminal enemy.

The volume will be a very great help to those young men who have taken up aircraft work to aid their country in its time of stress, as it deals with the subject in all its branches from actual aerial warfare to pure reconnaissance work. All the points are reasoned out in a manner that makes the reading deeply interesting, valuable, and convincing.

The military value of aircraft is taken up very vigorously by the author. "Now there are many otherwise competent authorities who would deny to the aeroplane (or to aircraft generally) the potential importance which the author hopes satisfactorily to demonstrate is its due; let us put the matter to the test. . . . The foregoing does not constitute a demonstration that the air service is in the future destined to become as important an auxiliary to an army in the field as the cavalry of to-day, although this is in effect the belief of the author." The author, later, discusses the combined use of the aeroplane and cavalry; "exactly how the combination of the two arms will be controlled and handled it is impossible, without actual experience, to lay down."

Here, perhaps, the author may have changed his ideas since 1914, and would willingly concede the added importance of aircraft in modern warfare and promote it from the fourth to the "third arm."

The only point where we find ourselves differing from the author is in one of his conclusions. "Germany backed the wrong horse. The Zeppelin, from a military standpoint, has proved a complete failure. If the resources thus diverted into a useless channel . . ."

We have not the slightest evidence that fleets of Zeppelins have been sent out from Germany for military purposes and reasons *only*.

The sowing of mines, the indiscriminate throwing of bombs and scouting, may, *in the German mind*, have more than justified the Zeppelin. Our enemy from the very first has never neglected psychical effects — indeed, they have been most carefully studied and their air raids may be regarded by the Kaiser's Government "to have paid." In other words, they have worried the people and thereby harassed the Government.

We heartily recommend the book to all readers.

J. Wemyss Anderson.

March 1916.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

Historical Introduction to Mathematical Literature. By G. A. Miller, Professor of Mathematics in the University of Illinois. New York: The Macmillan Company, 1916. (Pp. xiii + 302.) Price 7*s*. net.

Diophantine Analysis. By Robert D. Carmichael, Assistant Professor of Mathematics in the University of Illinois. Mathematical Monographs. Edited by Mansfield Merriman and Robert S. Woodward. No. 16. First Edition. London: Chapman & Hall; New York: John Wiley & Sons. 1915. (Pp. vi + 118.) Price 5*s*. 6*d*. net.

Revue Semestrielle des Publications Mathématiques. Rédigée sous les Auspices de la Société Mathématique d'Amsterdam. Par H. de Vries, Amsterdam; J. Cardinaal, Delft; J. C. Kluyver, Leyde; W. Kapteyn, Utrecht; F. Schuh, Groningue. Tome xxiv, Première Partie, 1915, Avril-Octobre. Amsterdam: Delsman en Nolthenius. Paris: Gauthier-Villars. Leipzig: B. G. Teubner. London: Kegan Paul, Trench, Trubner & Co. New York, U.S.A.: G. E. Stechert. 1916. (Pp. 136.) Annual subscription, 7*s*.

Wiskundige Opgaven met de Oplossingen door Leden van Het Wiskundig Genootschap. Tivaalfe Deel, 1ste Stuk, 1915. (Pp. 76.) 2de Stuk, 1916. (Pp. 77-156.) Amsterdam: H. C. Delsman. Annual subscription, 5 francs.

Nieuw Archief voor Wiskunde, Uitgegeven door het Wiskundig Genootschap te Amsterdam. Onder Redactie van J. C. Kluyver, D. J. Korteweg en F. Schuf. Tweede Reeks. Deel xi. Derde en Vierde Stuk. Amsterdam: Delsman en Nolthenius, 1915. (Pp. 239-416.)

The Integration of Functions of a Single Variable. By G. H. Hardy, M.A., F.R.S., Fellow and Lecturer of Trinity College and Cayley Lecturer in Mathematics in the University of Cambridge. Second Edition. Cambridge Tracts in Mathematics and Mathematical Physics. No. 2. General Editors, J. G. Leatham, M.A., and G. H. Hardy, M.A., F.R.S. London: Cambridge University Press, Fetter Lane, E.C., and Edinburgh, 1916. (Pp. viii + 67.) Price 3*s*. net.

Interpolated Six-place Tables of the Logarithms of Numbers and the Natural and Logarithmic Trigonometric Functions. Edited by Horace Wilmer Marsh, Head of Department of Mathematics, School of Science and Technology, Pratt Institute. First Edition. London: Chapman & Hall; New York: John Wiley & Sons, 1916. (Pp. xii + 155.) Price 5*s*. 6*d*. net.

The Moon Considered as a Planet, a World, and a Satellite. By James Nasmyth, C.E., and James Carpenter, F.R.A.S., late of the Royal Observatory, Greenwich. With 26 Illustrative Plates of Lunar Objects, Phenomena and Scenery, Numerous Diagrams, etc. London: John Murray, Albemarle Street, 1916. (Pp. xix + 314.) Price 2*s*. 6*d*.

The Physical Properties of Colloidal Solutions. By E. F. Burton, B.A. (Camb.), Ph.D. (Toronto), Associate Professor of Physics, University of Toronto; formerly 1851 Exhibition Scholar of the University of Toronto, and Research Student of Emmanuel College, Cambridge. With 18 Illustrations. Monographs on Physics. Edited by Sir J. J. Thomson, O.M., F.R.S., Cavendish Professor of Experimental Physics, Cambridge, and Frank Horton, Sc.D., Professor of Physics in the University of London. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta and Madras, 1916. (Pp. 200.) Price 6*s*. net.

A Text-Book of Practical Physics. By H. S. Allen, M.A., D.Sc., Senior Lecturer in Physics, University of London (King's College), and H. Moore, A.R.C.Sc., B.Sc., Lecturer in Physics, University of London (King's College). London: Macmillan & Co., St. Martin's Street, 1916. (Pp. xv + 522.) Price 8*s*. 6*d*. net.

A Text-Book of Mechanics. By Louis A. Martin, Jun., Professor of Mechanics, Stevens Institute of Technology. Vol. VI.: Thermo-dynamics. First Edition. London: Chapman & Hall; New York: John Wiley & Sons, 1916. (Pp. xviii + 313.) Price 7*s*. 6*d*. net.

A Manual of Practical Physics. By H. E. Hadley, B.Sc., Associate of the Royal College of Science, London, Principal of the School of Science, Kidderminster. London: Macmillan & Co., St. Martin's Street, 1916. (Pp. viii + 265.) Price 3*s*. net.

A System of Physical Chemistry. In Two Volumes. By William C. McC. Lewis, M.A. (R.U.I.), D.Sc. (Liv.), Brunner Professor of Physical Chemistry in the University of Liverpool, formerly Lecturer in Physical Chemistry, University College, London. With Diagrams. Text-Books of Physical Chemistry. Edited by Sir William Ramsay, K.C.B., F.R.S. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. Vol. I. xi + 523, Vol. II. vii + 552.) Price 9*s*. net each Volume.

The Thermodynamic Properties of Ammonia, Computed for the Use of Engineers from New Experimental Data Derived from Investigations made at the Massachusetts Institute of Technology. By Frederick G. Keyes and Robert B. Brownlee. First Edition. London: Chapman & Hall; New York: John Wiley & Sons, 1916. (Pp. v + 73.) Price 4*s*. 6*d*. net.

A Senior Experimental Chemistry. By A. E. Dunstan, D.Sc. (Lond.), F.C.S., and F. B. Thole, D.Sc. (Lond.), F.C.S. With 120 Diagrams by E. D. Griffiths, B.Sc. London: Methuen & Co., 36 Essex Street. (Pp. xiii + 522.)

Chemistry in the Service of Man. By Alexander Findlay, M.A., D.Sc., F.I.C., Professor of Chemistry in the University of Wales, and Director of the Edward Davies Chemical Laboratories, University College of Wales, Aberystwyth. With 3 Portraits and 23 Diagrams in the Text. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. ciii + 255.) Price 5*s*. net.

Notes on the Fenland. By T. McKenny Hughes, M.A., F.R.S., F.G.S., F.S.A., Woodwardian Professor of Geology. With a Description of the Shippea Man, by Alexander Macalister, M.A., F.R.S., M.D., Sc.D., Professor of Anatomy. Cambridge: at the University Press, 1916. (Pp. 35.) Price 6*d*. net.

The Gravels of East Anglia. By T. McKenny Hughes, M.A., F.R.S., Woodwardian Professor of Geology. Cambridge: at the University Press, 1916. (Pp. 58.) Price 1*s*. net.

Catalogue of the Mesozoic Plants in the British Museum (Natural History): The Cretaceous Flora. By Marie C. Stopes, D.Sc. (London), Ph.D. (Munich), F.L.S., Fellow of University College, Lecturer in Palæobotany at University College, London University. Part. I. Bibliography, Algæ and Fungi, with 25 Figures in the Text and 2 Plates (Pp. xxiii + 281). Part II. Lower Greensand (Aptian) Plants of Britain, with 112 Figures in the Text and 32 Plates. (Pp. xxxvi + 360.) London: Printed by Order of the Trustees of the British Museum. Sold by Longmans, Green & Co., 39, Paternoster Row, E.C.; B. Quaritch, 11, Grafton Street, New Bond Street, W.; Dulau & Co., 37, Soho Square, W.; and at the British Museum (Natural History), Cromwell Road, S.W., 1913.

The Deposits of the Useful Minerals and Rocks. Their Origin, Form and Content. By Prof. Dr. F. Beyschlag, Geh. Bergrat. Direktor der Kgl. Geolog. Landesanstalt, Berlin; Prof. J. H. L. Vogt an der Universität, Kristiania; Prof. Dr. P. Krusch, Abteilungsdirigent a. d. Kgl. Geolog. Landesanstalt u. Dozent, a. d. Kgl. Bergakademie, Berlin. Translated by S. J. Truscott, Associate

Royal School of Mines, London. Vol. II. Lodes, Metasomatic Deposits, Ore-Beds, Gravel Deposits. With 176 Illustrations. London: Macmillan & Co., St. Martin's Street, 1916. (Pp. xxi + 1262.) Price 20*s*. net.

Catalogue of the Serial Publications Possessed by the Geological Commission of Cape Colony, the Royal Observatory, the Royal Society of South Africa, the South African Association for the Advancement of Science, the South African Museum, and the South African Public Library. With an Appendix containing a list of the serials in the Bolus Herbarium of the South African College. Printed for the Trustees of the South African Public Library, Cape Town, 1912. (Pp. 53.)

Methods in Practical Petrology. Hints on the Preparation and Examination of Rock Slices. By Henry B. Milner, B.A., F.G.S., F.R.G.S., Assistant Demonstrator in Geology in the University of Cambridge, and Gerald M. Part, B.A., F.G.S., F.R.G.S. Cambridge: W. Heffer & Sons, 1916. (Pp. 68.) Price 2*s*. 6*d*. net.

Manuring for Higher Crop Production. By E. J. Russell, D.Sc. (Lond.), Director of the Rothamsted Experimental Station. Cambridge: at the University Press, 1916. (Pp. 67.) Price 3*s*. net.

Diseases of Poultry: their Etiology, Diagnosis, Treatment, and Prevention. By Raymond Pearl, Frank M. Surface, and Maynie R. Curtis, Biologists in the Maine Agricultural Experiment Station. With 72 Illustrations. New York: The Macmillan Co., 1915. (Pp. xi + 342.) Price 8*s*. 6*d*. net.

Memoirs of the Indian Museum. Fauna of the Chilka Lake. Vol. V., No. 2, October 1915, with 11 Plates. (Pp. 147–97.) Price Rs. 3 8 annas. No. 3, December 1915, with 13 Plates. (Pp. 199–325.) Price Rs. 9. Vol. VI., No. 1, with 9 Plates. (Pp. 1–107.) Price Rs. 7 8 annas. Calcutta: published by order of the Trustees of the Indian Museum. Printed at the Baptist Mission Press, 1915.

Records of the Indian Museum. A Journal of Indian Zoology. Vol. XII., Part I., February 1916. Calcutta: Published by Order of the Trustees of the Indian Museum. Printed at the Baptist Mission Press, 1916. (Pp. 1–57, with 7 Plates.) Price Rs. 2.

Records of the Indian Museum. Vol. XII., Part I., No. 2. On Some Indian Cestoda, Part II. By T. Southwell. February 1916. Calcutta.

The Rat. Reference Tables and Data for the Albino Rat (*Mus norvegicus albinus*) and the Norway Rat (*Mus norvegicus*). Compiled and edited by Henry H. Donaldson. Philadelphia, 1915. Memoirs of the Wistar Institute of Anatomy and Biology. No. 6. (Pp. iv + 278.) Price, post paid to any country, $3.

Modes of Research in Genetics. By Raymond Pearl, Biologist of the Maine Agricultural Experiment Station. New York: The Macmillan Company, 1915. (Pp. vi + 182.) Price 5*s*. 6*d*. net.

Men of the Old Stone Age. Their Environment, Life and Art. By Henry Fairfield Osborn, Sc.D., Princeton; Hon. LL.D. Trinity, Princeton, Columbia; Hon. B.Sc. Cambridge; Hon. Ph.D. Christiania; Research Professor of Zoology, Columbia University; Vertebrate Palæontologist U.S. Geological Survey; Curator Emeritus of Vertebrate Palæontology in the American Museum of Natural History. With 268 Illustrations and Maps. London: G. Bell & Sons, 1916. (Pp. xxvi + 545.) Price 21*s*.

The Involuntary Nervous System. By Walter Holbrook Gaskell, M.A., M.D., F.R.S. With coloured figures. Longmans, Green & Co., 39, Paternoster Row, London, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. ix + 178.) Price 6*s*. net.

Changes in the Food Supply and their Relation to Nutrition. By Lafayette B. Mendel, Professor of Physiological Chemistry in the Sheffield Scientific School of Yale University. London: Humphrey Milford, Oxford University Press; New Haven: Yale University Press, 1916. (Pp. 61.) Price 50 cents net.

Cerebro-Spinal Fever. By Michael Foster, M.A., M.D., Captain Royal Army Medical Corps, Territorial Force, and J. F. Gaskell, M.A., M.D., Captain Royal Army Medical Corps, Territorial Force. Cambridge: at the University Press, 1916. (Pp. viii + 222.) Price 12*s*. 6*d*. net.

The Pathology of Tumours. By E. H. Kettle, M.D., B.S. Lond., Assistant Pathologist, St. Mary's Hospital; Assistant Lecturer on Pathology, St. Mary's Hospital Medical School; formerly Pathologist to the Cancer Hospital, Brompton. With 126 Illustrations. London: H. K. Lewis & Co., 136, Gower Street, W.C., 1916. (Pp. viii + 224.) Price 10*s*. 6*d*. net.

The Daubeny Laboratory Register, 1904–15. With Notes on the Teaching of Natural Philosophy and with Lists of Scientific Researches carried out by Members of Magdalen College, Oxford. By R. T. Gunther, M.A., F.L.S., Fellow and Tutor of Magdalen College. Printed for the Subscribers at the University Press, Oxford, 1916. With Frontispiece and 1 Plate. (Pp. x, 139–292). Price 7*s*. 6*d*. net.

The Respiratory Exchange of Animals and Man. By August Krogh, Ph.D., Reader in Zoophysiology, University of Copenhagen. With Diagrams. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. viii + 173.) Price 6*s*. net.

The Microscopy of Vegetable Foods. With Special Reference to the Detection of Adulteration and the Diagnosis of Mixtures. By Andrew L. Winton, Ph.D., formerly Chief of the U.S. Food and Drug Laboratory at Chicago; formerly in charge of the Analytical Laboratory of the Connecticut Agricultural Experimental Station, etc. With the Collaboration of Dr. Josef Moeller, Aulic Counsellor, Professor of Pharmacognosy, and Head of the Pharmacognostical Institute of the University of Vienna, and Kate Barber Winton, Ph.D., formerly Micro-Analyst U.S. Bureau of Chemistry. With 635 Illustrations. Second Edition. London: Chapman & Hall; New York: John Wiley & Sons, 1916. (Pp. xiv + 701.) Price 27*s*. 6*d*. net.

Localisation by X-Rays and Stereoscopy. By Sir James Mackenzie Davidson, M.B., C.M. Aberd.; Consulting Medical Officer, Röntgen Ray Department, Royal London Ophthalmic Hospital, and X-Ray Department, Charing Cross Hospital; Fellow Physical Society; President, Radiology Section, Seventeenth International Congress of Medicine. With 35 Stereoscopic Illustrations on Special Plates and other Figures in the Text. London: H. K. Lewis & Co., 136, Gower Street, W.C., 1916. (Pp. xi + 72.) Price 7*s*. 6*d*. net.

The New Psychiatry. Being the Morrison Lectures delivered at the Royal College of Physicians of Edinburgh in March 1915. By W. H. B. Stoddart, M.D., F.R.C.P., Lecturer on Mental Diseases to St. Thomas's Hospital; late Resident Physician and Medical Superintendent of Bethlem Royal Hospital; Examiner in Psychology and Mental Disease at the University of London, etc. London: Baillière, Tindall & Cox, 8, Henrietta Street, Covent Garden, 1915. (Pp. iv + 67.) Price 3*s*. 6*d*. net.

Report of First Expedition to South America, 1913. Harvard School of Tropical Medicine. Members of the Expedition: Richard P. Strong, Professor of Tropical Medicine, Harvard University Medical School; Ernest E. Tyzzer, Assistant Professor of Pathology, Harvard University Medical School; Charles T. Brues, Assistant Professor of Economic Entomology, Harvard University; A. W. Sellards, Associate in Tropical Medicine, Harvard University Medical School; J. C. Gastiaburu, Director Municipal Laboratory of Hygiene, Lima. With 48 Plates, 9 Figures and Clinical Chart. Cambridge, Mass.: Harvard University Press, 1915. (Pp. xiv + 219.)

Atmospheric Circulation and Radiation. A Meteorological Treatise on the Circulation and Radiation in the Atmospheres of the Earth and of the Sun. By Frank H. Bigelow, M.A., L.H.D., Professor of Meteorology in the U.S. Weather Bureau, 1891–1910, and in the Argentine Meteorological Office since 1910. First Edition. New York: John Wiley & Son; London: Chapman & Hall, 1915. (Pp. xi + 431.) Price 21*s*. net.

Geodetic Surveying. By Edward R. Carey, C.E., Professor of Railroad Engineering and Geodesy, Rensselaer Polytechnic Institute; Member of the American Society of Civil Engineers; Member of the American Railway Engineering Association; Member of the New York Railway Club. First Edition. London: Chapman & Hall; New York: John Wiley & Sons, 1916. (Pp. ix + 279.) Price 10*s*. 6*d*. net.

Land and Marine Diesel Engines. By Giorgio Supino. Translated by Eng. Lieut.-Commdr. A. G. Bremner, R.N., and James Richardson, B.Sc. (Eng.) Lond., A.M.Inst.C.E. With 10 Plates and 380 Illustrations in the text. London: Charles Griffin & Co., Exeter Street, Strand, 1915. (Pp. xv + 309.) Price 12*s*. 6*d*. net.

Reminiscences of Fifty Years' Experience of the Application of Scientific Method to Brewing Practice. By Horace T. Brown, LL.D., F.R.S. Paper read before the London Section of the Institute of Brewing at a Meeting held on Monday, May 8, 1916, at the Trocadero Restaurant, London. To be published in the May—June 1916 No. of the *Journal of the Institute of Brewing*. London: Harrison & Sons, St. Martin's Lane, 1916. (Pp. 82.)

The Year-Book of the Scientific and Learned Societies of Great Britain and Ireland. A Record of the Work Done in Science, Literature, and Art during the Session 1914-15 by Numerous Societies and Government Institutions. Compiled from Official Sources. Thirty-second Annual Issue. London: Charles Griffin & Co., Exeter Street, Strand, 1915. (Pp. vi + 351.) Price 7*s*. 6*d*. net.

Theosophy and Modern Thought. Four Lectures delivered at the Thirty-ninth Annual Convention of the Theosophical Society, held at Adyar, Madras, December 1914. By Jinarājadāsa, M.A., St. John's College, Cambridge. India: Theosophical Publishing House, Adyar, Madras, 1915. (Pp. 171).

The Coaster at Home. By J. M. Stuart-Young. Vol. I. London: Arthur H. Stockwell, 29, Ludgate Hill, E.C., 1916. (Pp. 404.) Price 5*s*. net.

From the advertisement of this book one is led to expect that it contains much useful information about the West African Coast and one is therefore surprised to find that it is chiefly the personal expression of a particular trader. If one divests one's mind of such preconceived ideas, however, and takes the work on its own basis, there is to be found in it much of interest and merit. The writing, though colloquial, is powerful and holds the attention throughout. In the first part the reader obtains a vivid impression of the country, its inhabitants and the life of the traders; and the sketches of the different people with whom the author has come in contact, though slight, are so cleverly touched-in that each character is a distinct personality and bears the stamp of reality. Any one who contemplates settling in that country would derive from this work an adequate idea of what kind of life he might expect; and those who read this first volume will look forward to the publication of the second, which, the Editor says, will deal more fully with the West African life and the effect of the war on Colonial affairs.

Seventy-Seventh Annual Report of the Registrar-General of Births, Deaths, and Marriages in England and Wales (1914). (Cd. 8206.) Presented to Both Houses of Parliament by Command of His Majesty. London: Printed under the Authority of His Majesty's Stationery Office by Darling & Son, Bacon Street, E., 1916. Price 5*s*. 1*d*.

Alfred Russel Wallace. Letters and Reminiscences. By James Marchant. In Two Volumes. With 2 Photogravures and 8 Half-tone Plates. London: Cassell & Co., and New York, Toronto, and Melbourne, 1916. (Pp. Vol. I. xi + 320, Vol. II. 291.) Price 25*s*. net.

Annual Magazine Subject-Index, 1915. A Subject-Index to a Selected List of American and English Periodicals and Society Publications. Edited by Frederick Winthrop Faxon, A.B. Compiled with the co-operation of Librarians. Boston: The Boston Book Company, 1916. (Pp. 269.)

Sir Alfred Lewis Jones, C.M.G. A Story of Energy and Success. By A. H. Milne, C.M.G. Illustrated with 12 Plates. Liverpool: Henry Young & Sons, 1914. (Pp. vii + 113.)

A Book of Homage to Shakespeare, 1916. Edited by Israel Gollancz, Litt.D., F.B.A., Honorary Secretary of the Shakespeare Tercentenary Committee. Humphrey Milford, Oxford University Press. (Pp. xxvi + 553.) Price 21*s.* net.

The Universal Mind and the Great War. Outlines of a New Religion, Universalism based on Science and the Facts of Creative Evolution. By Edward Drake. London: C. W. Daniel, 3, Tudor Street, E.C. (Pp. 100.) Price 2*s.* 6*d.* net.

The Dramas and Dramatic Dances of Non-European Races, in Special Reference to the Origin of Greek Tragedy. With an Appendix on the Origin of Greek Comedy. By William Ridgeway, Sc.D., F.B.A., Hon. LL.D. (Aberdeen), Hon. Litt.D. (Dublin and Manchester); Disney Professor of Archæology and Brereton Reader in Classics in the University of Cambridge; President of the Royal Anthropological Institute of Great Britain and Ireland, 1908–10; President of the Classical Association of England and Wales, 1914; Gifford Lecturer on Natural Religion in the University of Aberdeen, 1901–11; Honorary Member of the Anthropological Societies of Paris and Brussels, and of the Archæological Society of Athens, etc. With 92 Illustrations. Cambridge: at the University Press, 1915. (Pp. xv + 448.) Price 15*s.* net.

What is Coming? A Forecast of Things After the War. By H. G. Wells. London: Cassell & Co., and New York, Toronto, and Melbourne, 1916. (Pp. 295.) Price 6*s.* net.

Instincts of the Herd in Peace and War. By W. Trotter. London: T. Fisher Unwin, Adelphi Terrace. (Pp. 213.) Price 3*s* 6*d.* net.

Some Aspects of the War as Viewed by Naturalised British Subjects. By August Cohn, of the Middle Temple, Barrister-at-Law. Issued by the Council of Loyal British Subjects of German, Austrian, or Hungarian Birth, 13, Clifford's Inn, London, E.C., 1916. (Pp. 24.)

Messrs. Williams & Wilkins Co., Baltimore, U.S.A., who have recently brought out a *Journal of Immunology*, edited by Arthur F. Coca, under a strong Advisory Board and a Board of Editors, now announce the *Journal of Cancer Research*, edited by Richard Weil under an Editorial Committee.

ANNOUNCEMENT

The forthcoming meeting of the British Association will be held this year at Newcastle-upon-Tyne from Tuesday, September 5, to Saturday, September 9, 1916, under the Presidency of Sir Arthur Evans, D.Litt., F.R.S., Pres. S.A., whose Inaugural Address will be delivered at 8.30 p.m. in the Town Hall. The names of the Presidents of the different Sections are as follows:

Section.	
A. Mathematics and Physical Science	A. N. Whitehead, D.Sc., F.R.S.
B. Chemistry	Prof. G. G. Henderson, D.Sc.
C. Geology	Prof. W. S. Boulton, D.Sc.
D. Zoology	Prof. E. W. MacBride, D.Sc., F.R.S.
E. Geography	D. G. Hogarth, M.A.
F. Economic Science and Statistics	Prof. A. W. Kirkaldy, M.Com.
G. Engineering	G. G. Stoney, F.R.S.
H. Anthropology	R. R. Marett, D.Sc.
I. Physiology	Prof. A. R. Cushny, M.D., F.R.S.
K. Botany	A. B. Rendle, D.Sc., F.R.S.
L. Educational Science	Rev. W. Temple, M.A.
M. Agriculture	E. J. Russell, D.Sc.

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THE MOLECULAR VOLUMES OF LIQUIDS

By SIR EDWARD THORPE, C.B., F.R.S.

As soon as it was clearly recognised that homogeneous chemical compounds had fixed and definite compositions, capable of being expressed quantitatively and in terms of the atomic hypothesis, and that the weights of their unit volumes under standard conditions were equally fixed and definite, it was inevitable that, as the number of such compounds increased, attempts should be made to ascertain what relation, if any, existed between the two physical quantities. Moreover, as with the increase in the number and complexity of such compounds it became possible to classify them into groups, of which the members exhibited relations among themselves more or less well defined, depending upon chemical nature, it became of additional interest to trace the possible connection between chemical nature, as such, and the weight of the unit volume; and the inquiry became still further widened when precise conceptions concerning chemical constitution, structure, molecular arrangement, and the various problems of spatial chemistry became part of the current doctrine of the science. Stated broadly, then, the object was to discover the connection between the molecular weights of substances and the weights of their unit volumes under comparable conditions, or, in other words, their molecular volumes, and their other characteristics, depending upon chemical nature, constitution, symmetry, homology, etc.

For obvious reasons the inquiry soon became more particularly restricted to the case of liquid substances. In the first case, the material was sufficiently abundant to afford the promise of comprehensive generalisations; in fact, many of the questions raised by the inquiry could only be solved by the consideration of facts deduced from the study of liquids. And in the second place, it was easier in their case than in that of solids to establish what might presumably be properly regarded as valid conditions of comparison. The systematic

study of these questions may be said to begin with the work of Hermann Kopp, whose results were published in a series of memoirs contributed to *Poggendorff's Annalen der Physik* and to *Liebig's Annalen der Chemie* at intervals extending over fifty years, viz. from 1839 to 1889. Strictly speaking, Kopp was not actually the first to broach the subject, but such tentative observations as were made before his time possess no practical importance and have only a very slight historical interest. This arose partly from paucity of material, but more especially from the absence of any well-defined basis of comparison, whereby the recognition of any possible generalisations became hopelessly obscured. It was of little use, for example, to determine the weight of the unit-volume of the liquids to be compared at some uniform temperature, say at the melting point of ice, or the mean atmospheric temperature, the conventional temperatures to which specific gravities are usually referred, since liquids are not necessarily under comparable conditions at these temperatures on account of their variable thermal expansibilities. Kopp imagined that a comparable thermal condition would be found at that temperature at which presumably the internal pressures are the same for all liquids, that is, at the temperature at which the liquid passes wholly into the state of gas or vapour, or, in other words, at its normal boiling-point. Other bases of comparison may be suggested nowadays and possibly some of them may be preferable to that adopted by Kopp, but at the time he wrote no other suitable method was open to him and it is at least doubtful even now whether any other system would serve to reveal any more comprehensive or more definite relations. Be this as it may, practically all subsequent workers have followed Kopp's example in principle, whilst in some cases varying his particular experimental procedure. Kopp's method was to select liquids of assured purity and, of course, of known molecular weights; to ascertain their boiling-points under standard conditions, the details of which he greatly improved; to determine their thermal expansibilities over a range sufficiently wide to enable the law of the thermal expansion of each to be ascertained so long as it remained a liquid under normal pressure; to find its specific gravity at some convenient temperature, and thence to calculate the weight of the unit-volume at the normal boiling-point. This value

divided into the molecular weight gave what may be termed the *specific* or *molecular volume* of the liquid at the normal boiling-point, or at the temperatures at which the inter-molecular pressures of all liquids are presumably the same, or at least very approximately the same. If the appropriate units are chosen, the molecular volume may be defined as the volume in cubic centimetres at the normal boiling-points occupied by the molecular weights of the liquid expressed in grammes. Thus, to take a case: the normal paraffin heptane C_7H_{16}, boils under standard conditions at 98·43°, has a relative volume at this temperature of 1·14111 (vol. at 0° = 1), and a specific gravity at 0°/4° of 0·70048. Then since the molecular weight of heptane is 100·16 (o = 16), its molecular volume, that is, the number of cubic centimetres occupied by 100·16 grammes of the hydrocarbon, at its normal boiling-point is

$$\frac{100{\cdot}16 \times 1{\cdot}14111}{0{\cdot}70048} = 163{\cdot}16 \text{ c.c.}$$

This volume may be legitimately assumed to be proportional to the real volume of the molecules together with the interspaces in which they (or the atoms) vibrate. That this assumption is valid may be shown by other considerations. Thus it has been established that the volumes of liquid compounds, however variable, at their normal boiling-points are 1·5 times their volume at the absolute zero. Hence both volumes are proportional to the real molecular volumes.

Since the time of Kopp, and more especially during the last thirty or forty years, the subject has been experimentally studied by numerous investigators, among whom may be mentioned Zander, Buff, Schiff, Gartenmeister, Naubeck, Pinette, Dobriner, Elsässer, and Lossen; and a considerable body of literature has been accumulated, mainly in the form of detached memoirs dealing with special aspects of the general problem. Until quite recently the most noteworthy attempt to co-ordinate this large amount of observational work, and to incorporate its main results into the systematic literature of the science, was made by Horstmann in successive editions of Graham-Otto's *Lehrbuch der Chemie*—a work not generally accessible to English students. In the autumn of last year, however, Mr. Gervaise Le Bas published a careful digest on the subject, as one of the excellent series of Monographs on

Inorganic and Physical Chemistry which are being issued under the direction of Prof. Alexander Findlay.[1]

Mr. Le Bas is well versed in the extensive literature of the subject, and in his previous contributions he has already identified himself with its study. His present work has involved a very considerable amount of preparation, extending, as he informs us, over a period of eight years. His book shows him to be an acute, painstaking, and well-informed critic, and it may be accepted therefore as the most authoritative exposition of the present condition of the subject that has hitherto appeared. We purpose in what follows to give a concise summary of the more significant additions to knowledge to which it leads.

Mr. Le Bas arranges the subject-matter of his work, so far as it relates to the point of view under consideration, in the following groups: Hydrocarbons; Halogen compounds; Organic compounds containing oxygen; Sulphur, nitrogen, phosphorus, arsenic and antimony compounds; Miscellaneous compounds of other elements. Such a scheme of classification is probably the most convenient that could be devised with the material at present available, and it will be desirable therefore to adopt it here.

Of all known groups of liquids the one which may be presumed to lend itself best to the recognition of physico-chemical relationships is that of the *hydrocarbons*. To begin with, their number is relatively large, and they may be sub-divided into well-defined classes, the members of which possess simple and progressive relations among themselves and to the members of other groups. Accordingly they enable the influence of homology, isology, structure, grouping, substitution, and other constitutive changes to be traced more readily than in the case of any other comprehensive class of compounds. As a rule, too, the disturbing effect of "association" is less frequently observed among the hydrocarbons than in other groups of organic compounds, as, for example, in that of the alcohols and certain other oxygenated substances.

Much of what is to be understood by these terms was, of course, quite unknown to Kopp and his immediate suc-

[1] *The Molecular Volumes of Liquid Chemical Compounds from the Point of View of Kopp.* By Gervaise Le Bas. (London: Longmans, Green & Co., 1915.) See also SCIENCE PROGRESS, April 1914.

cessors, since it is for the most part the result of comparatively recent attempts to unravel molecular structure. Although they, no doubt, fully recognised that important clues concerning the connection between molecular volume and chemical composition might be expected to follow from the study of the liquid hydrocarbons, their want of knowledge as to the essential differences in molecular arrangement which may occur among the members of this large group led them to compare substances between which no true analogy exists, and from which no valid or rational deductions could be drawn. Progress under such conditions was therefore impossible. And this leads to the general remark that anything in the nature of sound comprehensive generalisations concerning the connection of the physical quantity we connote by the term molecular volume and chemical nature, using that phrase in its widest sense, can only be expected to follow when we are dealing with groups of substances of which the structure and constitution are understood, or which are at least known to be related among themselves in a manner capable of precise definition. Not that this should discourage the accumulation of the necessary experimental material even in the absence of such knowledge. We agree with Mr. Le Bas that a knowledge of this particular physical constant should be regarded as necessary to the complete history of a chemical compound, even although under our present limitations we may be unable to interpret its full significance. As it is, experience has shown that this constant has served to afford a decisive clue to constitution when chemical considerations alone have given equivocal or contradictory results.

Although the actual numerical values of the molecular volumes given by Kopp for the particular hydrocarbons he studied have been found by subsequent observations to be substantially correct, proving the high degree of accuracy of his experimental work, the inferences he drew as to the fundamental values to be assigned to the respective atoms of carbon and of hydrogen are wholly invalid for the reason already given, that he drew no distinction between hydrocarbons of essentially different constitution; hydrocarbons of open-chain structure, for example, being treated as strictly comparable with those of ring structure. Further investigation has established that Kopp's assumption that similar atoms have

identical volumes in these two classes of compounds is erroneous. There is invariably a considerable contraction in the ring structure as compared with the open-chain hydrocarbon of identical molecular weight.

Kopp found that a difference of CH_2 in what he regarded as a comparable homologous series corresponded to a difference of 22 in molecular volume, and he deduced the separate atomic volumes for C and H in all compounds as respectively 11 and 5·5 or 2 : 1. It has now been shown that the normal values of C and H in all compounds of open chain structure at their boiling-points are respectively 14·7 and 3·7 or 4 : 1, and this relation is preserved at their critical points.

When the difference in constitution between aliphatic and aromatic compounds was recognised, the attempt was made to explain the contraction which occurred in passing from the open-chain compound to that of ring structure by assuming a different value for single-linked and double-linked carbon, whilst that of the hydrogen atom remained constant. Thus in benzene three carbon atoms were assumed to have the value 11·0, the other three having the value 14·0, whilst the volume of the hydrogen atom was uniformly 3·5.

There is, however, no necessity to assume that variable linkage has any influence on the ratio of the atomic volumes of carbon and hydrogen. The 4 : 1 ratio still holds good both at the boiling and critical points and at all equally reduced pressures. At the same time, in passing from the aliphatic to the aromatic class of compounds the atomic volumes of carbon and hydrogen do actually undergo contraction without the characteristic ratio being affected.

It is further established that the values thus obtained are equally true for unsaturated and saturated compounds: the values for corresponding members of the *n* paraffin, olefin and acetylene series are simply dependent on their composition. This is contrary to the conclusion of Buff, who sought to show that the atomic volume of carbon was greater in unsaturated than in saturated compounds. The evidence that unsaturation exerts no special effect would, however, seem to admit of no further doubt.

The scope of this article will not permit of a detailed examination of the various constitutive effects which have been found to influence molecular volume, such as complexity,

branching of the hydrocarbon chain, self-affinity between side-chains, cross-linking, and double bonds, ring systems, etc. All the experimental data hitherto obtained bearing upon these questions have been summarised and discussed in Mr. Le Bas's monograph, to which the reader who desires the information must be referred. There is the less need for dwelling on certain of these points in an article of this character for the reason that the evidence is as yet very meagre. Far more work is needed in several directions before conclusions of sufficiently well-proved generality can be drawn.

What, however, has been stated will suffice to show the character of information which the study of the relations between chemical nature and molecular volume is calculated to afford. In the space which remains we will briefly touch upon the facts yielded by the study of the other groups of substances above referred to and incidentally state what appear to be the more interesting points in the present condition of knowledge on the question.

The values for the *halogens* in a state of combination, as calculated by Kopp, are not very dissimilar from the average volumes obtained from a discussion of much subsequent work, both on organic and inorganic compounds; and these values are not very different from their volumes in the free state. Few observations on fluorine compounds are, however, available on account of the difficulty of preparing these substances and their instability, especially in contact with atmospheric moisture whereby glass vessels are liable to be attacked. In the family of the halogens the atomic volumes increase with the atomic weights, but not proportionally. Their values appear to be affected, but only to a relatively slight effect, by constitutive influences, and they have practically the same value in aromatic as in aliphatic compounds. There is, however, some evidence to show that in the chlorobenzenes the position of the chlorine atom in the molecule is not without influence upon its volume. Far more work, however, is needed before the precise effect of this and other constitutive influences can be said to be satisfactorily made out.

The study of the molecular volumes of substances containing *oxygen* presents features of special interest. It was recognised by Kopp that the value of the oxygen atom was not uniform, but was influenced by the particular method of

combination. According to him what we term hydroxyl or single-linked oxygen had the value 7·8 whereas carboxyl or doubly-linked oxygen was 12·2. These values are not very dissimilar to those now current. The interesting point is that Kopp was the first to detect the effect of what we now term constitution on molecular volume, and thus the first to familiarise us with the conception of its general influence. More extended inquiry has shown that in reality oxygen possesses a number of values depending upon its environment, position, function, etc., and limiting values are now known with approximate accuracy for the various types of combination, *e.g.* hydroxylic, ethereal, ketonic, etc., both in aromatic and aliphatic compounds.

Space will not allow us to pursue this particular matter at length, but we may point out as a matter of general interest its bearing on the question of the molecular constitution of liquid water. Water is known to be what is called an "associated" substance. A number of physical facts combine to show that the formula H_2O does not represent its nature as a whole under any conditions, so far as is known, so long as it remains liquid. Ordinarily water is made up of molecules which are polymers of H_2O, in amount depending upon temperature. Its formula therefore is $(H_2O)_x$ where x is greater than unity.

The determination of its molecular volume at its boiling-point would seem to show that strictly speaking it is not a hydroxyl compound at all, but is, in reality, the first term of the Symmetrical Ether Series, just as hydrogen is "the vanishing point" of the paraffins.

Combined *sulphur*, like oxygen, has at least two molecular volumes depending upon circumstances similar to those indicated in the case of oxygen: one of these values is identical with the molecular volume at its boiling-point in the uncombined state. The molecule of liquid sulphur is known to be very complex—probably S_{10} and S_8; that of gaseous sulphur is S_6 or S_2, depending upon temperature. A knowledge of its molecular volume throws light upon the structure of the sulphur molecule and serves to explain the phenomena of colour and ease of decomposition.

Information as to the molecular volumes of the halogen derivatives of sulphur, and of its oxides, and of a number

of organic compounds of sulphur also tends to elucidate their probable structural formula, and, incidentally, the varying valency of the sulphur atom.

A large amount of work has been done upon the molecular volumes of the different classes of *nitrogen* compounds, organic and inorganic, and a number of significant regularities have been detected which serve to throw light upon the structure of their members. It would exceed the space at our disposal to attempt to deal in detail with the mass of experimental material which has been accumulated. This has been sifted and discussed by Mr. Le Bas, and we must therefore refer the reader who desires fuller information to his treatise. Many illustrations might be given from the study of the nitrogen compounds of the value of observations on molecular volume in affording an insight into molecular arrangement and structural grouping.

The same remark applies with equal force to the liquid compounds of the other typical members of the trivalent series —*phosphorus*, *arsenic*, and *antimony*. Phosphorus, as in the case of certain other elements, would appear to have a much smaller molecular volume when free than when combined, and this fact is consistent with the statial arrangement of the phosphorus molecule, which is known from other physical considerations to be complex. The number of phosphorus compounds which are available for determinations of molecular volume is, of course, far less than in the case of nitrogen, as many groups of nitrogenous compounds have no analogues among the other members of the trivalent series. The greater number of the phosphorus compounds which have been examined are inorganic. They are of comparatively simple constitution, the results are consistent and offer little difficulty in interpretation.

As regards phosphorous oxide, Mr. Le Bas points out that its molecular volume might easily be found experimentally : apparently he is unaware that it was so found by the present writer and Dr. Tutton as far back as 1890 (*Journal Chemical Society*, 57 (1890) 559). The value thus obtained agrees with that calculated on the assumption that the ring grouping and consequent volume of the free phosphorus molecule (P_4) is preserved in P_4O_6, which seems otherwise probable, and that the oxygen atoms are singly linked and

possess Kopp's value of 7·8. It is, of course, easy to construct structural formula for P_4O_6 (which is known to possess this molecular formula) complying with these assumptions, and consistent with its reactions.

Fairly accurate values for molecular volume have now been obtained for the greater number of the non-metallic elements, including the metalloids, and for the relatively few metals which furnish suitable liquid compounds. It is unnecessary to set out these values here or to show in greater detail how they vary with constitutive influences. But one or two considerations arise in connection with them. It was pointed out by the present writer many years ago that a periodic relation may be traced between them. This fact is referred to by Mr. Le Bas and has been confirmed and extended by him. In his work he gives a suggestive diagram which serves to illustrate it very clearly. From it he draws the following conclusions:

(1) That there is a periodic relation between the atomic volumes of the elements.

(2) There is a tendency for the atomic volumes to diminish in each series as the atoms increase in weight. The smallest occur at Group 7.

(3) There is a general increase in the atomic volumes of the members of each Group from Series 1 onwards, that is, in the direction of increasing atomic weight. This increase is usually 3·6 or some multiple thereof.

Mr. Le Bas concludes his work, of which the present article is a very imperfect digest, with a thoughtful summary of the present state of the theory of molecular volumes in which from limitations of space we are reluctantly unable to follow him. The résumé is, however, most suggestive and cannot but serve to quicken renewed interest in the subject. What is needed is fresh investigation directed in the light of modern conceptions of molecular physics and of constitutional chemistry. Any earnest worker in physical chemistry who may be in search of a fruitful field of inquiry will find in this subject abundant opportunity for the exercise of his powers, and he can have no more profitable preparation for his task than a careful study of the monograph with which Mr. Le Bas has enriched the literature of science.

ADSORPTION PHENOMENA AND GIBBS' EQUATION

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The Nature of Adsorption

The operation of removing the colouring matter from solutions by shaking them with finely ground charcoal or by filtration through sand or other material which exposes a large surface is a familiar one both in the laboratory and on the manufacturing scale. The gradual diminution in efficiency, due to clogging of the porous material, arises from the deposition of the extracted matter upon the surface of the charcoal or sand. This removal of certain substances from solution and their condensation upon the surface of the finely divided solid is somewhat loosely termed adsorption. Further examination of the phenomenon has shown, however, that it is complex, involving not only a pure surface-concentration effect, but also a coagulation or flocculation of material at the surface, which may in turn be followed by chemical change in the material, *e.g.* oxidation. In many cases also we find that the extracted material penetrates into the body of the absorbent substance. To cover both adsorptive and absorptive effects the non-committal term " sorption " has been suggested. That charcoal and presumably many other substances exert this twofold effect has been most clearly demonstrated by J. W. McBain (*Phil. Mag.* [6] **18**, 916, 1909) in a paper entitled " The Mechanism of the Adsorption (' sorption ') of Hydrogen by Carbon." McBain shows that the process takes place in two stages. At first there is an extremely rapid increase of concentration of the hydrogen in a very thin layer at the surface of the charcoal, and later there is a slow diffusion of the gas into the interior of the charcoal. It is only the first of these stages which may properly be called adsorption. Adsorption is therefore one of those terms

which, after having been employed for many years to cover a variety of phenomena apparently similar in character, has attached to itself within recent years a much more restricted, but at the same time much more definite meaning. The result is that although the term is less comprehensive, its application is one of much greater precision, and therefore of more scientific value. The kind of phenomenon we are considering depends essentially upon the existence of a surface or interface separating two homogeneous phases. It is known that all such surfaces are characterised by a peculiar distribution of intermolecular forces, which give rise to surface-tension. These surfaces are also characterised by the existence of an electrical potential difference acting across the surface. Adsorption is primarily a capillary surface-tension phenomenon. When the electrical forces enter into the result, the extra effect produced is termed electrocapillary adsorption. It is theoretically possible to eliminate the latter effect or reduce it to negligible dimensions by working under conditions in which the pure capillary effects predominate. In the present survey of the problem the two effects will be considered separately.

Capillary Adsorption—Gibbs' Generalisation

Willard Gibbs, in his now celebrated memoir on equilibrium in heterogeneous systems, was the first to consider the problem of phase-equilibrium when modified by capillary forces. The method of investigation involves a very generalised thermodynamic treatment. The important conclusion reached by Gibbs may be stated as follows: If a substance (a gas or a dissolved substance) possesses the property of *lowering* the interfacial tension which exists between the gaseous or solution phase and some second phase which may be either liquid or solid, then the substance will be attracted to the interface, so that its concentration in the interfacial layer becomes *greater* than its average concentration throughout the bulk of the gaseous or solution phase. If, on the other hand, the substance considered possesses the property of *raising* the interfacial tension, the substance will be repelled from the surface and its concentration in the surface-layer will be *less* than that in the bulk of the homogeneous phase. In the particular case in which the substance exerts no effect upon

the interfacial tension, its concentration in the surface layer will be identical with its bulk concentration in the phase. The first case represents positive adsorption, the second negative adsorption or desorption. The quantity adsorbed,[1] denoted by the symbol Γ, is defined as the mass of the substance in grams reckoned per unit area of the interface in excess of the mass which would be there if capillary effects were absent.

The conclusion referred to above was expressed by Gibbs in a generalised equation involving the chemical potentials of the substances present, the masses adsorbed, the surface tension, and the entropy. This equation has been simplified and applied by Gibbs to the following case:

" If liquid mercury meet the mixed vapours of water and mercury in a plane surface, and we use μ_1 and μ_2 to denote the chemical potentials of mercury and water respectively and place the dividing surface so that $\Gamma_1 = 0$, *i.e.* so that the total quantity of mercury is the same as if the liquid mercury reached this surface on one side and the mercury vapour on the other without change of density on either side, then $\Gamma_{2,1}$ will represent the amount of water in the vicinity of the surface above that which there would be if the water vapour just reached the surface without change of density, and this quantity which we may call the quantity condensed [*i.e.* adsorbed] upon the mercury will be determined by the equation:

$$\Gamma_{2,1} = -\frac{d\sigma}{d\mu_2}$$

where σ is the surface tension. In this equation and the following the temperature is constant and the surface of discontinuity plane. If the pressures in the mixed vapours conform to the law of Dalton, we shall have for constant temperature:

$$dp_2 = c\,d\mu_2$$

p_2 denotes the part of the pressure in the vapour due to the water-vapour and c the density of the water-vapour. Hence:

$$\Gamma_{2,1} = -c\frac{d\sigma}{dp_2} \ . \ . \ ."$$

This expression may be converted into a more convenient

[1] Gibbs himself does not employ the term "adsorption." He speaks instead of matter condensed upon a surface.

form by applying the gas law, $P = RTc$. Gibbs' equation then becomes ·

$$\Gamma = -\frac{c}{RT}\frac{d\sigma}{dc}$$

This is the fundamental expression for adsorption. Three important assumptions are involved in it: (1) the process must be thermodynamically reversible, (2) the temperature must be maintained constant, and (3) the homogeneous phase consisting of gas or solution must be sufficiently dilute for the gas law to be applied. The expression is directly amenable to experimental investigation provided the system is one whose interfacial tension σ is measurable, as a function of the concentration c of the solution or gas, and if further the actual mass Γ of adsorbed material is sufficiently large to be measured. Before taking up the experimental investigation of Gibbs' equation, it is of interest to point out that the equation can be obtained by at least two other thermodynamical methods necessarily equivalent to his, but much less general in form. The first method is by means of a thermodynamical cycle, and is due to Freundlich (*Kapillarchemie*), who based it upon a method given by Milner (*Phil. Mag.* [6] **13**, 96 (1907)). The second involves the use of the perfect differential and has recently been published by Harlow and Willows (*Trans. Faraday Soc.*, **11**, 53, 1915). A. W. Porter (*Trans. Faraday Soc.*, **11**, 51, 1915), has worked out a modified equation to apply when the solution is concentrated. Of these methods we shall only consider that of Harlow and Willows.

Let U be the internal energy of a given mass of solution, whose osmotic pressure is P and volume V; let also σ be the surface tension, S the area of the surface at which adsorption occurs, T the absolute temperature, and ϕ the entropy. Let S and V be changed by small amounts dS and dV at constant temperature, where dV is an increase in the volume of the solution due to an influx of solvent through a semipermeable membrane. It may be necessary to supply a quantity of heat dH to fulfil this condition, where $dH = Td\phi$. Then the change in U is given by:

$$dU = Td\phi + \sigma dS - PdV \quad . \quad . \quad . \quad . \quad . \quad (1)$$

since work has to be done *on* the surface to increase its area and *by* the solution when the volume is increased.

Put— $\Phi = U - T\phi + PV$

Then, $d\Phi = -\phi dT + \sigma dS + VdP$ . . . . . (2)

or, since the temperature is constant and $dT = 0$:

$$d\Phi = \sigma dS + VdP.$$

But $d\Phi$ is a perfect differential, and therefore:

$$\left(\frac{dV}{dS}\right)_p = \left(\frac{d\sigma}{dP}\right)_s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

If the solution contains n grams of solute, and if Γ is the excess of solute in the surface, in grams per cm.², over and above that of the bulk concentration, the concentration in the rest of the solution, instead of being n/V, will be:

$$c = n - \Gamma S$$

Hence,

$$\left(\frac{dV}{dS}\right)_p = \left(\frac{dV}{dS}\right)_c = -\frac{\Gamma}{c} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

Also, for dilute solutions:

$$P = RTc, \quad \text{or} \quad dP = RTdc \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

Substituting equations (4) and (5) in equation (3) we have at once the required formula:

$$\Gamma = -\frac{c}{RT}\frac{d\sigma}{dc}.$$

Experimental Investigations of Gibbs' Expression

The great majority of the cases of surface-condensation with which we are familiar refer to effects produced at the surfaces of solids. Apart from the question of whether such effects are true adsorption effects or not, it is evident that the results obtained cannot be used to verify Gibbs' expression simply owing to the fact that it is impossible to measure the interfacial tension between a solid and another phase. To investigate the expression it is essential that the adsorption process take place at the interface between two liquids, or between a liquid and a gas, as it is only at such interfaces that the tension σ is directly measurable.

The earliest measurements of this nature are those of Milner (*l.c.*), who succeeded in showing that aqueous solutions of acetic acid and sodium oleate exhibit positive adsorption

at the liquid-air interface, and further, that these substances possess the property of lowering the surface tension of the solution. Milner found, however, that the quantity adsorbed, Γ, exceeded that calculated, approximately ten times. Milner's measurements, therefore, only qualitatively bear out the validity of Gibbs' generalisation. Prior to the publication of Milner's measurements, an investigation of Gibbs' expression had been begun in Donnan's laboratory, and a short account will be given of the main results obtained by Lewis (*Phil. Mag.*, April 1908 and April 1909), and by Donnan and Barker (*Proc. Roy. Soc.*, A. **85**, 557, (1911)). Lewis's measurements deal with adsorption at a liquid / liquid interface; those of Donnan and Barker at a liquid / air interface.

Capillary Adsorption at a Liquid/Liquid Interface

The systems investigated consisted of a series of aqueous solutions in contact with a large surface of hydrocarbon oil. The solutes employed were sodium glycocholate, congo red, methyl-orange, caustic soda, silver nitrate, potassium chloride, barium chloride, copper chloride, and caffeine. All these substances were found to lower the interfacial tension oil/water, and all should therefore be positively adsorbed. The first four of these substances exerted a very marked depressing effect upon the tension compared with the others. By way of illustrating the depression of interfacial tension and the mode of calculating the theoretical adsorption, some details will be given of the behaviour of sodium glycocholate. The following table contains some of the values of the interfacial tension:

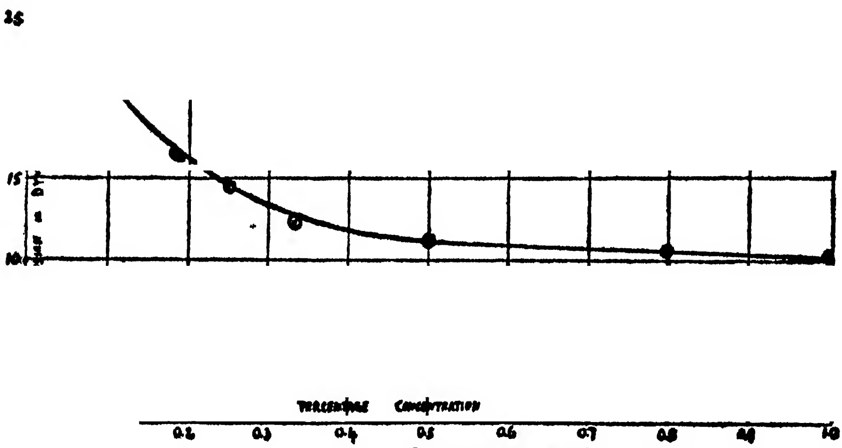
Table 1

Percentage concentration of sodium glycocholate.	Interfacial tension in dynes per cm.
0 (water alone)	48
0·0312	30·38
0·0625	26·35
0·125	19·39
0·165	16·37
0·200	14·73
0·250	13·05
0·300	11·76
0·360	11·04
0·400	10·80
0·500	10·51

These data are plotted in fig. 1.

To calculate the adsorption in the case of a glycocholate solution the concentration of which is 0·2 per cent., it is necessary to take the tangent to the curve fig. 1 corresponding to this bulk concentration and express the result in absolute units. Thus:

$$-\frac{d\sigma}{dc}=\frac{9\cdot 5 \text{ dynes per cm.}}{0\cdot 002 \text{ grams per cm.}^3}=4750.$$



OIL A. SOD GLYCOCHOLATE A.

FIG. 1.

Writing $T=289$, and expressing R in ergs, we obtain for the expression $-\frac{c}{RT}\frac{d\sigma}{dc}$ the value, $5\cdot 5\times 10^{-8}$ grams per cm.^{2}. It will be observed that the calculated adsorption is very small. To measure such a quantity directly it is obviously necessary to employ an exceedingly large adsorbing surface of oil. This measurement was carried out (1) by forming an emulsion of the oil in the glycocholate solution, in which case the adsorption takes place at a surface of great curvature, and (2) by passing the oil in the form of droplets (1–2 mm. radius) through the solution, and estimating the change in

bulk concentration which the solution has experienced. In both methods, the diminution in concentration of the solution was determined by measuring the interfacial tension between the oil and solution, and then reading off the concentration change from the curve of fig. 1. This procedure was adopted as surface tension in the case of glycocholate solutions is a very accurate means of estimating the content of the solutions. The same is true of methyl-orange and of congo-red solutions. In spite of the delicacy of the analytical means, however, the actual change in concentration was so small that the value obtained is not accurate to more than 25 per cent. In the case of the inorganic salts examined the tension is no longer sufficiently accurate, and the change in concentration was determined by the usual chemical means. In these cases the result is only accurate to the order of magnitude.

The emulsion method and the large-drop method of determining the mass adsorbed gave concordant results. A single case may be quoted. 500 ccs. of a 0·318 per cent. solution of sodium glycocholate was vigorously shaken with a known quantity of oil until a homogeneous emulsion was produced. The average radius of the droplets was found to be $4{\cdot}25 \times 10^{-5}$ cm. The total adsorbing surface is, therefore, 31,550 cm.^{2}. By measuring the interfacial tension of the solution before and after emulsification it was found that the fall in concentration amounted to 0·023 per cent. Hence the mass adsorbed per cm.$^2 = \Gamma = 3{\cdot}6 \times 10^{-6}$ gram. When the bulk concentration prior to emulsification was 0·2 per cent., the quantity adsorbed amounted to $4{\cdot}7 \times 10^{-6}$ gram. The quantity calculated by Gibbs' equation is $5{\cdot}5 \times 10^{-8}$ gram. There is, therefore, a very great discrepancy between calculated and observed values. It was on this account, indeed, that the second method of measuring Γ directly was resorted to. With this second method, however, the observed values of Γ lay between 3 and 5×10^{-6} gram. There is, therefore, no question of the real existence of the discrepancy. With congo red, methyl-orange, and caustic soda a similar discrepancy exists, the observed adsorption being 20 to 80 times greater than that calculated. In the case of caffeine, however, at an oil surface there appeared to be approximate agreement between the results, though it is to be remembered that the change in concentration is so slight that no great accuracy can be claimed for the observed

value. In the case of inorganic salts there is approximate agreement as regards order of magnitude, but very little reliance can be placed on the result. In the case of such complex bodies as sodium glycocholate, methyl-orange, and congo red, the discrepancy may with some confidence be ascribed to the colloidal nature of the solute, producing at the interface a layer of coagulated material, a process which, of course, is not taken account of in the simple reversible surface-effect postulated by Gibbs. The behaviour of caustic soda is less clear. In some later work by Lewis (*Zeitschr. Physik. Chem.*, **73**, 129, (1910)) an adsorbing surface of mercury was employed, the mercury being allowed to fall through the solution in a fine spray. To avoid the disturbance due to dissolved oxygen, the solvent was made up of 80 per cent. water and 20 per cent. alcohol. Employing a very dilute solution of aniline (0·007 per cent.), the calculated adsorption amounted to 1×10^{-8} grams per cm.^{2}, whilst that found varied from 2 to 3×10^{-8} gram/cm.^{2}; that is, there is approximate agreement. Measurements were also carried out with sodium glycocholate at the mercury/solution interface, and again, as in the case of the oil/solution interface, the observed adsorption exceeded that calculated about 25 times.

In general, therefore, the conclusion arrived at as regards the liquid/liquid interface is simply this: certain substances of a colloidal nature exhibit adsorption much in excess of that calculated, whilst simple non-electrolytes such as aniline and caffeine, as well as inorganic salts in aqueous solution, show agreement as regards the order of magnitude. It is evident that much work still remains to be done before the phenomenon can be regarded as properly understood.

Capillary Adsorption at a Liquid/Air Interface or Surface

Donnan and Barker (*loc. cit.*) were the first to investigate quantitatively the adsorption at the free air surface of an aqueous solution. The solute chosen was nonylic acid, a substance which possesses the property of markedly depressing the surface tension, and is at the same time sufficiently simple in chemical constitution that abnormal effects due to the colloidal state may be safely assumed to be absent. The value of the surface tension was employed in this case also as the analytical

means of estimating the change in concentration of the solute. The following table shows the surface tension as a function of concentration :

TABLE 2

Concentration (c) grams in 100 grams of solution.	Surface tension, in dynes per cm.
0·000	72·91
0·000977	71·83
0·00243	67·97
0·00500	57·33
0·00759	50·24
0·00806	49·09

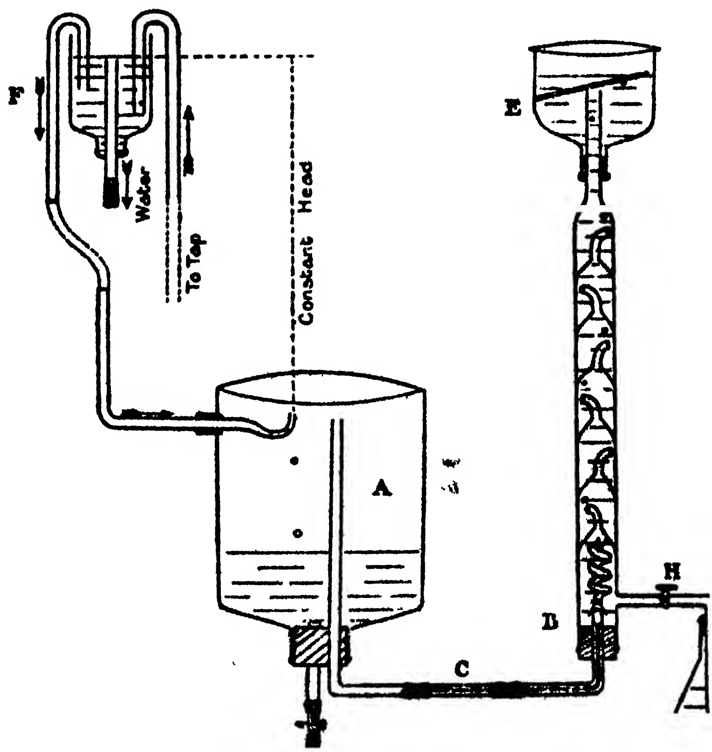


FIG. 2.
[From the paper by Donnan and Barker.]

The adsorption process was brought about by allowing a steady stream of air bubbles to pass up through a vertical tube containing the solution into an overhead reservoir. Mechanical stirring or mixing of the solution was avoided by the use of a large number of diaphragms through which the

bubble travelled, carrying with it the adsorbed layer of solute. The apparatus is shown in the accompanying figure (fig. 2). The total adsorbing surface exposed was obtained by knowing the total amount of air which had come from the reservoir and the total number of bubbles passed through the column. The degree of accuracy is considerably greater than in the experiments at the liquid/liquid interface. Donnan and Barker estimate that the change in concentration of the solute produced by the passage of the bubbles is accurate to about 6 per cent. We now turn to the calculated adsorption, viz. $-\frac{c}{RT}\frac{d\sigma}{dc}$. In evaluating this expression, the numerical value attached to R must be that for one gram of the solute, and this is obtained by dividing the molar value of R by the molecular weight. If we use the formula weight, we assume that nonylic acid is completely undissociated; but at the great dilution employed it may actually be nearer complete dissociation. In other words, van 't Hoff's factor i changes from 1 to 2. The results have been calculated on both assumptions, and the following table indicates the agreement between calculated and observed values :

TABLE 3

Concentration of nonylic acid.	$\Gamma \times 10^7$ observed.	$\Gamma \times 10^7$ calculated for $i = 1$	for $i = 2$
0·00243	0·95	0·55	0·26
0·00500	1·52	1·14	0·57
0·00759	1·09	1·26	0·63
0·00806	0·915	[1·63	0·81]

In view of the difficulties involved in measuring Γ directly, the agreement is very good, and the results may be regarded as a quantitative confirmation of Gibbs' expression. The same authors investigated the adsorption of saponine at the air surface and found approximate agreement between calculated and observed values, though, owing to the peculiar behaviour of saponine in other circumstances, the conclusion is less certain than it is in the case of nonylic acid.

Preferential or Selective Capillary Adsorption

This phenomenon is met with in the case of the simultaneous adsorption of two or more substances. If two substances are competing for a surface, it is evident that the resultant effect

can hardly be the sum of the two separate effects, since that substance which is most active in a capillary sense will tend to displace the other to a certain extent, and thus be preferentially adsorbed. It is to be expected that a similar state of things will exist when a partially dissociated salt is adsorbed. Qualitatively speaking we are familiar with this phenomenon in the removal of certain adsorbed materials by the addition of other substances, but no quantitative measurements have been carried out up to the present time.

Electro-capillary Adsorption

As already mentioned, the existence of a potential difference across the interface will necessarily modify the adsorption of electrically charged material, *e.g.* ions. This electrical modification of the adsorption process has been dealt with by Gibbs. His result may be reached by a simpler method. Let us consider a simple binary salt such as sodium chloride in aqueous solution, in contact with a second liquid, say, oil, which is electrically charged with respect to the solution. The result is, that one of the ions is attracted towards the surface, the corresponding ion following the first, so to speak, both together producing a small double-layer condenser at the interface. If a and b are the electro-chemical equivalents of the anion and cation respectively, whilst π is the potential difference at the surface, and σ is the surface tension, it can be shown by thermodynamic means that the adsorption of the ions is given by the expression:

$$\Gamma_{\text{cation}} + \Gamma_{\text{anion}} = -(a+b)\frac{d\sigma}{d\pi}$$

If, further, allowance is made for the ordinary capillary adsorption of the salt, we obtain:

$$\Gamma_{\text{salt}} + \Gamma_{\text{cation}} + \Gamma_{\text{anion}} = -\left\{\frac{c}{RT}\frac{d\sigma}{dc} + (a+b)\frac{d\sigma}{d\pi}\right\}$$

This expression is supposed to take account of the capillary and electro-capillary effects together. It is doubtful, however, whether this is a complete statement of the problem. No experimental test of this expression has yet been undertaken. The difficulty is to separate out the purely capillary part of the

adsorption, from the electrocapillary part. From some rough calculations made by the writer (*Zeitsch. Phys. Chem.*, **78**, 129, (1910)) it appeared that the electro-capillary term was approximately one-tenth that of the capillary.

A further point in this connection may be mentioned. It is well known that, on plotting potential difference against surface tension, when the applied P.D. opposes the naturally existing one, the tension passes through a maximum, which possibly coincides with the removal of the charge. At the maximum point the value of $\frac{d\sigma}{d\pi}$ is zero, and hence under these conditions we are necessarily dealing with a pure capillary effect. No attempt, however, has been made to investigate the adsorption under such conditions, and indeed the experimental difficulties would appear to be well-nigh insuperable. Our knowledge of adsorption must therefore be regarded at the present time as limited to purely capillary effects. The progress which has been made, though not extensive, is of very considerable importance, not only for the theory of capillarity itself, but equally so for physiological and biochemical problems.

PALÆONTOLOGY, ITS AIMS AND METHOD

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Palæontology is, by definition, the study of the remains of living things found buried in the rocks. Dealing as it does with such material, which, also by definition, falls within the scope of the biologist, it is not surprising that on many occasions and by many able men its claims to distinction have been dismissed, and it has been treated as a subordinate and inferior branch of the larger science. Such treatment, however, although it has been advocated by distinguished palæontologists, is unfortunate; the discovery by Wm. Smith that "Strata could be identified by the organised fossils which they contain," one of those great fundamental truths that we owe to British science, gives to our subject an obvious and fundamental connection with stratigraphical geology, and subsequent events have shown the light which a study of fossils may throw on the conditions under which rocks were laid down, and on the geographical changes which are associated with their origin. This important bearing of palæontology or geology has often been advanced as the basis of its claim to distinction. Such evidence is, however, not a sound foundation. Whole regions of chemistry and physics have an equally important bearing on geology, but are not on that account raised to independent rank. Recognition of a science as an independent branch of study must depend on a fundamental, philosophical method peculiar to it. From a practical standpoint it is now usual to divide the subject into a botanical and a zoological half, but such division is purely an artificial one, and the philosophical method of each is similar.

If we consider the history of palæontology we find the rapid establishment of two divergent interests, one, which in the early days was largely confined to those who studied vertebrates, being concerned solely with the morphology of the animals or plants with which it dealt; the other, pre-

dominantly held by men who were primarily stratigraphers, using their fossils merely as dating objects for the rocks in which they are found. The latter form of study had, until recently, no philosophical background; it was purely empirical, arguing solely from observed occurrences of individual types whose history was not only unknown but not even inquired into.

The morphological side of palæontology very rapidly acquired a distinct meaning; in the hands of Owen it was used to support, and to be in its turn assisted by, the "Archetypal theory." By the use of this theory, which unconsciously at first, but knowingly in its later and degenerate days, was distinctly evolutionary in its outlook, Owen was led to the correct appreciation of many homologies, to the distinction between homologous and analogous structures, and to an understanding of relationships which is often so accurate and so in advance of his time as to be uncanny.

The coming of evolution and its general acceptance gave a new meaning and a new interest to the many facts collected by Owen in relation to his archetypal theory; they fell at once into place as the evident result of evolution, whatever its method.

The first palæontologist to adopt a definitely evolutionary outlook in his work was A. Gaudry; but the full development of a philosophical method based on the fact that every organism is the modified descendant of a long line of ancestors was due to W. Kovalewsky, Professor of Palæontology in Moscow, who by this great achievement established an irrefragable claim to be called the second founder of this science.

Reduced to its ultimate terms the method of Kovalewsky is this:—Palæontological material to be valuable must be of a special kind; it must consist of a series of forms which either actually stand to one another in the relation of parent and child or are close blood relations belonging to different generations. Direct comparison of such forms with one another will show differences which may be due either to the fact that the animals compared are not really parent and child or to the evolutionary change which has taken place between the generations studied. It is the business of the student to discover the real direction of the evolution and finally to sum up his work by the construction of a genealogical tree.

Such is the primary aim of palæontology, the tracing of lines of descent. The same object lies before the taxonomist, but difference of material leads to a corresponding diversity in method. The two students differ, as do the historian who has as his material documents the age of which is known and the modern social anthropologist who must reconstruct the past from the actually existing organisation, customs, and traditions of his people.

The historian and the palæontologist by their direct and contemporary evidence have certainty of order so far as their material extends, and an abundance of detail; the anthropologist and the taxonomist can only reconstruct the broad outline of history and are at best uncertain of the relative periods at which changes in structure, of society, or of an organism respectively have come about.

Were the preserved contemporary documents complete, covering the whole field of study, the construction of a history would be a simple process; but their fragmentary nature, with whole periods unrepresented by documents, and many even of those which have been spared to us mutilated, renders their direct interpretation difficult.

The incompleteness of individual items cannot be overcome—we have to take them as we find them; but for the filling in of the gaps we can fall back on the methods of the anthropologist or the taxonomist.

These methods depend on the fact that indelibly impressed in structure are the traces of former conditions. The full realisation of the truth of this doctrine, so far as it concerns biology, was due mainly to the work of two palæontologists, A. Hyatt and L. Dollo, to the latter of whom we owe its clear statement and a demonstration of the possible extent of its use.

The method of Alpheus Hyatt depends on an application to fossil material of the theory that "ontogeny repeats phylogeny," the view perhaps first stated by Louis Agassiz, that an animal in its youth repeats the adult conditions of its ancestors. When applied, as it always has been by embryologists, to the reconstruction of structures occurring in very remote ancestors, this theory has seldom proved useful (its most striking success being Reichert's theory of the mammalian auditory ossicles), because early stages are usually very

much obscured, partly by the thrusting back into them of structures which really belong to much later times, but which by their actual physical bulk require a lengthy development, and partly by modifications imposed by functional needs of the growing organism.

The palæontologist uses this recapitulation only to bridge over slight gaps; the stages he uses, living under similar conditions to the adult, are free from special embryonic features. In most cases the characters used are not in all probability of much functional importance to the animals, being generally ornamental structures.

The study of phylogeny is beset by many difficulties and pitfalls. The determination of the actual relationship of the two animals, whether they be closely allied or remotely related, is seldom easy, resemblances of many distinct kinds having to be sorted out and differences evaluated. The experience of a century of study has shown that in the case of distantly allied forms gross resemblances, those which are obvious to all, are usually misleading, because owing to their very patency they have come into direct contact with the organism's environment and have been modified in adaptation to it in similar ways in diverse phyla. Palæontologists have thus been thrust into a thorough study of detail, an investigation far more precise and deep-seated than any undertaken in pre-Darwinian days; they now look for significant resemblances in those inconspicuous structures often of vital importance to the animal, which by their deep-seated nature may be supposed to be protected from adaptational change. These characters, however, by their very nature are common to all individuals of great groups and cannot serve for the finer separation of the members of the smaller twigs of a phylogenetic tree; for such distinctions other characters, also not likely to be affected by adaptation, must be chosen, and the very fineness of the divisions they are to be used in making renders it necessary that they should not be of any great importance in the animal's life.

The study of phylogeny, therefore, demands the searching out of deep-seated structures essential to the animal's life for the distinction of the greater groups, of the patent features of the animal and the investigation of its adaptations for the further sub-division of these groups, and finally of the small, non-adaptive details of its make-up for the disentangling

of those distinct but nearly allied phyla which are the smallest groups we need recognise.

The phylogenies so constructed, broken and incomplete as they must always be, and inaccurate either in grand features or in detail as they are, make closer and closer approximations to the truth as reliable material becomes more abundant and as the methods of its study become more refined.

Such histories, whose formulation is the primary aim of the palæontologist, may be used in many ways. They serve as a check to evolutionary theories, bringing them to the bar and confronting them with new series of the actual facts which they are intended to explain. They enable us to see and study the gradual improvement of an animal mechanism; to trace how, whilst retaining its capacity for performing its function at every stage, it gradually changes until it may be adapted to some quite new and highly special purpose.

The many phylogenies which have been studied by palæontologists during the last thirty years, whether they were actually expressed or remained implicit in the published work, have many features in common, features which distinguish them from the early efforts of zoologists and which remained unsuspected until they were abundantly revealed by palæontological evidence.

Palæontological phylogenies are of the most diverse kinds: they may represent the real development of some very restricted group, or they may deal in a broad way with the development of orders and superorders. They have been investigated in all groups of animals both vertebrate and invertebrate.

The fact that students of echinoderms and vertebrates produce phylogenies of similar type, and find that these agree in character with those which represent the histories of groups of cephalopods and brachiopods, shows that the underlying factors which have determined the evolution are similar in all groups. All modern carefully investigated phylogenies founded on palæontological data agree in the following broad features:

1. That in any one line evolutionary change, especially of those regions of the body which do not seem to show adaptation to any special mode of life, proceeds steadily, no matter what the stock's changes of habit, as if from the first devoted

to the production of a definite final structure, and that it is hence legitimate to speak of an evolutionary trend.

2. That an evolutionary trend is irreversible. Once committed to a course of evolutionary change a stock must follow it to the end, and as it does so its power of giving rise to branches with diverse trends becomes more and more reduced. In consequence of this loss of potentiality the greater groups, which are really initially distinguished only by their different trends, necessarily separate very soon after the establishment of the group from which they spring.

3. That in consequence of the imposition of a phyletic trend allied stocks pursue parallel series of changes, the accuracy of the parallel depending on the closeness of the relationship, and that in consequence, as Prof. W. H. Lang puts it, modern phyletic representations more resemble a bundle of sticks than a tree; the separate lines tend to radiate from a point and not to arise separately from an axis.

4. That the rate of parallel changes in allied stocks, and the relative rate of these changes of different regions in the same stock, may differ considerably.

5. That the mode of life is liable to complete changes and reversals: a stock may begin in the water, take to the land, launch out into the air, and then return again to the earth and even end with a life as thoroughly aquatic as that with which it began. Such an animal will retain in its structure features which it has acquired in every stage of its history, most clearly of course those of the later stages, but less and less clearly the results of the adaptations of its earlier ancestors.

6. That certain insignificant details of structure similar to those which which the Mendelian deals, and on which the systematist founds species, may persist unchanged through considerable changes of the animal's fundamental structure.

7. That certain features, to all appearance as unimportant as those which are usually regarded as only fit to separate species, not only change in definite directions but seem to retain this trend in all animals whatsoever. The classical case described by C. E. Beecher concerns ornament. Stocks which begin by being smooth, if they develop two sets of ornamental ridges which cut one another will develop tubercles at the intersections, which will subsequently become spines, only to

return to tubercles and finally to a smooth stage in the animal's second childhood.

It is not my purpose on the present occasion to discuss the bearing of these facts—for they are observed facts in spite of the necessarily somewhat mystical style of their presentation—on current biological theory. Nevertheless it will be obvious to all that they reveal the very great insufficiency of many favoured hypotheses.

The fact that the evolutionary change of any structure is not haphazard but proceeds along a definite track gives us a real foundation for the finer division of geological time on palæontological evidence. We have only to take some phyletic series which is really known, and divide the rocks in which the remains of its individuals are found into zones on the stage to which the evolution of some particular structure has proceeded. Simple as this process is in theory, it is impossible of direct practical application, because of the difficulty of distinguishing between the members of allied stocks pursuing parallel courses of evolutionary change, a difficulty which probably increases as the number of independent variables which can be observed becomes smaller, being least, though even then very serious, in vertebrates and increasing in echinoderms, brachiopods, and cephalopods down to such things as graptolites.

In practice, in most cases it is necessary to use all members of allied stocks which will differ to a greater or less degree in their rate of change. There is evidence that an animal which in any one structure has progressed more rapidly than its relatives will have fallen behind them in other directions; a horse with unusually progressive teeth, for example, may have retarded feet.

In this way, by taking many species of allied stocks found in the same bed and averaging up, first the stage of each and then that of all, we may be able to found definite time divisions, the size of which will depend on the closeness with which we restrict the animals we use to a single phyletic stock.

The more satisfactory attempts to "zone" a series of rocks do really depend on the conscious or unconscious use of this method, which is destined to play an increasing part in stratigraphical geology as the number of students of modern palæontology increases and as the determination (with greater

or less accuracy) of a miscellaneous collection of fossils ceases to be regarded as a necessary or indeed the essential function of a palæontologist.

The view of the true method of using fossils as geological time indicators developed in this essay leads to a justifiable doubt of the value of plant evidence in the discussion of the smaller divisions of geological time. With one exception no phyletic series of fossil plants, even of the broadest nature, has been established. Nothing whatever is known of the evolutionary trend of any of the plants found so abundantly as impressions in the rocks, and the number of independent variables which can possibly be recognised in such material, which alone can serve the purposes of geological work, is so small that the separation of allied stocks would be almost hopeless, even if phyletic trends could be established.

It therefore seems essential for the further development of the geological uses of fossils, as it is for their biological interest, that palæontologists should concentrate on the detailed study of zoological groups, preferably of common fossils, with the primary aim of discovering their true relations to one another, that is, of producing phyletic diagrams. The family histories so established will directly fill all the needs of the geologist, who will have the unfamiliar sensation of using evidence the nature and meaning of which he understands and the probable value of which he will have data for estimating.

All the uses of palæontology may thus be provided for by a single comprehensive study, and that this study must be founded on the recognition of the unique feature of palæontological material, that the relative ages of its subjects are known, is the only real foundation of its claim to distinction.

EVOLUTION AND MENDELISM

By R. BROOM, D.Sc.

The appearance of Darwin's *Origin of Species* fifty-six years ago is generally admitted to have been the most important event in the history of biology. Though others before him had believed in the evolution of living forms, it was not till Darwin had brought together and arranged his wondrous wealth of facts that the scientific world was convinced that whatever the cause or causes of evolution there was no longer any doubt as to the fact. Wallace and Darwin further advanced the very plausible theory that the natural selection of the varieties best fitted to survive in the struggle for existence was the chief factor in the evolution.

Many, perhaps the majority of biologists, accepted this theory of Wallace and Darwin as giving a satisfactory explanation: others, while accepting the truth of evolution, felt that though natural selection had undoubtedly played a part, there must have been some other agency. Of these latter Cope was one of the most prominent, and in his book *The Origin of the Fittest* he endeavours to get at the causes of the fitness that is selected.

In the last thirty years there has been very little advance made in our knowledge of the causes of evolution, but our knowledge of the facts of biology has increased enormously.

The embryological history of most of the principal living types is now fairly well known; while palæontology can now give a moderately satisfactory view of the types of animals, and to a less degree of the plants, which flourished in the various geological epochs.

New fields have been opened up by experimental embryology, and the minute study of the animal cell and the changes that take place during fertilisation and cell division has greatly added to our knowledge.

Since the beginning of the twentieth century, perhaps the most important biological work that has been done has been

that of the disciples of Mendel, who by cross breeding closely allied varieties of animals and plants have shown how parental characters are rearranged in the descendants. The Mendelians have undoubtedly thrown more light on the nature of heredity than all the earlier investigators together, and the facts they have revealed are of the utmost importance not only to the horticulturist and the stock breeder, but in showing how it may be possible to eliminate certain defects, and foster desirable qualities in the human species.

In his addresses as President of the British Association at Melbourne and Sydney in 1914, Prof. Bateson reviewed some of the more striking results obtained by the Mendelians, and the bearing of the discoveries on human progress. The Melbourne address was devoted mainly to the relations of Mendelian facts to evolutionary theories, and many of the statements made are so startling and so opposed to views that have been very largely held in the past that one feels somewhat bewildered.

As is well known, the Mendelians, by cross breeding two varieties of a species which differ in regard to a certain character which is being studied, find that though the character may not be manifest in the resulting offspring it reappears in a certain proportion of the next generation; and further that a definite proportion of this last generation breed true as regards the characters of the original parents experimented on. From this it is assumed that the characters of any plant or animal are due to certain genetic factors which are present in the germ cells, and that if the form could reproduce itself asexually or if the two sexes were perfectly similar as regards their genetic factors, each generation would be like the previous one.

It is accepted as an "essential principle, that an organism cannot pass on to offspring a factor which it did not itself receive in fertilisation," and also that "parents which are both destitute of a given factor can only produce offspring equally destitute of it." How then, it may be asked, can new forms arise? On this point there does not seem to be complete agreement among Mendelians. Lotsy believes that all new forms are the result of crossing, and even goes the length of suggesting that the first vertebrate arose from the crossing of two invertebrates. Bateson, while he does not disguise his

sympathy with Lotsy, believes that new forms may arise by the releasing of characters hitherto suppressed, as will be referred to presently, but he will not admit the possibility of any gradual modification of a species by the response of the organism to external agencies.

Darwin he dismisses in a few words. "We go to Darwin for his incomparable collection of facts. We would fain emulate his scholarship, his width and power of exposition, but to us he speaks no more with philosophical authority. We read his scheme of evolution as we would that of Lucretius or of Lamarck, delighting in their simplicity and their courage." Bateson admits, as every one must, that natural selection has played a certain part in evolution, but he is very doubtful about its being more than a secondary factor. He is "even more sceptical as to the validity of that appeal to changes in the condition of life as direct causes of modification, upon which latterly at all events Darwin laid much emphasis." A belief held by Darwin and Huxley, and strenuously maintained by Herbert Spencer and Cope, and which no one has ever disproved, may of course be erroneous, but can hardly be dismissed thus lightly on the strength of experiments which have little or no direct bearing on the question.

Other views very generally held he brushes aside with equal confidence. "We have done," he says, "with the notion that Darwin came latterly to favour, that large differences can arise by accumulation of small differences. Such small differences are often mere ephemeral effects of conditions of life, and as such are not transmissible." I do not know for whom Prof. Bateson speaks, but there are certainly still many who hold that modern research has abundantly proved the truth of Darwin's view that evolution has unquestionably been the result of the accumulation of small differences.

But let us look a little further at the suggestions Prof. Bateson has to offer us in exchange for the old-fashioned views of Lamarck and Darwin. "This is no time for devising theories of evolution, and I propound none. But we have got to recognise that there has been an evolution, and that somehow or other the forms of life have arisen from fewer forms; we may as well see whether we are limited to the old view that evolutionary progress is from the simple to the complex, and whether after all it is conceivable that the progress was the

other way about. I ask you simply to open your minds to this possibility. It involves a certain effort."

Bateson considers that there is no evidence that changes ever take place by the addition of factors, but that there is satisfactory evidence that new forms have arisen by loss or fractionisation of factors. " If then," he says, " we have to dispense, as seems likely, with any addition from without, we must begin seriously to consider whether the course of evolution can at all reasonably be represented as an unpacking of an original complex which contained within itself the whole range of diversity which living things present." As an example of this theory of unpacking he gives us the case of cultivated apples. " When the vast range of form, size, and flavour to be found among the cultivated apples is considered it seems difficult to suppose that all this variety is hidden in the wild crab-apple. I cannot positively assert that this is so, but I think all familiar with Mendelian analysis would agree with me that it is probable, and that the wild crab contains presumably inhibiting elements which the cultivated kinds have lost." The factors for the new forms have apparently been in the ancestors for countless generations, but kept down by other factors and only released when these others are by some agency removed. The fineness of merino wool, the multiplicity of the quills in the tail of the fantail pigeon, the scents of flowers and fruits are given as examples of releases of the factors which produce these results.

But still more startling is the statement with regard to the artistic faculty. " I have confidence," he says, " that the artistic gifts of mankind will prove to be due not to something added to the make-up of the ordinary man, but to the absence of factors which in the normal person inhibit the development of these gifts. They are almost beyond question to be looked upon as releases of powers normally suppressed." We have been told that no organism can hand on any factor which it did not itself receive in fertilisation, from which it necessarily follows if no new factors can be added that the artistic factor must have been present in man's ancestors—the anthropoid ape, the labyrinthodont, and the fish. Perhaps it is the presence of this artistic factor that accounts for the marvellous beauty of the Radiolaria and many of the Foraminifera! The old belief in teleology which Prof. Bateson holds up to

ridicule seems to me quite as worthy of credence as the view that the factor for the fineness of merino wool was present in the protozoan ancestor of the sheep.

While every one must welcome the brilliant and most important work being done by the Mendelians and cytologists, which has given us so much new light on the nature of heredity, we cannot admit they have helped us much to an understanding of the processes of evolution. They have shown us some reasons why each generation resembles the previous, but they have not thrown the faintest ray of light on the problem of why it is, though there is no manifest difference between two succeeding generations, that if we take the first and last of 10,000 or 100,000 generations, the differences are very appreciable. They even go the length as Prof. Bateson does of denying the fact, though the fact is beyond question.

Thanks to the brilliant palæontological work of Leidy, Cope, Marsh, Osborn, and others we have a very fair knowledge of the evolution of the horse, the camel, the rhinoceros, the titanothere, and of a number of other mammalian types. The experimenters discuss whether evolution took place by loss of factors, or by cross breeding, by slow changes or by rapid leaps: the palæontologist shows how it did take place and demonstrates that the evolution was gradual as held by Darwin, notwithstanding the remarks of Bateson.

When Marsh first called attention to the three or four most striking stages in the evolution of the horse, one might perhaps fairly have argued that the stages were too few to prove much; that there was no evidence that a *Mesohippus* had not more or less suddenly arisen from an *Eohippus*; and that there was no clear evidence of any gradual alteration. Now, however, all this is changed, and the difficulty is to define the limits of a genus like *Eohippus*, or of a species like *Mesohippus Bairdi*. The genera and species pass almost imperceptibly into others. The small low-crowned molar of the early *Hyracotherium* has slowly and steadily through perhaps 3,000,000 years evolved into the large complicated grinder of the modern horse. Are we to believe that this was because *Hyracotherium* had in it the factor for producing a horse-like molar?

Cope has shown that the *Phenacodus*-like molar is the ancestral type from which all ungulate grinders are derived. Must we believe that the small *Phenacodus*-like form which

was the common ancestor of all ungulates had not only the factor for producing horse-like molars, and the factors for all the intermediate stages, but at the same time the factors for producing molars such as are met with in the ox, the rhinoceros, the titanothere, the tapir, and the elephant ? Had it also the factors for the antlers of the deer, for the trunk of the elephant, and for the loss of the hind limbs in the dugong, in addition to the factor for the fineness of merino wool ?

The old views of Lamarck and Darwin may require slight amendment here and there, but they certainly have too much established truth to be ever altogether set aside. I quite agree that those zealous ultra-Darwinians who have endeavoured to explain all evolution by the working of natural selection have done much to discredit the theory. But apart from the undue importance placed on natural selection by Darwin and his followers, there is no doubt that most of Darwin's work will stand the test of time.

Lamarck appears to have been the first scientist who clearly recognised the importance of the part played by the use and disuse of organs in the modification of animal types ; and a large number of workers since his time have agreed that in function we have a prime factor. Darwin considered it played a secondary part, and there have been those who have argued that it played no part at all. Even now there are many who hold, with Bateson, that the actions and habits of an animal cannot produce any changes which can be inherited by the offspring. They are willing to admit that the increased use of a limb will result in the increase of the muscles and of the strengthening of the bone in the individual, but they refuse to admit that the next generation will be influenced even in the slightest degree by the action of the parent. I do not know what are Bateson's reasons for refusing to admit that acquired modifications can be inherited, but it has long seemed to me that the arguments of the opponents of the theory amount to this, that they cannot see how the sexual elements of an animal can be influenced by the habits of the animal, therefore they cannot be.

Those of us who hold that the actions of an animal do influence the next generation do not undertake to prove it experimentally. If it took 3,000,000 years or 1,000,000 generations to evolve the molar of the horse from the molar of *Hyraco-*

therium, one need hardly expect to be able to demonstrate any perceptible change by experiments in a human life-time. Nor are we able to say how the offspring can be influenced. But we do say that the evidence is quite conclusive that it is influenced.

In the evolutionary series of the horse we see the gradual increase in size of the middle toe and the gradual dwindling of the side toes. It has been very plausibly argued that the middle toe has increased through nature favouring those forms in which it is better developed, and less plausibly argued that the side toes have dwindled and become lost through nature eliminating those types in which the side toes proved a slight handicap as against others in which the side toes were even more reduced. But if we consider all the change that has taken place in 1,000,000 generations it will readily be seen that at no time has nature ever anything very tangible to select. And as we have the clearest evidence that certain changes could not have been produced by natural selection we are probably justified in doubting if any have been.

It is well known that underground and cave-dwelling animals have usually small eyes or have entirely lost their eyesight. We cannot of course demonstrate an evolutionary series showing all the stages by which the eyesight has become lost in *Notoryctes* or *Chrysochloris*. But by examining other animals of somewhat similar habit we see various stages in the reduction of the eye such as may have been passed through. In *Georychus* the eye is small; in *Talpa* it is still more reduced. In *Chrysochloris* it is quite under the skin and pretty certainly functionless. In *Notoryctes* only a rudiment is left. The old natural selection arguments brought forward to account for the reduction of the eye of the mole are seen to be of no value when we consider the problem of the further reduction of the functionless rudiment of the eye deep below the skin.

In all the divisions of the vertebrates we have examples of increased development with increased function and reduced development with lessened function. But one of the most striking examples is the reduction in size of the wing in birds which have ceased to fly. It matters not whether the bird is a rail, a pigeon, or a goose, if it takes up its abode on an island where it is free from ground enemies it no longer requires to fly, and as a result of its gradually ceasing to use its powers of

flight, the wing and its muscles gradually become reduced. Sometimes the reduced wing, though no longer useful for flying, may, by taking up other functions, be preserved, but if it does not the wing becomes more and more rudimentary. We know from the work of Jeffrey Parker that the struthious birds had flying ancestors. In the ostrich the wing, though no longer useful for flying, is still retained of fair size for other purposes, but in *Apteryx* and the moa it is quite rudimentary.

We see clearly the increased development of a part with use and the reduction and elimination with want of use, and we might at first readily assume that the modification is the direct result of the function, but there are good reasons to believe that this would not be quite a correct statement of the case, for even after an organ has ceased to have any function the rudiment still continues to decrease, and in the development of tooth cusps and many other structures we notice the increase taking place before the parts can be functional. We are therefore driven to believe that increase and decrease of parts are due to augmented or lessened stimulation.

I think we may safely conclude that evolution as we see it in the animal world, and most probably also in the vegetable kingdom, has been due to responses of the organism to changes in stimulation. The part played by natural selection has been the elimination of those types which have been unable sufficiently to respond.

I shall not in the present paper discuss how the organism responds to various stimuli, nor state what seems to me at least a plausible theory of how even slight changes in the parent may affect the germ cells, but of this I feel confident, that no theory of evolution by changes of stimulation, even though it requires the inheritance of acquired characters, will ever make such demands on human credulity as the theory which suggests that all characters seen in all living organisms of to-day, including the artistic faculty and presumably poetic genius, were present as factors in the Protistan germ from which all have been descended.

WHAT IS A DISEASE?

By CHARLES MERCIER, M.D., F.R.C.P., F.R.C.S.

A GENERATION or two ago it used to be a favourite dodge of counsel to begin their cross-examination of a witness who claimed to be an expert in mental diseases, by asking him to define insanity. The dodge was well calculated to confuse the witness, and to take the conceit out of him; besides giving to a not too intelligent jury the impression that a person who could not define his subject could not be an authority upon it; but if the witness had had the astuteness to turn the tables, and ask his cross-examiner for a definition of law, counsel would have been equally confounded. It is, in fact, evident, and it is remarkable, that the fundamental concepts in every calling are the most difficult to define, and that the definition of them is not arrived at until late in the history of that calling; and it is even more remarkable that the want of valid definitions of fundamentals is for a long time but little felt, and does not seriously interfere either with practice or with the advance of knowledge.

If a committee consisting of the Lord Chancellor and all the judges were appointed to draw up a definition of law it is very unlikely that any two of them would agree; but the absence of a definition does not embarrass the administration of the law. It would be as hard to find an engineer who could frame a good definition of matter and force as to find another engineer to agree with him; but the want of such a definition does not hinder engineers in building bridges or devising machines. No soldier can define war, no parson can define religion, no financier can define finance, nor any merchant what is meant by commerce; but yet soldiers can make war, parsons can preach religion, financiers can raise loans, and merchants can trade, without ever feeling the want of these definitions. It is not surprising, therefore, that doctors have formulated no definition of what is meant by a disease, nor that the want of such a definition has not embarrassed the

practice of medicine. In every calling, however, a time must at length come when a definition of its fundamental concepts is needed ; it may be to divide it from other callings ; it may be to settle some question that lies near the boundary line ; if may be for some purpose foreign to the calling itself. Such a time is now arrived in the history of medicine.

Owing to enactments casting upon other than injured or sick persons the burden of their treatment and maintenance, and of compensating the dependants of those who are killed, there has grown up a great amount of litigation about medical matters ; and as all decisions in law turn upon the meaning of some word of phrase, it is not surprising that much of this litigation turns upon the meaning of such words as injury, accident, disease, functional disease, organic disease, imaginary disease, and so forth. It was natural that law should turn to medicine for definitions of these terms, terms that are used daily and hourly in medicine, and without which the science and art of medicine could not be carried on ; and law must have been considerably surprised to find that medicine has been using these terms for centuries without any clear formulation of their meaning, a surprise which was no doubt none the less for the fact, which law did not recognise, that some of its own fundamental terms, such as motive and intention, were equally wanting in definite formulation. The time is now come when some definition of the fundamentals of medicine must be found. The definitions that I shall propose may not be perfect, but at least they do render more clear the fundamental concepts of medicine that are at present nebulous and inchoate, and they are supported by reasons that it is open to any one to question and refute if they are unsound. From the tyro to the veteran, every medical practitioner speaks and thinks scores of times every day of symptoms and of diseases, and has some vague notion in his mind that there is a difference between a symptom and a disease ; but what that difference is, no doctor can tell. I think this inability is a reproach to medicine, and I shall try in what follows to remove it.

The human body, as conceived by the physician and the surgeon—the alienist has, or ought to have, a very different concept of it—is a very complex mechanism, composed of many parts, each of which performs a certain duty towards

the whole, and a certain duty towards itself. These duties or actions are called the *functions* of the part, and according as the duty is well or ill performed, the function is normal, or defective, or disordered. The duty towards the rest of the body, which may be termed the extrinsic function, varies extremely with the nature of the part; but the duty towards itself, which may be termed the intrinsic function, is the same in every part, and consists in making good the waste consequent on the performance of its extrinsic function, in repairing any structural damage that may be inflicted upon it, and in repelling the attacks of injurious agents, whether chemical, microbic, or cellular.

It is often debated by novices in medicine—of established practitioners few have the time, and fewer still the inclination, to ponder general questions—but by novices in medicine the question is often debated whether disorder of structure precedes disorder of function, or *vice versâ*. Those who discuss this matter mean by function the extrinsic function only, and whichever conclusion they arrive at will not hold good when intrinsic function is taken into the consideration. If the intrinsic function is disordered or defective, the structure must fall into disrepair, and be unable to perform with full efficiency its extrinsic function; so that in such a case, while disorder of (intrinsic) function precedes disorder of structure, disorder of structure precedes disorder of (extrinsic) function. Even with respect to extrinsic function alone, the answer must depend on the meaning that is attached to structure. The (extrinsic) function of a part is seriously interfered with or abolished if the blood supply of that part is seriously diminished or abolished; but the structure, in any usual, and I think proper, meaning of the word structure, cannot be impaired, for the function is resumed as soon as the blood supply is re-established.

However this may be, it often happens that from one cause or another the function, either intrinsic or extrinsic, of a part of the body is disordered or defective. It is imperfectly performed; and when this happens there is usually, perhaps always, some sign or manifestation of the altered performance of function. The sign may be perceptible to the patient alone, as in the case of pain or deafness, or dimness of vision, or a whoreson tingling; or, like coma, it may be perceptible to

bystanders only; or, like a cardiac murmur, or fluctuation, or optic neuritis, it may not be perceptible except to the skilled examination of the physician; or it may, like cough, vomiting, swelling, and other deformity, be perceptible both to the patient himself and to others. It may be, like a barking cough or an elephantic leg, gross as a mountain, open, palpable; or it may be, like want of sensitiveness in a pupil, or diminution of a knee-jerk, so slight that we can scarcely be sure that it is present. It may, like a cavity in a tooth or a phlyctenule on the eye, show unmistakably what function of what part is disordered; or it may, like headache, give no indication whatever; but in every case in which function is disordered, some sign or manifestation of the disorder is perceptible to some one; and such a sign or manifestation is called a *symptom*. This, then, is the definition of a symptom. It is a sign or manifestation of disorder of the function, intrinsic or extrinsic, of some part of the body.

Disease is a wide and comprehensive term, covering not only all disorders of function, and all symptoms, or signs of disordered function, but also all results of disorder of function, whether the function disordered is intrinsic or extrinsic. Thus, indigestion, which is disorder purely of extrinsic function, is disease: atrophy, which is disorder purely of intrinsic function, is disease: albuminuria, which is a result of disorder of extrinsic function, is disease: a lardaceous liver or a cancerous breast, which is a result of disorder of intrinsic function, is disease: pain, which may be a manifestation of disorder of either intrinsic or extrinsic function, is disease.

So far all is plain sailing, and I do not think any one will object to anything that has been said; but when we modify the term by the addition of the article, and speak of 'a disease' we are at once entangled in a thicket of difficulties. Pain is disease; but it is not, at least it is not necessarily, a disease. Albuminuria is disease; but it is not a disease. Hæmatemesis is disease; but it is not a disease, at least it is not now so considered, though there was a time when not only hæmatemesis, but cough, hæmaturia, albuminuria, vomiting, dropsy, palsy, jaundice, syncope, and many other instances of disease that are now ranked as symptoms only, were ranked as diseases.

From this we gather, first that not everything that is

disease is a disease; second, that there is some difference between a symptom and a disease; and third, that diseases may change into symptoms. Since, however, the things themselves do not change, but a cough remains a cough; dropsy, dropsy; and syncope, syncope; the same in all material respects now that they are symptoms as they were when they were diseases, it is certain that whatever change has taken place, since it is not in the things themselves, must be in our way of contemplating them. There is no other possible way of accounting for the change of diseases into symptoms. Regarded in one way, contemplated from one point of view, an instance of disease is a disease. Regarded in another way, contemplated from another point of view, the same instance of disease becomes a symptom. To medical men, the difference that the mode of contemplating a thing makes in our estimate of that thing is not very familiar, and may appear startling; but it is quite well known to psychologists, and instances are familiar enough. That which was once a species of birds, or insects, or plants, or what not, is now a mere variety: that which was once a variety is now become a species. The birds, or insects, or plants, are not changed: even our knowledge of them is not changed: what alone is changed is our way of contemplating or estimating them. The revered philosopher of our youth is now a self-opinionated dogmatist. He is not changed, but our mode of contemplating him is changed. We may look upon the sea as a barrier between nations, or we may look upon it as a means of intercourse between nations. The sea has not changed, but our mode of contemplating it has changed. So when a disease becomes a symptom, it is not the thing that is changed: the change is solely and entirely in our estimate of it; and therefore the question, What is the difference between a symptom and a disease? is resolved into the question, What is the difference in our mode or standard of estimating an instance of disease that determines us to call that instance a symptom instead of calling it a disease?

What our method or point of view is in contemplating it as a symptom we have already discovered. In so contemplating it, we contemplate it as a sign or manifestation of disorder of function. What is the point of view from which we regard the very same occurrences when we call them

diseases and regard them as diseases? How do we contemplate them and estimate them? That is the problem that we are to solve.

Before we can attack this problem with success, we must settle certain preliminaries and clear up certain obscurities; and it will assist us in our task if we show that certain things sometimes called diseases are not diseases.

In the first place, let us note that we always regard a disease as an individual thing. It may include many symptoms, and it may include other things that are not symptoms, for instance, it may include structural change and damage in internal organs; but these are parts or factors that go to make up the disease, and what we mean by a disease is always an individual thing or whole. We speak of treating the disease; we speak of the disease being fatal; we speak of fluctuation of the disease: we speak of the disease becoming more severe, or less severe: we speak of its beginning and its ending; and in each case the phraseology shows that we regard the disease as an individual thing. The use of the article alone is conclusive of this.

It is unnecessary to show that a symptom is not a disease, is not at any rate necessarily a disease, for this is the thesis with which we started; but it may be advisable to show that structural disease, deteriorating change of structure, or structural damage, is not a disease. It is advisable to show this because we often speak as if structural damage of an organ were a disease. If we ask what disease a patient is suffering from, and are told that it is inflammation of the lung, or ulcer of the stomach, or cancer of the liver, we accept the answer as satisfactory, and are not struck by any incongruity between the answer and the question. But this is because we do not take the answer in its literal sense. We do not understand it to mean the structural damage alone. An inflamed lung, an ulcerated stomach, or a cancerous liver may exist in a dead body; may be taken out of the body and preserved in spirits; and it will be acknowledged that it would be erroneous and absurd to call the preserved organ a disease. It is a diseased organ, but it is not a disease; and if it is not a disease when taken out of the body, neither is it a disease when it is in the body. If it is not a disease after death, neither is it a disease during life. Yet we speak of ulcer of the stomach as a disease.

But when we speak of ulcer of the stomach or any other structural change as a disease, we do not mean what we say. We express ourselves inaccurately. The expression is approximate only, as so many of our expressions are. We do not regard a patient who suffers from ulcer of the stomach merely as a person in whose stomach there is an ulcer, and there end our contemplation of him. We think of him as a person who not only has an ulcer in his stomach, but also suffers the consequences and presents the symptoms of ulcer of the stomach; and it is these additions to the bare structural change that constitute, with that change, the disease from which the patient suffers. There are, in fact, two very different concepts connoted by the names of most damaging changes of structure. The name may mean the structural damage, the ulcer in the stomach, the lumps of cancer in the liver, the consolidation of the lung, and nothing more; and then it is a term in morbid anatomy. It means disease of an organ, or it means a diseased organ; but it does not mean a disease. The name may, however, be employed with the connotation, not only of the structural damage, but also of the consequences, accompaniments, and symptoms of that damage when it exists in the living body, and, so employed, the name of the structural damage does stand, or may stand, for the name of a disease.

It may stand for the name of a disease, but even when it carries the connotation of its consequences and symptoms, the name of a structural damage does not always stand for a disease, at least for the whole of a disease. In the course of acute rheumatism a valve of the heart may suffer structural damage, and this structural damage has its own group of consequences and symptoms. It allows of regurgitation of the blood into the cavity it has just left, it gives rise to a murmur, it causes shortness of breath, cyanosis, œdema, and so forth, all of which are summed up and included in the term 'heart disease'; yet this structural damage to the heart, associated though it is in our minds with its consequences and symptoms under the name of heart disease, does not constitute 'the disease' from which the patient suffers. The group of structural damage, consequences, and symptoms is an important part of the disease, no doubt, but a part only; for 'the disease' is acute rheumatism. Again, from the

valve thus diseased a vegetation may be detached, and may block an artery in the brain, causing structural damage to the brain, with its characteristic train of symptoms. The term 'embolism of the brain' has, like other names of structural change, its double meaning. It may mean the presence of the block in the artery, or it may mean this together with the consequent disorganisation of the brain, or it may mean these things together with the paralysis and other symptoms that are evidence of them; yet, even if the term is understood in the latter sense, embolism of the brain is not the disease, is not the whole disease, from which the patient suffers. His disease is still acute rheumatism, of which the heart disease and the embolism are consequences and parts. If, however, the rheumatism subsides and disappears, leaving the valve of the heart damaged, then this structural damage, plus its symptoms and consequences, becomes the disease from which the patient suffers. We find, therefore, that structural damage to an organ, together with the consequences and symptoms it produces, may constitute a disease, but does not necessarily constitute a disease.

(*To be continued in January* 1917)

SPALLANZANI

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Spallanzani's dates (1729–1799) form the *fons et origo* of many important departments of biological research. The genius of Spallanzani touched and adorned so many things that it is impossible to avoid coming constantly upon his work. But the remarkable personality of the man behind the name will possibly come as a surprise to English workers who, if tempted for once in a way to make an incursion into the field of biography, shall find their curiosity in this instance amply justified.

There is a large Italian literature about him.[1] Even in his own country and among his own friends he always was and still is regarded as a prophet and a great man, so that his fellow-countrymen have not thought it superfluous to study his life and character in the minutest details; but in the small compass of this article only the bald facts can be given.

His personality is striking. The Abbé Spallanzani was a priest and a savant, although in fact he possessed none of the characteristics one is accustomed by convention to associate with those two vocations. Greedy, ambitious, arrogant, and at times violent, Spallanzani was a bull-moose type of man who charged through life with his head down. There were many obstacles to his success, but he brushed them aside; he had many detractors, but he pinned them down. To his opponents in biological controversy he never expressed any flabby desire to agree to differ. They were attacked with acerbity, and whether right or wrong he emerged triumphant. False modesty was not one of the Abbé's faults. When, as a young man conscious of his own genius, he ventured upon a criticism of the illustrious Buffon, he did so with a sardonic expression of his own incompetence. He never showed the smallest inclination to mislead his contemporaries into giving him less than his deserts. He set out to be second to none—not even in salary—and he succeeded and was proud of it.

[1] See *Lazzaro Spallanzani*, Pavia, 1871 (Gibelli).

There is indeed a gamey flavour about Spallanzani, and it is easy to understand his popularity among his students. They must have found it invariably safe to shelter themselves, their hopes and ambitions, within the shadow of a personality so mountainous as his.

Lazzaro Spallanzani was born at Scandiano in Modena on January 12, 1729. His father, an advocate, gave him his first lessons, and subsequently he passed into the Jesuit College at Reggio di Modena with the intention, we are told, of entering that body. But as a matter of fact he passed into the University of Bologna, and thus entered upon the critical phase in his intellectual development, for his celebrated cousin, Laura Bassi, was Professor of Physics at Bologna and it is believed that her influence was the principal factor in determining his taste for natural philosophy.

By the year 1758 he had become Professor of Logic and Geometry in the University of Reggio, and in 1760 he was translated to Modena to hold the Chair of Physics. The youthful professor had already made a reputation when in 1769 he became the first to hold the newly appointed Chair of Natural History in the University of Pavia, which at the instigation of Maria Theresa, then ruling over Austrian Lombardy, was being reorganised and re-equipped.

He inaugurated his series of lectures with "an elegant Latin discourse" on the controversy between the Preformists and the Epigenists. Buffon, whose flights of imagination were well calculated to arouse antipathy in a hard-headed and prudent investigator like Spallanzani, was propagating his doctrine of "organic molecules"—a Buffon-like embroidery of the preformation hypothesis tending towards epigenesis. Spallanzani, an orthodox believer in the preformation faith, accepted it as sheer epigenesis (vide *Dissertations Relative to the Natural History of Animals and Vegetables*," vol. ii. p. 160, London, 1784), and, wielding that damaging epithet "imaginative," made battery and assault on the speculative Frenchman.

Plus ça change, plus c'est la même chose. The controversy in a more developed form continues still, and it looked at one time, before Roux's initial experiments with the frog's egg were carried further, as if the preformation idea of Spallanzani and his supporters might prove to be sound.

More than one of Buffon's claims were attacked with spirit

by Spallanzani, who placed over against Buffon's interesting speculations his own still more interesting facts obtained under conditions of rigid experiment—notably his work with hermetically sealed flasks in which he showed no life developed if they were subjected to powerful heat. Spallanzani's methods were an enormous advance upon those previously used, although they by no means set the matter at rest. The old bone of Spontaneous Generation has since been dug up many times and chewed. And it is not buried yet.[1]

Of course, Spallanzani made mistakes—indeed to his credit it might be said, if the ancient adage be true. In those days it used to be thought by some that fecundation was effected by some sort of aura or gas given off by the semen of the male, and though Spallanzani succeeded in showing that the semen itself is the responsible agent, he considered he had "irrefragably proved" the falsity of the doctrine of Leeuwenhoek and his followers, who, in a measure rightly, were advocating the "spermatic vermicelli" as the "immediate authors of generation."[2] It is permissible to feel a certain amount of sardonic satisfaction at the *ex cathedra* pronouncements the professor gave upon questions in which Time, the Enemy, has found him out. Spallanzani's loyalty to his own observations made him over-confident, too cocksure.

An incident in connection with his translation of Bonnet's *The Contemplation of Nature* is worth recording for the illumination it sheds upon his point of view in biology and in university education. Each professor was required to select a book for the use of the students, and Spallanzani's choice fell naturally on his translation of Bonnet. But this selection, on being submitted, did not meet with approval in Vienna, where ideas of university instruction in biology were diametrically opposed to those now in vogue. That is to say, great importance was attached to systematic work to the exclusion of a more philosophical treatment of the subject. The Professor of Natural History in Vienna—a man unknown to fame and the author of a single modest treatise entitled *Additamenta quædam ad*

[1] I am referring to the experiments of the late Dr. H. Charlton Bastian.

[2] *Loc. cit.* p. 159. Spallanzani aggressively claimed to have fertilised frog's eggs with seminal fluid devoid of spermatozoa. Leeuwenhoek, on the other hand, denied the ovum any important part in the formation of the embryo, apparently regarding it simply as the nidus in which the spermatozoon developed.

Entomologiam—sat in judgment upon the exasperated Spallanzani, and reported that while he admired the philosophic character of Spallanzani's selection, he did not believe Bonnet's book sufficient to give the necessary instruction in nomenclature, which was the universal language used by naturalists of many countries to make themselves and their works understood. Spallanzani's philosophic temper made him already impatient with the systematists at whom he flung the gibe of " nomenclature naturalists " ; his contumely was prodigiously increased by this obscure Viennese professor's criticisms.

On being requested, Spallanzani sat down and wrote out a reasoned programme of the lectures he intended to give on natural history. This programme amounted really to a defence of his point of view in natural history, but the higher authorities, in spite of all, were adamant, and Spallanzani was forced to come to terms on the subject of nomenclature instruction with the bribe of a promised increase of salary—always an irresistible lure to the professor.

But Spallanzani was by nature an *intransigeant*. And it is hardly probable that he would entirely succumb on a principle of such vital importance to his biological teaching. In fact there is evidence to show that, as in the early days of his professorship, he continued to demonstrate respiration in molluscs, fecundation in Amphibia, and other unorthodox matters.

To the efficiency of his lectures all his biographers bear witness. Senébier wrote : " Une éloquence simple et vive animait ses discours ; la pureté et l'élégance de son élocution séduisaient ceux qui l'entendaient." He possessed the teacher's gift of inspiring with enthusiasm both students and the men of science who came to hear him from every part of Europe. The tributes of his European contemporaries were generous without reserve. Bonnet said that he had discovered more truths in five or six years than all the academies in half a century, while " the dying hand of Haller consigned to him the defence of truth and nature."

During the first part of his residence in Pavia, he lodged in an ex-convent with Prof. Scopoli, and, although when and where is not known, he must have already taken Holy Orders, as he was accustomed to increase his income by taking Mass in a church close at hand. On quitting these lodgings he engaged some rooms in a house in the attic of which his famous

experiments on bats were carried out. The house has been identified, and in the attic some interesting relics were discovered in the strings and dried-up pipistrelles used by him in these investigations. He blinded the animals sometimes by burning the eyes with a red-hot wire and sometimes by removing the organs altogether, and even filling up the orbital cavity with wax. Notwithstanding these mutilations, the little creatures were able to fly as well as before, avoiding the walls, and the strings suspended in the path of their flight. These and other experiments led him to the conclusion that bats find their way in the dark by means of some special sense situated in an unknown organ in the head. It is now generally accepted that this astonishing faculty in bats of directing their flight is due to an exceptional development of the sense of touch, especially in the wing membranes.

Before finding fault with the brutality of Spallanzani as an experimenter, it is just to remember that his passionate curiosity led him to turn his ruthless hand even against himself. For in his Studies in Digestion[1] he describes how he swallowed bone, cartilage, and tendon concealed in perforated wooden tubes, and how, in order to obtain gastric juice for the purposes of artificial digestion, he caused himself to vomit on an empty stomach by tickling the fauces. This knowledge ought to soften the heart of the most fanatical zoophilist towards the Abbé.

In August 1779 we find him in Switzerland on a visit to his friend Bonnet at the latter's "delightful villa" at Genthod. Abraham Trembley was also present, and one likes to think of these three with heads bent and hands folded behind the back walking and talking together, each of them engaged upon researches of great moment in biology—Bonnet perhaps on his studies of asexual propagation in Aphides, Trembley on regeneration in Hydra the fresh-water Polyp, and Spallanzani occupied just then with fertilisation in toads. In Bonnet's presence he cut off the hind legs of a male toad during its embrace of the female without effecting a separation. The female, he points out, may begin to discharge eggs later, and the male with his blood flowing all the time continues to impregnate them with his semen. In reply to a question, "he did not hesitate to say that this persistence was less the effect of obtuseness of

[1] Proving the theory of digestion by solution as against the principle of trituration.

feeling than vehemence of passion." In these days of comparative psychology, the idea of a vehemently passionate toad raises a smile.

The Abbé was an enthusiastic traveller and his expeditions to the Milanese Mountains, to Marseilles, Sicily, and his visits to Vesuvius and the Lipari Isles brought in a rich harvest of scientific results. Moreover, Spallanzani by no means confined his attention to biology. He studied natural history in the broadest meaning of the term. He helped to lay the foundations of vulcanology and meteorology, he discovered the true explanation of "ducks and drakes" on the surface of water (formerly attributed to "elasticity" of the water), he experimented with the water divining rod, and by the aid of Pennet's instrument called "the Minerographico" he and Pennet claimed to have revealed subterranean currents of water in the courtyard of the University.

In 1784 Spallanzani was projecting his great journey to Constantinople and entered into a correspondence concerning it with his Excellency Count Firmian, the Austrian Minister Plenipotentiary at the Court of Milan. The professor was a past master in the gentle art of pulling strings, and he had hitherto been egregiously successful not only in obtaining permission to undertake expeditions but also in obtaining funds for them and in increasing his stipend.

Whether or not the University was at length beginning to kick against the pricks is not evident, but his proposal hung fire and the arrangements were being protracted.

It was at this juncture towards the end of the year that Spallanzani engineered a piece of admirable bluff—or, as he himself called it, a "giro politico"—by asking to be relieved of his post with the excuse that the air in Pavia was unsuitable to his health. Vienna straightway, in order "to preserve for the University a celebrated person" and in order not to prejudice the University in public opinion, promised him handsome compensation in the way of salary if he remained, and also gave him permission to go to Constantinople. And so, "the fogs cleared, the humidity disappeared, and every ill was cured, even the gout," remarks his biographer slyly.

Spallanzani stayed nearly a year in Turkey, made many valuable observations, was received by the Sultan and, on his way home overland, stopped in Vienna to be presented

by Joseph II. with a gold medal. The return home was a triumphal progress, for on reaching Pavia he was met and acclaimed outside the city gates by numbers of his students and escorted by them through the streets.

It is a well-known fact that the museum at Pavia was founded by Spallanzani. As he himself claimed, it had been born under his hands, it had grown under his direction, and owed its prosperity to his correspondence, activity, and travels. Now during Spallanzani's absence in Turkey, Canon Volta, acting as curator of the museum, made the discovery that several objects, though mentioned in the catalogue, were missing from the museum. Volta, alas! was among the few who knew that at Scandiano the professor owned a private museum. So, pretending to set out on an excursion to Tuscany, Volta went to Scandiano and, under a false name, asked to see the Spallanzani Museum. On coming out, he went straight to an inn and made a note of all he had seen. He next wrote to Counsellor don Luigi Lambertenghi in Milan informing him that the numerous objects missing from the museum at Pavia were to be found in Spallanzani's Museum in Scandiano, and that some of the objects were still marked with their original numbers, the jars for the most part having the red labels of the jars at Pavia. He requested the Counsellor to see that the Government verified his assertions. He also gave information to the Supreme Ecclesiastical Commission and the Commission of Studies, and in Pavia he talked frequently of "Spallanzani's thefts," so that the scandal soon came to be divulged.

Professors Scopoli, Scarpa, and Fontana were also drawn into the conspiracy, which went to the incredible length of sending to persons in authority, to Spallanzani's friend Bonnet, to Tissot and others, to the heads of the Italian Universities, and generally of distributing throughout the continent, a circular informing the world at large of the "unexpected," "ignominious," "atrocious" crime of their famous colleague.

The motive actuating these men was said to be envy of Spallanzani's eminence as a man of science, intensified by their fear of showing it on account of his influence at court. Probably, Spallanzani's own intolerant attitude towards his intellectual inferiors was scarcely likely to adjust matters. "What wonder," he exclaims, speaking of Pavia, "that in districts so low, so foggy, so marshy, talents are so rare."

Confronted with the charge of theft of which he was early advised, Spallanzani hurried home from Vienna. By a special decree of September 14, 1786, the Government of Lombardy was ordered to intervene. The latter sent secretly to Scandiano, where it was reported that though certain objects missing from the museum at Pavia were observed, there was no indication to show that they belonged to the museum at Pavia. An inquiry was opened at the Royal Palace of Milan, where Spallanzani's reply to the charge succeeded conspicuously. The missing birds were badly prepared, had lost their feathers and were eventually thrown away. The armadillo, the snakes, the seal, the hammer-headed shark, and the sword-fish had been given away in exchange. Other things had been used in experiments, and finally the rare Conus—" Cono ammirale "—turned up again in the museum and had never really been lost.

The Abbé preferred a counter-charge against Volta of breaking up agates and precious stones and distributing the pieces among his friends. He also showed that the curator often left things out on the table of the museum when students and workmen were free to come in and out.

A report of these lamentable proceedings was forwarded to Vienna with a letter from the President to the Imperial Chancellor Kaunitz, in which insistence was placed on putting an end to intrigues among the professors, as it created a spirit of faction among them and brought discord even among the students.

As a result of the inquiry Spallanzani was declared innocent, Canon Volta was deprived of his office as Curator of the Museum and sent away from Pavia, while Profs. Fontana, Scarpa, and Scopoli were censured " for the grave prejudice to the reputation of Prof. Spallanzani by having imputed to him without proof " so grave a charge as theft.

Spallanzani was delighted. He sent a warm letter of gratitude to Wilseck, his " great protector and great Mæcenas," who presided at the inquiry, and distributed to all the European centres of learning a circular, in reply to the one sent by the conspirators, showing how his character had been cleared.

In spite of the issue of a royal decree imposing silence upon those concerned in the scandal, the reverend Abbé was unable to restrain himself from reviling his calumniators with vitu-

peration of a kind that betrayed at least a clumsy wit. Volta was "a bladder, full of wind, an object of abomination and horror"; Scarpa was "a cabalist, one of the most inferior of scholars, a perfect plagiarist"; Scopoli was a "Physis intestinalis," this being a name published by Scopoli for a portion probably of a bird's trachea in mistake for an intestinal worm which is given all the usual honours of a figure and description in Scopoli's book, *Deliciæ Floræ et Faunæ Insubricæ seu Novæ aut minus cognitæ species Plantarum et Animalium quas in Insubrica Austriaca*, i. 1786 (p. 46). In addition to these sledge-hammer blows he also dealt out the stiletto thrusts of anonymous communications to the newspapers which have been dealt with by Prof. Pavesi in *Il Crimine Scientifico di Spallanzani giudicato* (Milan, 1899).

Some doubts, after all, of Spallanzani's integrity in the affair have been expressed. These probably originated in the fact that the professor is reported to have subsequently suppressed a part of his first memorial of defence in which he confessed that at Scandiano he kept some of the objects belonging to the museum at Pavia, but only with the idea of studying them and returning them afterwards to Pavia. His natural astuteness helped him to foresee the danger of such a confession at such a crisis.

Although this was not the only battle the Abbé fought with his aggressors, no one ousted him from his position or deprived him of his reputation. He continued to enjoy his fame and received many signal honours. He was Dean of the Faculty of Philosophy several times, and in 1778 the students by a majority of votes elected him to the Rectorship. At the museum he received many distinguished visitors, including the Emperor Joseph II. It is amusing to read that to the "gentili Signore" he was always happy to show the museum—"provided they were beautiful and intelligent." Even this granite character, perhaps, had its softer side.

Although for diplomatic reasons Spallanzani used often to complain that he was not well in Pavia, he really enjoyed a florid state of good health, and the day before he was attacked by the apoplexy which ended in his death he was pursuing with the most youthful ardour his experiments in respiration, the results of which were published posthumously. Three days after his seizure he had recovered sufficiently to be able

to recite verses from Homer, Tasso, and Virgil. But "canto di cigno," as Prof. Pavesi says—a droll metaphor having regard to Spallanzani's raptorial countenance, particularly as it must have looked peering above the bedclothes!—"Canto di cigno," for at 2.30 a.m. on February 11, 1799, after having received the Papal benediction, he fell back and expired suddenly.

At the post-mortem his heart was taken out and deposited by his brother Nicolo in the church at Scandiano. The bladder and urethra, being of pathological interest, are still preserved in Pavia—mortal relics as notorious as Mr. Babbage's brain or Lord Darnley's left femur in the museum of the Royal College of Surgeons.

Spallanzani's reputation beyond any doubt has declined from the meridian height it occupied during his lifetime. His genius of character and his attainments were evidently a potent influence among his contemporaries, and the nature of some of his experiments in those dark days was well calculated to excite the wonder and admiration of the crowd. It used to be said that fecundation was among the mysteries of nature, and, like many of her operations, an object of admiration rather than of inquiry. But the reverend professor, unwilling to cast too much responsibility on the Divine Power, however agreeable that might be to the idleness of man, set to work and succeeded in artificially fertilising a bitch spaniel with the spontaneous emissions of a dog injected by a syringe. Sixty-two days afterwards three lively whelps were born. "I can truly say," he remarks, "that I never received greater pleasure upon any occasion since I cultivated natural philosophy."

His work in pond life and protozoa—"myriads of which peopled a single drop,"—and his observations on Rotifers "which came to life again" after desiccation, lent colour to the hyperbolic expression of admiration with which a poet suggested that he had divine power.

I trust it is no very cynical asperity to say that there was nothing divine at all about the Abbé. Spallanzani was not an angel; yet he was something more than a great biologist—he was a great man. A study of the extensive biographical literature which has grown up around him will give the curious reader some idea of his masterful personality and of the way in which it gripped the scientific world in which he lived.

POPULAR SCIENCE

SOME RESULTS OF OBSERVATIONS ON THE ECONOMY OF THE HOUSE SPIDER, *TEGENARIA ATRICA*. By THEODORE SAVORY, Exhibitioner of St. John's College, Cambridge.

THE ease with which house spiders, or in fact any sedentary spider, may be kept alive in captivity is partly responsible for many observations which, while they are of the greatest interest to the specialist in arachnology, may be of some value to the general zoologist. A brief note of a few of these appeared in the *Field* of January 9, 1915, and it is here proposed to continue and elaborate them.

We have all watched the garden spider, when uncertain of its prey's presence on the outskirts of its web, give one of the threads a tug which decides the question. The house spider's modification of this is of peculiar interest. Its tarsi fixed in the silken sheet, it draws in each leg a distance of a millimetre or two, thus decreasing the perimeter of the figure surrounding it, and giving just that twitch to the web that is required to make a fly or other insect move on. As the garden spider wraps up its captive, so does the house spider, lest its struggles irrevocably destroy the web. For this purpose it holds the fly down on to the sheet of the web, and, itself walking around it, twists it up in sufficient silk to suppress it.

There is, I believe, a little rhyme about a centipede, which runs—

> A centipede was happy quite
> Until a toad in fun,
> Cried "Pray, which leg moves after which?"
> This roused her doubts to such a pitch,
> She fell exhausted in a ditch,
> Not knowing how to run.

Wondering myself how a spider ordered the movements of its eight legs, I allowed several to tire themselves by struggling on the surface of water and then set them to run slowly over a

regular surface, so that their motion could be carefully watched. It was thus rendered evident that—

1. The longest legs, those of the first and fourth pairs, move along the lines of their own directions by vertical bending of the joints; the shorter legs of the second and third pairs move forward by rotation from the coxæ, at right angles to their own directions.

2. First near leg moves with the fourth off leg. (*a*)
Second ,, ,, ,, ,, ,, third ,, (*b*)
Third ,, ,, ,, ,, ,, second ,, (*c*)
Fourth ,, ,, ,, ,, ,, first ,, (*d*)

3. Walking consists of (*a*) and (*c*) simultaneously, followed by (*b*) and (*d*) simultaneously.

During this investigation it became quite clear that the spiders that were tiring themselves on the water were most certainly swimming. A spider dropped on the surface of a sheet of water does not sink. From below, total reflection shows that a film of air lies between its ventral surface and that of the liquid, while so long as the creature remains still the legs indent the surface but do not pierce the " skin " caused by capillarity. Motion, when induced, begins with extreme rapidity, but after half a minute slackens and proceeds in precisely the same manner as in walking.

The first and fourth pairs of legs slide over the surface, the latter lying flat thereon for the last two joints. It would seem that they have little if any propulsive effect. The second pair penetrates the surface about two joints deep, the third pair one joint. I think that nearly all the propulsive effort is afforded by these pairs. It is manifest that these Tegenaria were indeed swimming, and not, like Lycosidæ, Pisauridæ, etc., running on the water. For, in these latter cases, the first and fourth pairs of legs are not slipping; the second and third pairs are not thrust into the water; and the body is supported on the legs in the ordinary way and not resting on the water. Again, though entirely distinct from the subaqueous activities of the water spider, it resembles them in that free and not dissolved oxygen is provided.

Finally, the spiders that had swum and walked at my command were returned to their cages and one and all settled down at once to what was, perhaps, the most interesting series of actions of all. They proceeded to complete their

toilet after their bathe! The use of the palpi for cleaning the falces and fangs after a meal was described in some detail by the present writer in the *Field* of May 23, 1914, but these were a far more elaborate process. The following operations were observed:

1. The second and third pairs of legs are dried where necessary by pulling them slowly through the opening and shutting maxillæ, and finishing with a long stationary "suck."

2. The palpi are dried in the same way.

3. The first and fourth pairs of legs are cleaned a little as in 1, but mainly by rubbing carefully with the second and third pairs followed by a sucking of that limb.

4. The ventral side was dried by rubbing the metatarsus over it; and this joint is then sucked. A very few applications seemed to suffice.

These separate actions do not take place in any orderly manner. A little of one is followed by a little of another, and often 2 and 3 are simultaneous. The spider dodges from limb to limb and from side to side with no regard for sequence. The whole operation may take as much as half an hour.[1]

The ecdysis of one of these spiders is readily witnessed since, before moulting, the legs turn to a dull, almost black tint. A male which I saw undergo its final moult in extracting its legs from the old skins, heaved fourteen times a minute as regularly as clock-work for half an hour and then, hastening, pulled for ten minutes seventeen times to the minute. This gives a total of nearly six hundred pulls to remove a leg about thirty millimetres long. Smaller spiders can moult completely in a quarter of an hour or twenty minutes. After ecdysis, the legs, palpi, and falces are a quite pale green and do not turn brown until some hours later.

Some interesting and fairly original results have been obtained concerning the fertilisation of the females by the male and her willingness to accept his advances or to eat him.

A male placed on the web of a female at once utters the sexual call by drumming on the web with his palpi, in exactly the same way as Warburton and Moggeridge have described in the cases of the wolf and trap-door spiders. In many cases this brings out a fierce paramour and leads the male to think

[1] My sister, V. Savory, has recently observed these same actions performed by the garden spider, *Epeira diadémata*.

more of his own life than that of the future generation. In happier circumstances, the female takes absolutely no notice of his presence, when he will cautiously advance towards her and with his long forelegs tentatively feel her outstretched forelegs. If the female is even yet motionless, it is well and he can proceed with the essentials of his suit. The actual fertilisation requires about ten to fifteen minutes with each palpus.

Oviposition has been well described by Warburton in an almost classical memoir, and I have but few additions to contribute. The sign of preparation which has usually been considered infallible is the spinning of a little sheet of closely woven silk, upon the lower surface of which the eggs are deposited. A spider of mine, however, last autumn spun this sheet and subsequently made no attempt to lay eggs or complete the cocoon, in fact it was obviously not the possessor of any eggs at the time when the sheet was spun. Warburton tells how the spider will finish its cocoon even if the eggs be removed immediately after laying. I have one example of the opposite case in which the spider placed the eggs in position but made no effort to cover or protect them in any way, and incidentally enabled me to secure a unique photograph of a spider's egg-cocoon at the middle of construction.

It is not necessary to take Warburton's advice and sacrifice a night's rest to see the egg-laying, for a spider which, like mine, spends all its days in a dark cupboard may often be deluded into laying its eggs in daylight. Three cocoons is the usual number; appearing at intervals of a fortnight, and each containing from 75 to 85 eggs. A spider that I caught in the early days of April 1914, having laid three cocoons by June, refused to die of old age and lived until December, producing no less than eight additional cocoons of fertile eggs in that time. These last cocoons contained only about forty eggs each. Such an unnatural extension of life I can only attribute to the fact that this spider was an object of interest to a large number of friends—contemporaries at Aldenham School—and that it was thus supplied with a relatively enormous amount of food.

The egg is originally quite spherical and a pale primrose-yellow in colour. It later becomes ovoid and then develops tightly folded but obvious legs. The young when first able to move have a primrose abdomen and creamy translucent legs

and cephalothorax, are about two millimetres long, and weigh just a centigramme. First the eyes become visible as black dots, and next the legs change to a greenish yellow. Later the cephalothorax becomes yellowish brown, and the abdomen dark green, with a lighter pattern of the same design as that on the adult spider. Ultimately the colour becomes brown and remains so.

One reference to a half-finished investigation in conclusion. Dr. A. H. Cooke, studying the attack on the mussel by a particular foe, found that the creature tended to plunge its weapon into the most vital spot of its prey. This conclusion was reached by accumulation of the pierced shells of the mussel. The little that I was able to do at the end of last autumn seems at present to render it quite possible that the spider, too, deals the *coup de grâce* to the trussed-up insect also in the most vital spot.

ESSAY-REVIEWS

THE CULT OF INCOMPETENCE, by Geraldine E. Hodgson, Litt.D. (Trin. Coll., Dublin): on **Le Culte de l'Incompétence,** par Émile Faguet, de l'Académie Française. Published by Bernard Grasset, Paris, in the Series *Les Études Contemporaines.* Eighteenth Edition, 1912. [Pp. 231.]—English translation, **The Cult of Incompetence,** by Beatrice Barstow, with Introduction by Thomas Mackay. [Pp. 236.] (London: John Murray, 1911. Price 5*s.* net.)

The opening years of the twentieth century were marked by an unfortunate taste for self-exaltation. The infant era, scarcely attaining to perfect good manners, proclaimed its own wonderful superiority to all the "dark ages" lying behind it. The war's unexpected revelations of more than barbaric horrors have possibly chastened this temper sufficiently to make it tentatively patient of the arresting title which M. Faguet has chosen. Unlike some pre-war publications, this book seems to have gained cogency from subsequent events. Instead of revealing only the failures of the past, it presents reflections auxiliary to future reconstruction; which is natural enough since M. Faguet does not content himself with superficialities, but reasons from a close, penetrating, discriminating observation of fundamental facts. Though this method renders none of us infallible in our conclusions, yet whether, according to their bias, men call it scientific or philosophical, it is the way to advance knowledge and edify civilisation. The whole book is a considered and, in the main, measured indictment of the essential failings of Democracy; weaknesses obvious enough in the ancient polities of Greece and Rome, familiar in the city-communities of fifteenth-century Italy, not unknown in modern Europe. An author writing of France must sometimes deal with points not applicable to England, just as some of our deficiencies are unknown in France. English readers, then, will select such parts of the indictment as fit us, adding our peculiar tributes to incompetence.

At the outset, M. Faguet, with French thoroughness and

lucidity, inquires into the ruling principle of democracy. He notes that in the lower animal kingdom, and in the world of business men, *division of labour according to ability* obtains. In an ordinary business house, so he affirms, " each one does what he has learnt to do, that which he is most capable of doing best." The very opposite of this wise principle operates, according to him, in a democracy : " it gave me little trouble to discover that its principle is the cult of incompetence." Not its failing, mark, but its principle. He reminds us of that Athenian tribunal which condemned Socrates, adding the biting comment that if his death were regrettable, at least " the principle, the sovereignty of incompetence was saved." Humorously, he describes how in the beginning the French people, deciding that not every one of them could claim legislative capacity, chose some of their number to select legislators. Seemingly they did not hold the view which Protagoras attributed to the Athenians, that " every man ought to share in political virtue." This quaint French remedy he dubs *compétence par collation*—a phrase which his translator only once attempts to anglicise. He glosses it with the remark, " the crowd, or rather the constitution, imagined that legislators elected by the crowd are more competent to make laws than the crowd itself." With illustrative irony, he observes that while a university may legitimately settle that a particular non-graduate deserves a degree, only adverse circumstances having prevented him from gaining it in the ordinary way, yet if the whole crowd of non-graduates decide that one of their number shall be decorated with a doctorate in mathematics, " it seems paradoxical and not a little humorous." He makes a qualification, declaring that the crowd should have some power of choice, because though such choice does not indicate " what the crowd is thinking, since the crowd never thinks," it is a guide to popular feeling or passion, an element which he claims should at least be represented in the high places of the social organism. He seems partly to overlook, or, at least, to underrate another valid qualification. When we have unreservedly admitted the dangers of ignorance, corruption, and party passion, may we not also concede that uneducated simple people, if they be straightforward, may possess intuition for capacity and integrity in other men? For integrity? yes, says M. Faguet, but not for capacity nor

specialised knowledge! But is it certain that the crowd has no *flair* for capacity, no instinct leading it to sort out the wheat from the chaff, in the throng of political aspirants? Let men turn back to those pages in J. S. Mill's *Autobiography*, where he describes his "political meeting" with the working-class electors of Westminster. Yet, there may be a racial difference here, those Westminster people being not quite typical of either nation.

M. Faguet's insistence on specialised knowledge is specifically French, while we raise to the "*n*th" our method of removing the shoemaker from his last. Where we debate the wisdom of letting a lawyer control the Army, a philosopher manage the Navy, and anybody in general conduct Education and Finance, the French do not hesitate to declare that such plans originate in Bedlam and go out in disaster. Yet, sometimes, among ourselves, it is not so; what, in France, would prove sheer folly, turns out, in England, shall we say a respectable second best?

Modern France, too large to practise "pure democracy"—that system possible in ancient Athens where the adult males could assemble at the summons of one herald—has chosen to elect law-makers by the crowd from the crowd, a device M. Faguet calls *compétence par collation arbitraire*. He criticises it by a comparison: "As a certain bishop, addressing a haunch of venison, said, 'I baptize thee carp,' so the people says to those of its choice, 'I baptize you jurisconsults, I baptize you statesmen, I baptize you social reformers.'" Further comment seems needless. He proceeds in the third chapter to arraign salaried officialism, describing it with singular felicity by employing an indirect method; indicating the "refuges of efficiency" whereunto the abler members of the community, having been eliminated or excluded from public functions, betake themselves, finding an outlet for their ability in private employment. Even some of these can still be terrorised, if they have relatives in the public service who could be made to suffer for their too independent temerity. To those who argue that a thoroughly nationalised, or socialised, State would be free, he replies—Not while the government is electoral, for that involves the retention of the party system. His dismal conclusion is: "Nothing would be altered, save that wealth and the last remnants of freedom would be suppressed. . . .

There is no solution there." With relentless pen he describes complete nationalisation as "a harsh oligarchy, with defenceless creatures under it, equal in their poverty, on a level in their misery."

This short chapter raises two points of capital importance, viz. the greater burden of mass-tyranny than of individual despotism benevolent or otherwise, and the ruin of all higher life consequent on salaried dependence. Is he not absolutely right when he contends that the harsh rule of the vague "many" is more intolerable than any exactions of an individual, and is so just on account of that vagueness? In the last resort, one man can be, often has been, dealt with, when his abuse of power becomes too outrageous. But who can put restraint on a shifting crowd of officials? True, the electoral system purports to afford a chance of change: yet of what avail is change while the necessity of "toeing the party line" destroys men's integrity? We ought never to forget, when we are estimating the chances of democracy, that public morality never exceeds the private morality of the individuals composing the "public"; and that, as a general rule, mass-action, lacking, as it does, the stimulus of direct responsibility, tends to fall considerably below private standards. Nor should we overlook the common inability to turn abstractions into concrete action. Philosophically, theory and practice are inseparable. In England, on the rare occasions when we propound abstract principles, we may still acquiesce in the most flagrant neglect of their concrete realisation.

Though an ideal form of government has never been agreed on, some odd perversity in human things contrives occasionally that schemes which look futile on paper work out fairly in practice. The theory of the Roman Consulship, which allowed each of the two to veto what the other did, has been criticised as a fine example of what government should not be. Probably it was superior to the rule of one, and infinitely so to that of the irresponsible many. Nor is the reason far to seek. If we look at psychological facts and not at fine-spun political theory, have we not here an instance of rare practical sagacity? A paradox, let us remember, is not necessarily the equivalent of a folly. Endow two persons with precisely equal powers for thwarting each other, then, whatever their work, provided they really care to do it, their obvious interest

will be each to do his own, and, so far as may be, leave his fellow alone. As a matter of fact, the Roman Consulship worked !

We shall never equalise ability; the effort to do so is the chimera of dreamy fanatics. But we might go a long way towards equalising opportunity. If we put people into places suited to their bent and capacity, and then gave them a free hand and a decent subsistence, we should have accomplished more perhaps than we can even guess, until we try the experiment and see it at work.

Since the possession of wealth is a species of power, we need in our modern Plutocracies, more probably in England and America than in France, to perceive the greater threat to freedom of thought and activity inherent in the silent insidious pressure of diffused riches than in any rewards of a wealthy autocrat. The plutocracy gains this power through seats on public bodies, and by its private methods of so-called philanthropy: it is an undiagnosed and increasing danger to individual liberty and independence. Faguet dwells on the State-aspect of the peril. Possibly, this is the end of the mischief in France. But it is not the beginning nor the centre in England, where we still permit wit and brilliance in our statesmen, even if we try to relegate them to the Upper House, when they scintillate beyond bearing in " another place." In England, this threat to liberty centres in the numerous non-political bodies which we encourage and the French eschew, this power residing in municipalities, councils, and great commercial combinations. Faguet sets over against the enslaved salaried official " a barrister, a solicitor, a doctor, a business-man, a manufacturer, a writer, no one of these is State-paid." If there be a class whose freedom from sordid influences is vital to the State, it is the teaching profession, which he excludes from his "free-list," and who tend indeed more and more towards salaried officialism. Doubtless their freedom should have limitations. As Juvenal, not an over-particular person, observed, " Great reverence is due to the child." Further, no man, teacher or otherwise, is, as Mill said, free to preach violent counsels to an inflamed mob, nor, as some of us think nowadays, " pacifism " to a country fighting for freedom and existence. Such limitations of common sense and decency being admitted, the teacher need only remember further that

liberty differs from licence. In our public life, certainly in England, probably in France, we all should realise increasingly that work in itself requires protection, whatever sort of work it be, for the State, for knowledge, for commerce, or what not; and that a man's claim to security and consideration is not his ability to kotow to this superior, or take the fancy of that colleague, but efficiency to do his work. In many departments of life the Cult of Incompetence has brought us to the verge of extreme fatuity, and nowhere nearer than by the attempt to reduce the conditions of work to the amenities of club life. That incompetence should be, as Pestalozzi long ago declared it, and as some of us have seen it to be, the direct outcome of a faulty educational system is regrettable enough: but that France can be accused of raising it to a national aim, fostered by a definite State policy, and that England, true to type, should be drifting into rather than steering for it, is a disaster of the first magnitude. If the method be identical, the use differs; France practising it deliberately, we vaguely, unconsciously, therefore more dangerously. M. Faguet names it ostracism. Democracy, he writes, "can systematically prevent any man who betrays any superiority whatever, either of birth, fortune, virtue, or talent, from obtaining any authority or social responsibility. . . . Ostracism is . . . still feeling its way. . . . This will continue till it has been reduced to a science, when it will contrive to level, by one method or another, every individual eminence, great or small, that dares to vary by the merest fraction from the regulation standards. This is ostracism, and ostracism, so to speak, is a physiological organ of democracy. Democracy by using it mutilates the nation, without it democracy would mutilate itself."

But it is a mistake to attribute this condition wholly to political causes; our moral condition, engendered by the successful pursuit of knowledge for mechanical purposes, of wealth at whatever cost, of power no matter how won, must answer for much. Ostracism is the logical issue of politics coloured by materialistic ethics, since originality, variety, and spontaneity are even more taboo under the Prussian autocracy than in the French Republic or our constitutional monarchy.

Whatever its environment, there is the deadly thing—ostracism of originality, the triumph of the commonplace. In his wittiest chapter, M. Faguet attributes the ruin of originality

first to the soul-destroying system of examinations, which in France absorbs some ten to twelve years of the life of a vast army of aspiring officials " rendered passive at the age of their keenest intellectual activity," and secondly to an obligation laid on those salaried by the State. This obligation the translator renders " obliged to be civil." But here, as elsewhere, the edge and salt of the original are lost, M. Faguet's *obligé de devenir demi-agréable* " being untranslatable. Against this evil demand *de devenir demi-agréable,* too strong a protest cannot be entered. All through the community, in the shop, the factory, the shipyard, the office, the school, the university, this demand for pliability is heard. Not so is greatness attained. No one wishes to defend bad manners, they are too common to be attractive: no one ought to excuse deliberate boorishness. But that being agreed, it remains true that the pressing need to-day is for efficiency, competence, not for invertebrate, often insincere, pleasantness. Justice and efficiency rank above cheap amiability, or should do so. Often people of original power are not *personæ gratissimæ* to the mediocre and intellectually indolent. But even if they be tiresome and difficult, they are the necessary pinch of salt, lacking which, life loses its sapidity. The trade-union iniquity of setting the pace by the slowest least efficient worker is robbed of some of its malfeasance by the cynical frankness of its avowal. But in the worlds of business, of invention, of learning, the same ill-practice is attempted cryptically: wherever circumstances allow, ostracism is practised by the heavy mass of the half-competent and unoriginal at the expense of the able and strenuous minority. It may be true that the older universities present many imperfections, but, at any rate, they have not raised ostracism to a fine art. That may be the secret of some attacks on them. Again, the perpetual trend of legislation, still more of administration, is to reduce schools to one pattern. Apart from all other considerations, those acquainted with youth's struggles to grow up will admit that the immense variety of individual abilities and temperaments demand at least some difference in their places of education. M. Faguet is temerarious enough to plead for a little consideration even for *l'autodidacte*! He admits that " the man who has taught himself is apt to be a vain, conceited fellow, who takes pleasure in thinking for

himself, and has an absolute delight in despising the thoughts of others." We can all admit the truth of that, even those of us who are mainly "autodidacts." But the clinching plea follows: "It is among these self-taught men that we find those vigorous spirits who venture boldly beyond the domain of human science, and extend its frontier."

One might suppose that this fact, not difficult to prove from history, would decide. But no: the mediocre mass lifts up its idol of numbers: "Ten times more numerous, I am told, are the pretenders to originality whom we save from themselves by discipline, than the true geniuses whose wings we clip." The murder is out: precisely there is the root-fallacy on which the Cult of Incompetence is reared: and should the edifice show sign of buckling, it is patched up with sentimental philanthropy. The mass declares that quantity is everything, and quality little or nothing. Yet, as a matter of fact, it is, all down the ages, the quality, the fine spiritual, moral, intellectual, imaginative quality of the few which has pioneered, created, imagined for that vast unoriginal herd who, whatever their diurnal utility, are not constructive, and who, without such leaders, could hardly have emerged even yet from primitive precariousness and discomfort. M. Faguet's conclusion is lit and edged with wit. Will those on whom lies the burden of conceiving political and social arrangements on which Europe's future depends consider his verdict? It may have some savour of pungent wit, it may even show a trace of that disdain which thwarted ability sometimes feels towards massive dulness: but beyond and above these venial errors, it proclaims a truth vital to Europe's future efficiency: "Je réponds qu'en choses intellectuelles les questions de chiffres ne comptent pas. Un esprit original étouffé est une perte qui n'est pas compensée par dix sots préservés d'être ultra-sots. Un esprit original laissé libre de l'être vaut mieux que dix sots à moitié contenus et réprimés."

Though M. Faguet adds one more chapter, "The Dream"—his hope for the future—yet the real gravamen of his book is here: here is the final condemnation of the Cult of Incompetence, here the irrefragable charter of Efficiency: "en choses intellectuelles les questions de chiffres ne comptent pas." How true it is that in everything, everywhere, quality, not quantity, is the *summum bonum*. The critics may chafe as

they will, yet the poet Gray, *e.g.* lives, and while the English race endures, will live, on the strength of some dozen illumined pages. It is the deadly triumph of the semi-competent mass that it has already more than half imposed on us the sterilising belief that quantity is everything, and quality scarcely anything. It has extended instruction at the price of making it shallow and repetitive; it has multiplied workers at the expense of the product: it has popularised the sempiternal dulness of a plethora of identical machine-made commodities; it has enthroned the plutocrat above the thinker, and this, despite Mulcaster's withering dictum, "of all the means to make a gentleman it is the most vile to be made for money." Faguet quotes Nietzsche's "Modern education consists in smothering the exceptional in favour of the normal," but he had already said it implicitly himself when he pleaded for quality before quantity.

While France, with her political ostracism and her rigid examination system, "cuts down the tall ears of corn," England achieves a like result with less forethought. We produce incompetence largely by educational chaos, which in its turn springs from our profound indifference. It may seem incredible, but it is true, that about a dozen years ago the editor of one of the foremost English monthlies returned an article on the Training of Teachers with the remark that it was well written and interesting, but "unfortunately education is of no public interest." Could he have put the truth more succinctly?

Lastly, as we seek to lay M. Faguet's lessons to our own hearts, we must reckon for our national passion for compromise. Despite all our weaknesses, faults, and even turpitudes, it would be difficult to draw quite so definitely black a picture of our own incompetence, as M. Faguet's relentless pencil has limned of our great Ally, a picture, however, which her *risorgimento* has gone some way already towards obliterating. But if we are not so deliberate, we are as wrong. The English love of compromise, so alien to the Gallic genius, so genuinely stupid as it demonstrably is in theory, and often so irritating in practice, yet accounts, in some sort, for the nation's mysterious fortune in triumphing over those predictable calamities which, in all logic and sense, should follow on our vagueness, and not least on our inveterate habit of trying to do two opposite things at once, and of entrusting expert direction in one sphere

of work to an expert in another or in none; our ingrainedly English practice of removing the shoemaker from his last. However imperiously theory would urge us to condemn our muddled, out-at-elbows methods, yet the national genius for "getting through" in the most unlikely, impossible circumstances does give a sort of "pragmatic sanction" to our cherished whim for using the wrong tool wherever we can; and because we do thus succeed at last, we are deluded into overlooking its essential flaw. The worst that can be said of this, our peculiarly national form of incompetence, is not that it fails, but that it succeeds with horrible waste of material, with criminal sacrifice of our ablest and our best; succeeds indeed at the price of destroying just those things and those persons whose existence the country, could it learn what "belonged to its peace," would recognise as being really vital, absolutely indispensable. A worse worst need hardly be sought or desired.

CROWDOLOGY, by JOSHUA C. GREGORY, B.Sc., F.I.C.: on **The Crowd in Peace and War**, by Sir MARTIN CONWAY. [Pp. 332.] (Longmans, Green & Co., 1915. Price 6*s*. net.)

IN these days thought has taken all knowledge for its province, deprives few things of their share of attention, and is quite ready with a generous meed of criticism for all. Writers have jeered at crowds throughout history and studied their foibles, when they condescended to study the crowd at all, to make sport of them, to mock at them, or to play upon them. Sir Martin Conway's *The Crowd in Peace and War* is a recent addition to a rapid series of studies of the "crowd" to which science is now seriously turning its attention. It is, perhaps, impossible to consider the crowd with the same dispassionate, detached regard that science bestows upon the atom. Atoms have no votes and obey their own laws without trying to make us conform to them. Sir Martin, like Le Bon and others, has certainly not attained to a purely contemplative or coldly scientific view of his subject. He might have been less racy if he had. Perhaps crowds which, if not of emotion all compact as Sir Martin would have us believe, tend distinctly to the emotional cannot be adequately studied in a purely contemplative spirit. Emotion needs an answering

feeling to apprehend it and, most of all, to interpret it. Also, Sir Martin may not be merely prolonging the jeering tradition even when he, gradually widening his denial, refuses to the crowd any intellectual capacity whatever. He honestly thinks, no doubt, that his opinion is scientifically impartial. Besides, the crowd will presumably have faults if it is composed of imperfect individuals—it offers plenty of scope for criticism, a scope that Sir Martin does not fail to appreciate. He makes handsome amends, however; if he does say hard things, he also adds that the crowd has secured the elevation of the mass of mankind from the level of the brutes. He thus moves farther away from the classical and contemptuous attitude than does Le Bon when the latter suggests that the present rule of the crowd is a barbaric phase about to destroy civilisation. In any case it is well that the crowd should receive systematic and careful study, for only in this way can we hope, in conjunction with other knowledge, to acquire the more perfect understanding so necessary for our guidance in dealing with the complications of modern societies.

The term "crowd" is now used to denote a group of individuals connected by some common interest or interests. It includes both aggregates of people gathered together (crowds in the usual sense) and dispersed groups. The audience at a meeting is a crowd, and so, in the second sense, are the habitual readers of a newspaper. The latter are, and may always remain, dispersed, but they constitute a crowd because of their common interest. The comprehensiveness of this definition, though justified, must be kept closely in mind if errors are to be avoided. As a matter of fact, students of the crowd do forget this comprehensiveness and do fall into considerable errors. The link of interest that connects individuals into crowds may be almost anything whatever. Music, science, rat-catching, dining, sport of any kind, political principles in their diversities, religion, philanthropy, even burglary may be the common animating interest that unites aggregates or dispersals of men and women into crowds. Obviously these different crowds will vary enormously, according to the nature of their constitutive principle. The Royal Society is a crowd very different from the multitude assembled at a football match. Now the word "crowd" naturally suggests to us, in the first place, an assembly rather than a dispersed group,

and in the second place an assembly more resembling in its enthusiasms and mental action the footballer than the F.R.S. Learned writers on "crowds" are not prevented by the width of their definition from a very perceptible tendency to narrow their notions. The natural associations of the word "crowd" assert themselves as soon as the ink is dry on their definition—they remember the footballer, forget the F.R.S., and their deductions suffer accordingly. One illustration will show how Sir Martin Conway, like most other writers on his subject, attempts to squeeze the infinite variety of crowds, a variety almost as infinite as that of individuals, within the limits of a narrow generalisation.

In his second chapter we learn "that a crowd has all the emotions and no intellect. It can feel, but it cannot think." The Royal Society has already sunk below the mental horizon. It is, of course, the crowd-aspect of the Royal Society that is in question—the collective mind or opinion, and not the individual minds or sentiments. We may safely assume, however, that Sir Martin Conway would hardly deny that the Royal Society is quite capable of thinking out a solution to a scientific problem. He has simply forgotten that it is a crowd.

Forgotten data mean false conclusions, and both involve false reasoning. "The fundamental reason why a collective body of human beings differs *toto caelo* from so many individuals is because no two individuals can ever think alike, whilst any number can feel alike." Two individuals can and do think alike on one, two, or more separate points. It is precisely because men do think alike in particular instances that they become members of a crowd incorporating these particular opinions. Protectionists all believe in protection. Some of the Protection crowd may believe in Natural Selection and form a Darwinian Society; others may believe that Natural Selection "is not only dead but damned" and combine into an anti-Darwinian movement. The "crowding" follows the lines of agreement. If each single person held opinions absolutely different from those of every other, a supposition practically unthinkable, there would be no crowds at all. Crowds are seldom composed of people who think alike in all respects, but they are composed of men and women who share common beliefs. Crowds, in fact, represent beliefs, opinions, and sentiments common to a number of human

beings. There may be diversity of opinion concerning a common aim, but the common aim organises the crowd.

Sir Martin Conway's book must not be judged by its errors alone. No living being could write 332 pages on crowds that would stand undisproved through all time. It is a book to be read and to be reckoned with. Though time bury it at last, it will have a long life. It should be read by all who are interested in the pressing and complicated problems presented by modern societies—problems that, urgent as they were before, are being intensified by the Great War. Those who are not interested will become so if they will only read it. We are at present, however, in the critical vein and belong to the critical crowd. This need not prevent us from being also members of an appreciative and approving crowd, to meet when its turn comes. But in this article the critical crowd assembles.

It is, no doubt, true in a general sense that emotions are stronger and intellect weaker in the crowd than in the individual. It is true that suggestion, on the whole, is more influential over our beliefs than our individual reason. It is true again that the suggestive influences of the crowd tend to predominate over logic or intellect. We are moulded and influenced and driven into beliefs by the steady pressure of the opinions expressed by the newspapers we read, of the sentiments and habits of thought of the class to which we belong, of the prevailing customs of our occupations or professions, of the authors, speakers, and traditions that unite to form our milieu. We do absorb, in short, the suggestions of the crowds to which we belong. Sir Martin quotes from "A Neutral Correspondent," who writes that when "he entered Germany he believed himself . . . proof against atmosphere." He found everything published or written in Germany was carefully calculated to suggest the virtue of the Fatherland and the evil designs of England and the Entente Powers. "The cumulative effect upon me of this constant suggestion . . . was such that I seemed . . . to become merged in the German mass." The "crowdologist," however, in his turn, is subject to the suggestion of his own particular crowd when he assumes that the individualities of the units are invariably merged in the collective mind of their group, under all circumstances and whatever the nature of the "crowd" to which

they belong. "It is an entertaining if somewhat saddening occupation to sit where people congregate for talk and to listen for the expression of a really independent personal opinion." People do come independently to the same opinion, even in hotel lounges. Crowdologists appear to miss the opposite entertainment of noting how each individual in a company fundamentally in agreement often approaches the common opinion by a different route. A lady staying in the hotel at Bath found daily diversion in the variety of opinion scattered around her. In opinion on the war, for example, there is the unity in diversity, or diversity in unity, so much beloved by metaphysicians. Few of our opinions are original in the sense that we first conceived or expressed them—it is a merciful dispensation of Providence that it is so. Some sections of society suffer severely from a profuseness of "original opinions." They are for the most part introduced to us by other members of the various crowds to which we belong. Very often they are pumped into us like food into hens when they are being fattened for the market. But it is quite untrue that we meet but few people "who are not even partially independent individuals." Most men of mature age have evidently used their own judgment in accepting, if not in originating, the principal opinions they express. The crowd serves one main purpose of passing along for inspection the beliefs originating in certain individuals. It frequently undertakes (and often succeeds in the endeavour) the duty of compelling its units to accept or reject them. So far crowdologists are right. They generalise too freely when they assume that the individual always, or even usually, forgoes his right of private judgment on the beliefs presented for his inspection. He forgets that the crowd is not only a stamping-in agent, but also a sieving mechanism for straining out. Independent judgments of individuals often need a good deal of straining. Uniformity of opinion results among different individuals through their separately arriving at the same conclusions, as well as from an overpowering by an all-powerful suggestion. Especially is this so in crowds of the dispersed type, where groups or units sit *in camera* on the crowd-judgment. Dispersed crowds are apt to escape the eye of the crowdologist, though they are the most numerous, and, on balance, the most potent. Sir Martin Conway makes the extraordinary

statement that different individuals " if they would think for themselves (they) would have to express infinite divergency of view." They would all agree that the sea is salt, and rational beings frequently come to similar conclusions because they are rational.

Like all young sciences, crowdology has yet to perceive its youthful error of hasty generalisation from some of the more striking and obvious of its data. It is anxious to get its facts into order and tidied up into a system. This is laudable, but usually results in a false simplification as the first-fruits of this early enthusiasm. Some crowds are carried off their mental feet by emotion, therefore all crowds are essentially foolish and moonstruck. The crowd is responsive to suggestion, therefore it can never think. Crowdologists also invariably assume that there are certain fixed characteristics for the crowd, apart from the character of the units. They naturally conclude that a foolish crowd will always remain foolish. They forget, as a consequence, that to the progress of the individual there corresponds a superior character in the crowd. We suggest to Sir Martin Conway and others a motto that is not all the truth, but at the same time is a wholesome corrective of extreme views: Take care of the individual and the crowd will take care of itself.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

THE *Revue Semestrielle des Publications Mathématiques*, which was mentioned at length in the number of SCIENCE PROGRESS for July last (**11**, 89), can now be obtained in London from Mr. D. J. Bryce, 149, Strand, W.C., and it will be seen, from what has been said in the place referred to, that it is most important that all British mathematicians should keep themselves abreast of the very extensive literature of mathematics with the help of this periodical. Two numbers appear in the course of every year, and the yearly subscription is 7*s*., post free.

The Mittag-Leffler Institute of the future that was mentioned in the last number of SCIENCE PROGRESS (**11**, 91) is shortly described in *Nature* (1916, **97**, 384). The object is principally to encourage the study of pure mathematics in Sweden, Denmark, Finland, and Norway; but a prize for pure mathematics is to be awarded, if possible, at least once every six years, and it will be open to the whole world.

History.—G. Mancini (*Memorie della Reale Acad. dei Lincei, classe di Scienze Morali, Storiche e Filologiche* (5), 1915, **14**) publishes a manuscript *De Corporibus Regularibus* of Pietro Franceschi called Della Francesca and maintains the thesis, which is not quite new, that the substance of this work was purloined by Luca Pacioli (1445–1514) in his *Divina Proportione*—a work dealing with the mensuration of plane and solid figures. To Mancini's memoir is prefixed a report on it by Prof. Gino Loria (cf. also *Nature*, 1916, **97**, 287).

J. H. Graf (*Boll. di bibl. e st. delle sci. mat.* 1916, **18**, 49; cf. SCIENCE PROGRESS 1916, **11**, 91) continues the series of letters from Casorati to Schläfli.

The history of Lie's theory of contact-transformations is dealt with in the fifth of the supplementary volumes to the *Jahresbericht der Deutschen Mathematiker-Vereinigung* (Leipzig, 1914) entitled *Die Berührungstransformationen, Geschichte und Invariantentheorie* by H. Liebmann and F. Engel (*Boll. di bibl. e st. delle sci. mat.* 1916, **18**, 78).

Principles of Mathematics.—Prof. G. Mittag-Leffler was, as

we know, one of Weierstrass's pupils, and has always regarded, not without reason, his old master's theory of real numbers as presenting certain advantages over the theories of Méray, Cantor, Heine, and Dedekind. The exposition which Mittag-Leffler (*Rev. gén. des Sci.* 1915, **26**, 504) gives of the foundations of the theory of numbers is thus very interesting, especially in connection with a paper on Weierstrass's theory which the same author published in 1909.

In a very able article on the calculus of probabilities and intuition, S. Pincherle (*Scientia*, 1916, **19**, 417) points out that the causes which we consider in such schematic representations of reality as mechanics and mathematical physics are reduced to a small number of predominating ones while the others are deliberately left on one side; but in sciences such as political economy, medicine, meteorology, and biology, the selection of dominant causes is not imposed by the question in itself, and a subjective element comes into play. Thus, in the latter cases, the statistical method, which is closely connected with the calculus of probabilities, must supplement deduction. The very definition of probability is founded on ignorance of which, if any, causes are dominant, and this ignorance is translated into a principle of equivalence between the various causes possible. Pincherle goes on to refer to modern researches into probabilities in which there are an infinite number (in the Cantorian sense) of possibilities, and the part played by the intuition in the calculus of probabilities; but what seems most important to the present writer is that some light may be thrown on the question as to whether, as Russell maintains, probability is fundamental in the principle of induction. If, in fact, probability depends on the notion of cause, it would seem that the law of causality, which depends on the principle of induction, cannot depend on probability.

In connection with the theory of relativity and an optical geometry of space and time, we may refer to two papers by Dr. H. Bateman which are difficult to abstract on account of their technical character: "Time and Electromagnetism" (*Mess. of Math.* 1915, **45**, 97) and "The Structure of the Æther" (*Bull. Amer. Math. Soc.* 1915, **21**, 299).

Theory of Numbers.—In a paper read to the Royal Society on May 11, 1916, Major P. A. Macmahon gave a detailed study of the enumeration of the partitions of multipartite numbers.

In connection with G. H. Hardy's researches on Dirichlet's divisor problem (see SCIENCE PROGRESS, 1916, **11**, 93), the investigation by Hardy (*Proc. Lond. Math. Soc.* 1916, **15**, 192) of the average order of two arithmetical functions occurring in the above problem is of interest.

H. H. Mitchell (*Trans. Amer. Math. Soc.* 1916, **17**, 165) writes on the generalised Jacobi-Kummer cyclotomic function.

Algebra and Analysis.—J. Littlejohn, in a paper read to the Royal Society of Edinburgh on July 3, 1916, showed, in the case of numerical equations, how certain operators—differentiations and integrations with respect to the coefficients—could be applied to evaluate the roots.

It is well known that abstract definitions of groups were gradually arrived at by Cayley in papers published in 1854, 1859, and 1878, that Hamilton (1856) first defined abstractly the groups of the regular polyhedra, and that Kronecker (1870) first gave a really abstract definition of a group in the case in which the operators are commutative. Miss Josephine E. Burns (*Amer. Journ. Math.* 1915, **37**, 191), starting from an article by G. A. Miller which was published in 1911, proves a few general theorems relative to the groups generated by two operators satisfying certain defining relations, and then gives abstract definitions of the substitution-groups of degree 8 and some applications of the above theorems.

To the Royal Society of South Africa on April 19, 1916, Sir Thomas Muir read two papers on determinants: one gave the discovery of the connection of Pfaffians with the difference-product, and established a series of theorems bringing Pfaffians into relation with permanents and other integral functions; and the other was a note on the so-called Vahlen relations between the minors of a matrix.

Prof. E. T. Whittaker, in a paper read to the Royal Society of Edinburgh on June 14, 1916, gave a general process for expressing a continued fraction as a continuant, and showed how to express the differential coefficient of a continued fraction as the ratio of two determinants the constituents of which are definite functions of the terms of the continued fraction.

C. N. Haskins (*Trans. Amer. Math. Soc.* 1916, **17**, 181) investigates the measurable bounds and the distribution of functional values of summable functions, and Dunham Jackson (*ibid.* 178) proves otherwise one of the theorems of Haskins.

T. W. Chaundy and A. E. Jolliffe (*Proc. Lond. Math. Soc.* 1916, **15**, 214) obtain the remarkably simple necessary and sufficient condition for the uniform convergence throughout any interval whatever of the series $\Sigma a_n \sin n\theta$, where (a_n) is a sequence of positive numbers decreasing steadily to zero, is that na_n tends to zero.

M. B. Porter (*Proc. Nat. Acad. Sci.*, Washington, D.C., 1916, **2**, No. 4) gives a simple proof of Lucas's theorem that the zeros of any polynomial $F'(z)$ lie inside any closed convex contour inside which the zeros of $F(z)$ are, and the theorem is extended to give information concerning the distribution of zeros of the derivative of certain rational or transcendental functions (cf. also *ibid.* No. 6).

Lord Rayleigh, in a paper read to the Royal Society on May 11, 1916, gave an approximate formula for Legendre's function $P_n(\theta)$, when n is great and θ has any value, which is sufficient for practical purposes and whose derivation is more within the reach of ordinary physicists than is Hobson's elaborate mathematical investigation.

W. L. Miser (*Trans. Amer. Math. Soc.* 1916, **17**, 109) continues the work of Picard (1880) and others on linear differential equations having elliptic function coefficients; Miser, however, investigates multiform solutions.

G. M. Green (*Proc. Nat. Acad. Sci.*, Washington, D.C., 1916, **2**, No. 4) generalises the theory of linear dependence to the case of n functions of several independent variables and applies it to the study of certain completely integrable systems of partial differential equations.

W. L. Hart (*ibid.* No. 6) treats three problems: (1) Certain fundamental theorems concerning a type of real-valued functions of infinitely many real variables; (2) The problem of infinite systems of ordinary differential equations; (3) The fundamental problem of the theory of implicit functions in this field.

Continuing some work of Wilczynski, E. B. Stouffer (*Proc. Lond. Math. Soc.* 1916, **15**, 217) is concerned with the problem of calculating the seminvariants for a system of linear homogenous differential equations.

Dr. H. Bateman, in a paper read to the Royal Society of Edinburgh on July 3, 1916, showed how the general equation of wave-motion associated with Maxwell's electromagnetic

theory could be transformed into the Laplacean form of equation in three variables.

In continuation of the well-known work of Schwarz on the fifteen cases of the hypergeometric equation which admit of algebraic integration, in which the conformal representation of a curvilinear triangle is discussed, J. Hodgkinson (*Proc. Lond. Math. Soc.* 1916, **15**, 166) discusses the conformal representation of the various triangles bounded by the arcs of three intersecting circles.

G. A. Bliss (*Trans. Amer. Math. Soc.* 1916, **17**, 195) investigates Jacobi's condition for problems of the calculus of variations in parametric form.

Geometry.—In April 1915 Dr. R. L. Moore presented to the American Mathematical Society a paper on the foundations of plane analysis situs, and these researches, when expanded (*Trans. Amer. Math. Soc.* 1916, **17**, 131), give three systems of axioms each of which systems is sufficient basis for a considerable body of theorems in the domain of plane analysis situs on what may be roughly termed the non-metrical part of plane point-set theory (cf. SCIENCE PROGRESS, **10**, 433 and 616; **11**, 92). The axioms of each system are stated in terms of a class of elements called *points* and a class of point-sets called *regions*. It may be mentioned that, on p. 136, the author cites Veblen's proof of 1904 of the equivalence of the Heine-Borel process and the Bolzano-Weierstrass process, but no mention is made of the proof which the present writer gave soon afterwards of the non-equivalence of these processes. In May 1916, Moore (*Proc. Nat. Acad. Sci.*, Washington, D.C., 1916, **2**, No. 5; *Nature*, 1916, **97**, 395) set up two systems of postulates for plane analysis situs based on the fundamental notions of *point*, *limit-point*, and *regions* of certain types. Each set is sufficient for a large number of theorems.

G. N. Watson (*Proc. Lond. Math. Soc.* 1916, **15**, 227) investigates a theorem of analysis situs somewhat similar to Jordan's well-known theorem that a regular closed curve possesses an interior and an exterior. Watson's theorem is that when a simple closed curve with a continuously turning tangent is given, if ψ be the angle between the tangent at a variable point P of the curve and the x-axis, then ψ changes by 2π as P describes the curve.

In a paper read to the Royal Society on May 18, 1916, Col.

R. L. Hippisley described linkages illustrating the cubic transformation of elliptic functions.

Gabriel M. Green (*Amer. Journ. Math.* 1915, **37**, 215) gives the introductory part of an investigation of the projective differential geometry of one-parameter families of space curves, such a family being regarded as a component family of a conjugate net on a curved surface. Indeed, the author makes use of the fact that, in studying a geometric configuration, it is often convenient to study the configuration as part of another which is characterised by some peculiar geometric property. In the same number of the same journal (*ibid.* 281) R. D. Beetle considers some congruences associated with a one-parameter family of curves, which are other than the congruence formed by the tangents to the curves.

The classification by invariants of plane cubic curves modulo 2 is dealt with by L. E. Dickson (*ibid.* 107), and he points out that the methods employed are applicable to other problems of the same nature and that they indicate the decided advantage to be gained in the theory of modular invariants from modular geometry as developed by himself, Bussey and Veblen, and Coble.

L. P. Eisenhart (*ibid.* 179) develops the equations of a surface from the point of view of regarding a surface as the locus of a one-parameter family of curves C, using for moving axes the principal directions of the curves C. The fundamental equations of condition to be satisfied by a set of functions determining a surface are established.

In 1855 Hesse pointed out the connection of the plane quartic curve with the figure of the general net of quadrics in space; J. R. Conner (*ibid.* 1916, **38**, 155) investigates, by almost exclusively geometrical methods, the correspondences determined by the bitangents of a quartic; the intimate connection between the plane and space figures makes intuitive many geometrical relations in both which might otherwise be difficult to prove.

E. B. Wilson and C. L. E. Moore (*Proc. Nat. Acad. Sci.*, Washington, D.C., 1916, **2**, No. 5) develop a theory of two-dimensional surfaces in n-dimensional space by the method of analysis outlined by Ricci in his absolute differential calculus. This work continues the work of Kommerell, E. Levi, and Segre.

ASTRONOMY. By H. SPENCER JONES, M.A., B.Sc., Royal Observatory, Greenwich.

The Gravitational Constant and Temperature.—An important memoir by Dr. P. E. Shaw has recently appeared (*Phil. Trans. R.S.*, ser. A, vol. ccxvi. pp. 349–92, 1916) in which, as a result of an extensive series of experiments, the author announces a dependence of the Newtonian constant of gravitation upon temperature. The experiments lead to the conclusion that "when one large mass attracts a small one, the gravitative force between them increases by about 1/500 as the temperature of the large mass rises from, say, 15° C. to 215° C." It is not possible to say definitely that the relation is a linear one, but assuming that this is so the result may provisionally be stated in the form that the constant of gravitation has a temperature coefficient of $+1{\cdot}2 \times 10^{-5}$ per 1° C., under the conditions just specified.

As far as can be judged from the account given in the paper, the experiments have been carried out with great care and thoroughness and precautions seem to have been taken to eliminate every possible source of error. Whether or not the startling result which Dr. Shaw has announced will be substantiated by later experiments, a tribute should be paid to the care with which he has carried out these difficult and delicate experiments. The method used was an elaboration of the original Cavendish experiment—consisting in measuring the differential attraction of two large masses on two small masses when the position of the large masses is changed. The small masses were suspended by delicate quartz fibres in a tube from which the air could be pumped so as to leave a very high vacuum. Special precautions were taken to avoid any possible errors which might arise from electrostatic or magnetic actions, and the pressure of the residual gas left in the exhausted tube was such that the effects of radiation pressure and convection were a minimum. The pressures used were about 14 mm. and 20×10^{-6} mm. The former pressure has the advantage that the residual air is sufficient to damp the torsion system in an efficient manner; to overcome the trouble arising from the lack of damping at the very low pressure, the ingenious plan was adopted of making the suspended weights in the form of chains, so that the vibrations might be damped out by the

rolling friction of one link upon another. The attracting masses were large leaden weights, which contained a heating coil of nichrome for raising their temperatures.

Reference should be made to the original paper for an account of all the details of the apparatus finally adopted as the result of experiments extending over several years. The final series of experiments undoubtedly point to an increase in the Newtonian constant with temperature, for no point seems to have been left unexamined which might impair the validity of this conclusion.

The results obtained are so unexpected and the magnitude of the effect found is so large, that one naturally turns to consider to what extent fundamental conceptions are concerned, and whether any other evidence can be adduced to support or disprove this result. It may first be mentioned that Prof. J. H. Poynting and Mr. P. Phillips had previously made careful investigations into this question (*Proc. Roy. Soc.* ser. A, vol. lxxvi. p. 445, 1905) with negative results. The method adopted was to counterpoise a weight upon a balance and to vary its temperature over a very wide range, the attracting mass being the earth. The difference between these experiments and those of Dr. Shaw is that in the one case the small (attracted) mass, in the other the large (attracting) mass was heated. If then the attraction between two masses M, m of respective temperatures t, t' and distance apart d is of the form

$$f = G\left[1 + a\frac{Mt + mt'}{M + m}\right]\frac{Mm}{d^2}$$

the two results are not in opposition. Other forms for the attraction can of course be found which will reconcile the two series of experiments. Another mode of expressing the results is to say that the attraction of one body by a second depends upon the temperature of the attracting body only, but not upon that of the attracted body.

The astronomical aspect of the question has been discussed in *The Observatory*, vol. xxxix. p. 318, 1916. It is known that the planets have widely differing temperatures, and the accuracy with which Kepler's laws for the motion of the planets around the Sun are obeyed proves that the attraction is independent of the temperature of the attracted (small) body, in agreement with the result found by Poynting and Phillips. But it seems

that astronomical evidence can throw very little light on Dr. Shaw's conclusion that the attraction depends upon the temperature of the *attracting* mass. All astronomical measurements of mass are made by measuring the attracting power; and it may be said that it is only an assumption to say that this is proportional to the inertia, although experiments have been made which tend strongly to support this assumption. Dr. Shaw in *Nature* (vol. xcvii. p. 401, 1916) indicates one result which may throw some light upon the subject, viz., as pointed out by Poincaré, that the mass of Jupiter as derived from the orbits of its satellites, as derived from its perturbations of the large planets, and as derived from the perturbations of the small planets has three different values.

The final confirmation or otherwise of the result will no doubt come from further experiments. Meanwhile, a whole series of interesting questions are opened up; *e.g.*, Newton's principle of the equality of action and reaction may be upset. The results when finally established will help to decide between some of the many theories of gravitation which have recently been formulated. The difficulty in connection with such theories has been that, as almost all gravitational experiments have given negative results, it has been impossible to test them. We await with interest further experiments in this direction.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Electrolytic Dissociation. The Degree of Dissociation and the Activity of an Ion.—As is well known, the relation $\gamma=\lambda_v/\lambda_\infty$ which was first deduced by Arrhenius, and which gives the degree of dissociation γ of an electrolyte in terms of the equivalent conductivity λ_v at the dilution v and the equivalent conductivity λ_∞ at infinite dilution, is the fundamental equation in the theory of electrolytic dissociation. This very simple relation contains the assumption that the mobilities of the ions are independent of the concentration or dilution of the system. Further investigation has shown, however, that this assumption is not correct. The mobility varies with the dilution. Since it seemed likely that the viscosity of the system would affect the mobility, the original expression of Arrhenius was modified in this sense, the resulting expression

being $\gamma = \frac{\lambda_v \eta_v}{\lambda_\infty \eta_o}$, where η_v denotes the viscosity of the solution at dilution v and η_o is the viscosity of the pure solvent. In general the viscosity of a solution is greater than the viscosity of the solvent, so that the correction term introduced assumes that the mobilities of the ions are smaller at moderate concentrations than they are at great dilution. By making use of this expression, data have been found for the degree of dissociation or ionisation of a considerable number of acids, bases, and salts. Two strong electrolytes very carefully investigated are hydrochloric acid and potassium chloride. Judging from conductivity measurements, the degree of dissociation of HCl is distinctly greater than that of KCl at the same concentration. Associated with this is the fact that the mobility of H ion is much greater than that of any other ion, and this very naturally raises the question whether the larger ionisation of HCl is not more apparent than real. Recently G. N. Lewis has even suggested that the degree of ionisation of HCl is to be taken as identical with that of KCl as given by the conductivity expression. The doubt which exists regarding the accuracy of the conductivity expression is accentuated by the lack of agreement between the γ values obtained by this method and those obtained by measurements of the electromotive force of concentration cells. One would have expected to obtain values from electromotive force measurements for the degree of ionisation and the concentration of an ion (the latter being simply the product of the degree of dissociation into the total concentration of the electrolyte) in agreement with those obtained from conductivity measurements. The type of cell used in an inquiry of this kind may be represented thus :

silver electrode	silver nitrate concentration c_1	silver nitrate concentration c_2	silver electrode

It can be shown thermodynamically, that the E.M.F. of such a cell depends upon the concentration, or, as we shall call it, the " effective concentration " of the silver ions, the E.M.F. being proportional to the logarithm of the ratio of the effective concentration of the ion in c_1 to the effective concentration of the ion in c_2. If we denote by a the factor which connects

the total concentration of the dissolved salt with the effective concentration of the ions, we can write:

$$\text{E.M.F.} \propto \log \frac{a_1 c_1}{a_2 c_2}$$

where a_1 is the value of a in the solution c_1, and a_2 is the value of a in the solution c_2. As this expression stands it only allows us to calculate the *ratio* of a values for two different concentrations of the salt. In order to get absolute values of a we choose some solution of very considerable dilution, say N/1000, and assume that the real dissociation is so large that the value of a given by E.M.F. measurements is identical with the value of γ given by conductivity measurements. From this starting point it is easy to work out a whole series of a values for different dilutions of the salt. On doing this, however, as will be seen in a later table, the list of a values diverges from the list of γ values, in the sense that the a values become smaller than the γ values as the concentration of the electrolyte increases. The term a has been called by G. N. Lewis the "activity coefficient" of the ion and the term (ac) the "chemical activity" of the ion, for it is certainly this term, as measured by E.M.F., which possesses the thermodynamic significance of chemical activity. Incidentally it may be mentioned that even the employment of the a values in place of the γ values does *not* suffice to bring the dissociation of a strong electrolyte into agreement with the Ostwald dilution law, so that the anomaly of strong electrolytes still remains, *i.e.* a strong electrolyte does not obey the law of mass action, whether conductivity results are correct or not.

We are therefore faced with two problems—(1) Does the conductivity method give an accurate measure of the actual concentration of the ions? and (2) Why does the concentration of the ions as determined by conductivity differ from the effective concentration as determined by electromotive force? In considering these important questions attention must be drawn to a paper by J. H. Ellis entitled "Free Energy of Hydrochloric Acid in Aqueous Solution" (*Journ. American Chem. Soc.* **38**, 737, 1916), which does not indeed solve the difficulties, but goes a long way towards giving a clear presentation of the position reached at the present time.

In this summary we shall restrict ourselves to a com-

parison of γc values obtained from conductivity and ac values obtained from electromotive force. The experimental work consists of measurements of the E.M.F. of concentration cells containing HCl in aqueous solution at various concentrations and temperatures, the cells being of the type

Hydrogen electrode | HCl, Hg_2Cl_2 (solid) | Mercury electrode.

The determinations have been carried out with extreme care and cover a wide range of HCl concentrations. If E_1 is the E.M.F. of the above cell when the total concentration of HCl is c_1 and E_2 is that obtained when the HCl concentration is c_2 then $E_2 - E_1 = 2RT \log \frac{a_1 c_1}{a_2 c_2}$. This equation involves the assumption that the activity of the hydrogen ion is equal to that of the chlorine ion in each of the hydrochloric acid solutions. If this is not valid then the calculated values of ac represent the square root of the product of the activities of the two ions. In the following table are given the most accurate data so far obtained for the conductivity ratio $\frac{\lambda_v \eta_v}{\lambda_\infty \eta_o}$, *i.e.*, γc, and likewise for ac from E.M.F. measurements. It has been provisionally assumed that at the lowest concentration employed, viz. 0·00167 molar, the a is substantially the same as the γ, this assumption being necessary, as already pointed out, in order to obtain a series of absolute values for a. The conductivity values are partly those of Bray and Hunt, Noyes and Falk, and Kohlrausch.

ACTIVITY COEFFICIENTS AND CONDUCTIVITY-VISCOSITY RATIOS FOR HCl AT 18° C.

Moles HCl per 1,000 grms. H_2O.	Activity coefficient a.	γ	Moles HCl per 1,000 grms. H_2O.	Activity coefficient a.	γ
0·001677	0·988	0·988	0·20	0·818	0·909
0·002	0·987	0·988	0·30	0·804	0·903
0·005	0·971	0·981	0·50	0·793	0·890
0·010	0·947	0·972	0·75	0·820	0·870
0·020	0·918	0·962	1·00	0·857	0·845
0·050	0·874	0·944	2·00	1·086	—
0·100	0·843	0·925	4·484	2·228	—

It will be observed that with increasing concentration the values of both a and γ fall, but a falls the more rapidly. That is, the effective concentration of an ion is less than the value

given by the conductivity method. Thus in the case of a deci-molar solution the respective values are 0·843 and 0·925. Further it will be noticed that the γ values continue to fall steadily as far as they have been determined, *i.e.* up to one-molar, but that the α values fall to a minimum at about half-molar, thereafter increasing rapidly, passing through unity and ultimately rising to 2·228. What the actual physical meaning of this may be, it is difficult to say. (*To be continued.*)

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School.

In a lecture delivered recently before the Chemical Society, Prof. Hopkins chose for his subject "Newer Standpoints in the Study of Nutrition," and some of the more important points dealt with are contained in the following summary.

The protein molecule is not absorbed by the body as such and all processes of oxidation or molecular reconstruction are preceded by a complete hydrolysis into amino acids. The proof of this is two-fold; in the first place examination of the blood of anæsthetised animals before and after a meal shows that nitrogenous food is conveyed by the blood to the tissues mainly in the form of amino acids, and secondly, it has been repeatedly shown that animals may be kept alive on a diet of amino acids as their sole source of nitrogenous food, and that young animals will grow on the same diet. Furthermore, if proteins such as albumoses or peptones which we consume in ordinary food are injected into the blood-stream, they are rapidly excreted by the urine, showing that they are foreign to the blood, whereas amino acids injected into the blood-stream are metabolised quite normally. In starvation the life of the animal is maintained by drawing upon the tissue proteins for its supply of nitrogenous material, but here again, on examining the blood, one finds not the unchanged tissue proteins, but the amino acids arising from these by hydrolysis by means of autolytic ferments.

The fact that the animal is able to maintain itself on an amino acid mixture enables one to ascertain experimentally the relative value of individual amino acids and whether certain amino acids are indispensable to life or are possessed of any particular function in nutrition by virtue of their molecular structure. In all experiments in which animals are fed on

amino acids it is essential to include in the mixture a minute quantity of vitamine in the form of a nitrogen-free alcoholic extract of fresh milk. Casein hydrolysed by boiling for forty hours with 20 per cent. sulphuric acid forms a suitable amino acid mixture, but is deficient in cystine and tryptophan, the latter substance being destroyed by heating with acid. Accordingly, the hydrolysis mixture has to be treated with small quantities of these two acids in order to make it suitable for supporting growth. It has been found that if tryptophan is purposely withheld loss of body weight results almost immediately, and the animal dies more or less rapidly. The explanation offered for this phenomenon is that the body is unable to synthesise the indole ring requisite for the protein molecule from other substances than tryptophan. Similar effects are produced by the removal of the two amino acids histidine and arginine, and it is argued that this means that the body is unable to synthesise the guanidine and iminazole complexes from other sources than these two acids. In striking contrast to these observations it is found that the removal of two other acids, glutamic and aspartic, has no harmful effect, and this in spite of the fact that they are important constituents of casein, seeing that they make up no less than 28 per cent. of the weight of the molecule. It may however be assumed that the body is in this case capable of making up the deficiency in such comparatively simple molecular structures by synthesis from fats or carbohydrates in the presence of ammonium salts, and that the synthesis can be maintained at a sufficient rate to keep pace with the requirements of the body. As illustrating the fact that one amino acid may to a certain extent replace another closely allied acid it has been found that growth could be maintained on an amino acid mixture containing no tyrosine but a small quantity of phenylalanine, which differs from the former acid only by a single atom of oxygen, as may be seen from the formulæ

$$\underset{\text{Tyrosine.}}{C_6H_4OH.CH_2CHNH_2COOH} \quad \text{and} \quad \underset{\text{Phenylalanine.}}{C_6H_5CH_2.CHNH_2COOH}$$

On the other hand gelatine, which contains neither tryptophan nor tyrosine, fails entirely to support life, but the fact that on addition of tryptophan life could be maintained points to the fact that the body contains some mechanism for synthesising

the benzene nucleus required for tyrosine although it is quite unable to produce indole.

With regard to the question as to whether particular amino acids are associated with any special function, some such connection has been established by the author between arginine and histidine on the one hand, and purines on the other, for he has been able to show in the case of rats that the excretion of allantoin fell off markedly when these two acids were removed from the diet, but rose again as soon as they were included. The relation between these particular acids and the purines is not far to seek on chemical grounds, since they are characterised by the peculiarity, not found in any other amino acids, of having a carbon atom between two nitrogen atoms, which arrangement is of course typical of the purines.

The causation of the staleness of bread forms the subject of an investigation by J. R. Katz (*Zeitsch. Physiol. Chem.*, 1915, **95**, 104). The staleness of bread according to this author is not merely due to drying, since bread kept in an atmosphere saturated with moisture also becomes stale. What actually occurs is that the starch grains harden and become less able to absorb water and the soluble polysaccharides contained in them are rendered insoluble, with the result that the bread becomes harder and less sweet. The crumbly nature of the stale bread is due to the transfer of water from the starch grains as they harden to the gluten, which lessens the adherence of the particles to each other. Exactly the reverse process takes place in baking: the absorptive power for water and the proportion of water soluble polysaccharides is increased. Equilibrium is apparently attained at the baking temperature, and as the temperature falls the reaction goes in the reverse direction. In support of this view it has been shown that bread may be kept fresh for twenty-four hours if maintained at a temperature of 60° C.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., F.G.S., University, Glasgow.

Regional and Stratigraphical Geology.—Two papers on Welsh stratigraphy appear in the current *Quarterly Journal*. The geology of the district between Abereiddy and Abercastle (Pembrokeshire) is dealt with in detail by Dr. A. H. Cox (*Quart. Journ. Geol. Soc.* 1916, **71**, 273). The region is extensively folded, and a complete sequence of Ordovician rocks from the

Arenig to the Glenkiln is exposed. Contemporaneous igneous rocks appear in the *Tetragraptus* shales and in the *Dicranograptus bifidus* beds. Prof. O. T. Jones and W. J. Pugh describe the geology of the district around Machynlleth and the Llyfnant valley (*ibid.* 343). The rocks here range from the Hartfell to the Valentian, and are arranged in large corrugated folds which are accompanied by strike-faults parallel to the axes of folding.

An outline of the geology of Prince Charles Foreland, Spitzbergen, is given by R. M. Craig (*Trans. Edinburgh Geol. Soc.* 1916, **10**, 276). He describes folded grits and quartzites of Silurian age, a coarse conglomerate probably of Devonian age very like some of the Old Red Sandstone conglomerates of Scotland, and Tertiary sandstones and shales the age of which was determined by plant remains collected by Dr. W. S. Bruce.

C. R. Stauffer publishes a detailed memoir on the Devonian of South-western Ontario (*Memoir* 34, *Geological Survey of Canada*, 1915, 341 pp.). It is mainly a richly fossiliferous limestone formation, yielding important economic products such as petroleum and gas, building stones, and lime for Portland cement.

The study of Cordilleran geology is furthered by yet another memoir, " The Geology and Ore-deposits of Rossland, British Columbia," by C. W. Drysdale (*Memoir* 77, *Geological Survey of Canada*, 1915, 317 pp.). The oldest formation is believed to be Carboniferous, but the bulk of the area is occupied by igneous rocks of Jurassic and Tertiary age which are associated with two main periods of mineralisation.

Petrology.—By means of volumetric and chemical analyses, aided by determinations of density and grain, A. C. Lane has demonstrated gravitative differentiation in an extrusive Triassic basalt from Nova Scotia (*Trans. Amer. Inst. Min. Eng.* February 1916, 535). There is a concentration of felsic constituents toward the top of the flow and of mafic constituents towards the base. This result is in accordance with recent experiments by Bowen and Anderson on artificial melts.

The pitchstones of Mull are described by E. M. Anderson and E. G. Radley (*Quart. Journ. Geol. Soc.* 1916, **71**, 205). They are divided into porphyritic and non-porphyritic types respectively, which, with their stony equivalents, are classed as two new types of rocks, *leidleite* and *inninmorite*. The boundary between crystalline and glassy facies is very sharp, and devitri-

fication is connected with the escape of water from the rock very soon after solidification.

The petrography of Angola has been dealt with recently in two papers. A. Holmes (*Geol. Mag.* (VI) **2**, 1915, 228, 267, 323, 366) describes many interesting alkaline rocks including ægirine-riebeckite-granite, nepheline-syenite, nepheline-phonolite, nepheline-monchiquite, and a camptonitic tinguaite which is identical with Marshall's ulrichite from New Zealand. These rocks come from Loanda, the northern province of Angola. The petrology of Benguella, the central province, has been described by the author from a rock collection made by Prof. J. W. Gregory (*Trans. Roy. Soc. Edinburgh*, 1916). The rocks include a probably Archæan basement consisting mainly of igneous gneisses, intruded by a charnockite series identical with that of Peninsular India, and also by a curious series of hornfelsed cassiterite-bearing porphyries. These rocks are intruded by batholiths of non-foliated granites and granodiorites, the latter with cassiterite. A thick series of ancient rhyolites occur in the Oendolongo Mountains. In the Ochilesa district there is a series of alkaline rocks including nepheline-syenite, sodalite-syenite, akerite, shonkinite, tinguaite, solvsbergite, and ouachitite, which are probably of Cretaceous or Tertiary age, and comparable with the similar series of Loanda described by Holmes.

H. S. Washington has made a quantitative study of Holland's charnockite series from Madras (*Amer. Journ. Sci.* 1916, **41**, 323), and has corrected the earlier unsatisfactory chemical analyses. He shows that there are at least five or six comagmatic regions of ancient plutonic rocks of the "charnockite" character. (A seventh has been added in Angola, see above.)

The topaz-bearing rocks of Gunong Bakau, Malaya, intruded as veins into granite, have recently been described as of primary igneous origin, but W. R. Jones (*Geol. Mag.* (VI), 1916, **3**, 255) brings forward strong reasons for the secondary origin of the topaz and cassiterite in these rocks, and for their correlation with the similar occurrences of other tinfields.

A detailed account of the volcanic rocks of South-eastern Queensland, with twenty-five new chemical analyses, is given by H. C. Richards (*Proc. Roy. Soc. Queensland*, 1916, **27**, 105–204). These rocks consist of rhyolites, trachytes, dacites, andesites, and basalts, and are believed to be of Kainozoic age.

There is an upper and lower series of basaltic rocks, with an intermediate series of alkaline lavas, a sequence which is paralleled in the Kainozoic volcanic rocks of New South Wales and Victoria.

In a useful paper on the application of petrological and quantitative methods to stratigraphy (*Geol. Mag.* (VI), 1916, **8**, 105, 163) P. G. H. Boswell shows that detrital mineral assemblages may be of considerable value for stratigraphical correlations within limited areas. For this purpose it is necessary to aim at a knowledge of the mineral and mechanical composition of all sedimentary rocks of the British geological column. A contribution to this knowledge has been made by G. M. Davies, who has determined the mineral constituents of a large number of rocks from the Croydon area (*Proc. and Trans. Croydon Nat. Hist. and Scientific Soc.* 1915–6, 53). Conclusions as to the source of supply and conditions of deposition are withheld until more information is forthcoming relating to the composition of the rocks over a much wider area.

Mr. B. Smith describes ball or pillow-form structures in the Llandeilo and Bala sandstones of Denbighshire (*Geol. Mag.* (VI), 1916, **8**, 146). These are associated with bucklings and foldings, and are explained as due to the internal readjustments of freshly and unevenly deposited sediments under the control of gravity.

Economic Geology.—Fluor-spar forms the subject of the fourth report of the Geological Survey on the Mineral Resources of Great Britain. This mineral occurs in greatest abundance in the zinc and lead veins of Derbyshire and Durham. The output for many years has been in excess of home requirements and much fluor has been exported, more than half the production going to the United States.

A full description and discussion of Florida phosphates is given by G. C. Matson (*Bull.* 604, *United States Geological Survey*, 1915, 101 pp.). They are of Tertiary age and of secondary origin, having been redeposited by mechanical or chemical action.

The sulphur deposits of Sicily are described by W. F. Hunt (*Econ. Geol.* 1915, **10**, 543). These occur in isolated basin-like areas and are unquestionably sedimentary, as they are intercalated with thin layers of bituminous salty shales, underlain by tripoli, and covered by massive deposits of gypsum. Hunt believes that the sulphur has been produced by the bacterio-

logical reduction of sulphates in land-locked lagoon areas, with occasional influxes of water from the ocean to reinaugurate the cycle of sulphur deposition.

An important memoir on the oolitic iron-ores of Wabana, Newfoundland, is contributed by A. O. Hayes (*Memoir 78, Geological Survey of Canada*, 1915, 163 pp.). The ore occurs in shales and sandstones of Lower Ordovician age and is believed to be a primary bedded deposit, the iron content being present in the sediments at the time the series was laid down. The oolites were formed out of the fine-grained, unconsolidated, ferruginous sediments of the sea floor.

A very full discussion of the origin of the lead and zinc ores of the Joplin (Missouri) area is given by C. E. Siebenthal (*Bull. 606, United States Geological Survey*, 1915, 283 pp.). These are of great economic importance, and form a conspicuous example of the occurrence of sulphide ores in a region remote from igneous activity. The theory is elaborated that the Joplin ores were segregated from disseminated lead and zinc minerals in the Cambrian and Ordovician rocks of the Ozark uplift, by the agency of circulating artesian waters of alkaline-saline sulphuretted type.

PALÆOBOTANY IN 1915. By Marie C. Stopes, D.Sc., Ph.D., University College, London.

Palæobotany is a science of few professors, though there are, of course, very large numbers of both geologists and botanists whose researches contribute to the body of facts encompassed by the science. Three years ago it might have been said truly that there were less than two dozen notable palæobotanists all the world over, and of this small number no fewer than four of the most prominent have died recently. Germany has lost both her senior men; Potonié, Professor in Berlin, died shortly before the war, and the aged and still more famous Count Solms-Laubach died in Strassburg in November (1915): there are no men of equal standing left in Germany. France has also lost two of her seniors within a few months. In René Zeiller, Professor in Paris, not only France but the whole world has lost one of the greatest palæobotanists who have ever lived. He was shortly followed by Octave Lignier, Professor in Caen, famous for his admirable work on *Bennettites*. In this country we have lost Prof.

Gwynne-Vaughan, a younger man who, jointly with Dr. Kidston, had contributed a series of very valuable papers on the anatomical study of fossil ferns.

General Palæobotany.—Appropriately the first work to be considered is a booklet by the late Prof. Zeiller, published as one of a series on French Science (Libraire Larousse, Paris, 1915). In this is given a short account of the most famous French palæobotanists—and no country has done more for the science than France—with portraits and an historical bibliography. Not only has France had great palæobotanists, but the country has been exceptional in availing herself of, and rewarding, their services. Zeiller justifiably concludes with the words: "La France est ainsi l'un des pays où les indications fournies par la paléobotanique ont été le mieux utilisées au profit de l'exploitation des mines."

Curiously enough, in this year of war, the most important general work from Germany is a photographic reproduction of the great Frenchman, Brongniart's, classic and still indispensable *Histoire des Végétaux fossiles*, first published in Paris between the years 1828–37.

As I write I have before me more than one hundred volumes, papers, and titles of papers (some of which have not yet reached this country) all published in 1915. It will be necessary, therefore, to select for mention only a small number of these.

Stratigraphic Palæobotany.—The plants of the Devonian of Norway are dealt with by Prof. A. G. Nathorst, well illustrated with eight plates (*Bergens Museums Aarbok*, nr. 9, 1914–15). Dr. E. A. N. Arber and R. H. Goode describe some fossils from the Devonian of North Devon in the *Proc. Cambridge Phil. Soc.* vol. xviii.

Work on the Carboniferous period is naturally the most abundant. Of this the largest and most important publication is a joint work by Dr. R. Kidston of Scotland and Dr. W. J. Jongmans of Holland, dealing with the Calamites of Western Europe. In some respects this is the most notable publication of the year. The first instalment consists of 158 amazingly superb quarto plates of a number of species, illustrated entirely by photographs. Of a single species over 38 plates are given, thus illustrating its variations, and making possible a visual image of its form which has never been

attained before. The publication is an official Dutch production: and that Holland, a small country in which collieries have played no such national part as they have with us, should possess a Government supporting such palæobotanical work while our own does not, ought to shame us, so much of whose wealth for long past has been derived directly from the Carboniferous flora, preserved as coal. The Dutch National Herbarium publishes some more of Dr. Jongmans' invaluable bibliographic work in his "List of the Species of Calamites"; the same author publishing also two sections on the Equisetales in the extensive *Fossilium Catalogus*, II. *Plantæ*, with Junk in Berlin. A. Carpentier records *Whittleseya* from the North of France (*Bull. Soc. bot. France*, vol. lxi.); G. Depape and A. Carpentier describe a number of fossil seeds from the same region (*Rev. Gén. bot.* vol. xxvii.); E. Bureau publishes an appendix to his important *Flore fossile de la Basse Loire*; and M. Lindsey describes the branching of *Bothrodendron* (*Ann. Bot.* vol. xxix.). A posthumous work of Prof. Potonié's on the "Diathermie einiger Carbon-Farne" appears in the *Beihefte bot. Centralbl.* vol. xxxii., and there are several minor German papers on Carboniferous plants.

A number of mining and geological papers give lists or short references to the associated fossil plants; important among these is the large volume of E. Erdmann, *De Skånska stenkolsfälten* (Sveriges geol. Undersök. ser. c. 1911–15), and W. J. Jongmans' "Paläobotanisch-stratigraphische Studien im Niederländischen Carbon" (*Arch. f. Lagerstättenforsch.*).

Special work on Coal has been done by Prof. Zalessky (*Mém. Com. Géol. Petrograd*, N.S. livr. 139), which was reviewed in Science Progress last quarter (p. 73). James Lomax published a well-illustrated paper on the microscopic structure of coal (*Trans. Inst. Mining Engineers*, vol. l.), and Prof. E. Jeffrey discussed the Origin of Coal (*Journ. Geol.* vol. xxiii.). A second and enlarged edition of Drs. Strahan and Pollard's work on *The Coals of South Wales* was published by our Geological Survey (see also Science Progress, p. 73).

The nomenclature of the Carboniferous, Permo-Carboniferous, and Permian rocks of the southern hemisphere was considered at length in the Report of the British Association for the year, published with useful correlation tables. F. Pelourde considered the geological significance of beds con-

taining *Schizoneura, Cladophlebis, Dictyophyllum* and other forms (*Bull. Soc. Geol. France*, ser. 4, vol. xiv.).

There is no outstanding work on the JURASSIC, but there are several interesting papers, principally Russian. Prof. A. Kryshtofovich describes plant remains from the lake deposits of Transbaikalia (*Mém. Soc. imp. russ. Minéral*); and also from Amurland (*Trav. Mus. Géol.*); and A. Lesnikow has a paper on "Plantes Jurassiques du Caucase" (*Bull. Comm. Géol.* 34). Our scanty knowledge of the Southern hemisphere is augmented by A. B. Walkom's *Mesozoic Flora of Queensland* (Publ. no. 252, Queensland Geol. Surv.).

On the CRETACEOUS the bulkiest work is the second volume of M. C. Stopes' *Catalogue of the Mesozoic Plants in the British Museum—2, The Lower Greensand (Aptian) Plants of Britain.* This gives a complete account of all that is known of the fossil flora of this age, and describes a number of new species, mostly with anatomical details, illustrated by plates and text figures (see SCIENCE PROGRESS review, p. 167). Dr. E. W. Berry considers the age of some American deposits of Upper Mesozoic deposition (*Bull. Geol. Soc. Amer.*, vol. xxvi.). Fossil plants, and a consideration of Cretaceous and Tertiary Coal, are dealt with by A. L. Beekly in his comprehensive work on the *Geology and Coal Resources of North Park, Colorado* (Bull. 596, Geol. Surv. U.S.A.).

There are, as usual, a number of small papers on TERTIARY fossil plants, notably from America, the most interesting of which are E. W. Berry's work on the Mississippi Bluffs (*Proc. U.S. Nat. Mus.* vol. xlviii.) and the same author's work on *Erosion Intervals in the Mississippi Eocene* (Prof. paper 95—F, U.S. Geol. Surv.). The Russians also contribute some papers on the Tertiary, particularly Prof. Kryshtofovich (*Bull. Acad. imp. Sci. Petrograd*). The Tertiary flora of Ellesmere Land is amplified by Prof. A. G. Nathorst's work on the Norwegian Arctic Expedition's Report, No. 35. By far the most important work on Tertiary plants, however, is by the two English authors, C. and E. M. Reid, which is sumptuously published by the Dutch Government, *The Pliocene Floras of the Dutch-Prussian Border* (Med. Rijksopsp. Delfstoffen, No. 6). This is the most useful and authoritative work on the Tertiary flora which has appeared for many years.

Family Histories.—The ANGIOSPERMS are dealt with in a

few isolated papers, chiefly by Dr. E. W. Berry, who publishes "The Origin and Distribution of the Family Myrtaceæ" (*Bot. gaz.* vol. lix.); "A Species of Copaifera from the Texas Eocene" (*Torreya*, vol. xv.); and "An Eocene Ancestor of the Zapodilla" (*Amer. Journ. Sci.* vol. xxxix.). K. Nagel deals with the Juglandaceæ in Junk's *Fossilium Catalogus*, II. *Plantæ*; while Prof. Kryshtofovich has a memoir on "The Butternut (*Juglans cinerea* L.) from fresh-water deposits of the province of Yakoutsk" (*Mém. Comité Géol.* livr. 124, Petrograd). Sinnott and Bailey discuss in a very general way the early history of the Angiosperms (*Amer. Journ. Bot.*). M. C. Stopes in the *Lower Greensand Flora* (*ante*) describes the anatomy of very early members of the group.

The Higher Gymnosperms are dealt with in a number of papers elsewhere mentioned, particularly in those touching on wood anatomy. R. Holden describes the details of a Jurassic wood from Scotland (*N. Phytologist*, vol. xiv.). L. L. Burlingame in "The Origin and Relationships of the Araucarians" (*Bot. Gaz.* vol. lx.) goes carefully into the palæobotanical evidence for the various theories on the phylogeny of the group, which affords a useful general summary of recent work.

The Bennettiteæ are dealt with by F. Pelourde (*Prog. Rei bot.* 5), in his general article "Les progrès réalisés dans l'étude des Cycadophytes de l'époque secondaire." H. H. Thomas describes a specimen of the male flower of *Williamsonia* from the Paris museum (*Proc. Cambridge Phil. Soc.*, vol. xviii.); Dr. D. H. Scott popularly re-describes the famous Isle of Wight *Bennettites* (*Proc. Hampshire Field Club*); and F. Krasser records male flowers of *Williamsonia* from the L. Lias of Steierdorf (*Ans. K. Akad. Wiss. Wien*, vol. lii.). But the important contribution to this group is the work of H. H. Thomas, who describes a new genus of fructifications, "On Williamsoniella, a new type of Bennettitalean flower" (*Phil. Trans. Roy. Soc. London*, ser. B, vol. ccvii.). From the associated foliage and stems, together with the fructifications, he deduces the whole appearance of the plant, of which he gives a restoration showing its suggested habit. The outstanding features of the fructifications are their extremely small size and relatively simple organisation. This description affords the basis for a general comparison of members of the group.

Work on the Pteridosperms and lower vascular families

is chiefly anatomical (see below). On fossil THALLOPHYTA little is ever done, but Dr. D. Ellis describes some "Fossil Micro-organisms from the Jurassic and Cretaceous Rocks of Great Britain" (*Proc. Roy. Soc. Edinburgh*, vol. xxxv.), including fungi with hyphæ and spores, and also several supposed bacteria from various horizons.

Anatomy.—Plant cuticles have extraordinary resistent powers, and often the epidermis of an otherwise disintegrated plant is well preserved. As the shape and arrangement of the epidermis cells sometimes form decisive criteria in determining species, considerable attention has been paid to their study recently, and, as usual, there is a sheaf of small papers on the subject this year. Dr. T. G. Halle in "Some Xerophytic Leaf-Structures in Mesozoic Plants" (*Geol. Fören. Stockholm*) deals principally with epidermal structures; cuticle preparations are also important in Ernst Antev's paper "Einige Bemerkungen über *Cycadopteris Brauniana*, Zingo and C. *Zeilleri* n.sp." in the same journal. E. Bayer describes "Mikroskopische Præparate der Kutikula der fossilen Pflanzen *Sclerophyllum alatum*" (*Bull. Kongr. böhmisch Natf.*). Dr. W. Gothan has a paper on the epidermis of Carboniferous Neuropterids (*Jahrb. K. preuss. Geol. Landesanst.*) and on the methods of dealing with them (*Monatsber. deutsch. Geol. Ges.*). E. M. Barbour records the discovery of large numbers of small sheets of cuticles supposed to be Cordaitean, in the Eurypterid beds of Nebraska (*Amer. Journ. Sci.* vol. xxxix.). The peculiarly preserved sheets of cuticle of the "paper coal" of Russia play an important part in the beautifully illustrated memoir by Prof. Zalessky on *Lepidodendron Olivieri* and *L. tenerrimum* (*Mém. Comité Géol. Petrograd*, livr. 125).

Dealing with the petrified internal anatomy, Dr. Kidston and the late Prof. Gwynne-Vaughan conclude their valuable series on the Osmundaceæ (*Phil. Trans. Roy. Soc. Edinburgh*, vol. l. part 2) which was published at the end of 1914, but came into circulation in 1915 and is mentioned as the last work of a very significant anatomical and phylogenetic series. O. A. Derby (*Amer. Journ. Sci.* vol. xxxix.) gives illustrations showing the complex vascular anatomy of the stem described as *Tietea singularis* by Count Solms-Laubach. Work on the Pteridosperms is represented by Dr. D. H. Scott's paper on the Heterangiums of the British Coal Measures, and by the

preliminary account of an interesting new *Zygopteris* of an early age, both at the Manchester meeting of the British Association; and by N. Bancroft's detailed and well-illustrated work on the anatomy of *Rachiopteris cylindrica* Will. (*Ann. Bot.* vol. xxix.).

The presence of foreign pollen in fossil seeds is discussed by Prof. F. W. Oliver (*N. Phytologist,* vol. xiv.) and the conclusion reached that on the whole the pollen in a petrified seed is that of the corresponding microsporangia.

The year 1915 is surprisingly rich in handsomely produced, comprehensive, and important monographs which consolidate known data and contribute many new facts to the science of Palæobotany. No outstanding new theory appears to have been advanced. An interesting feature of the year's output is the relatively large number of papers from Russia, which in the last few years has become an active centre of palæobotanical work.

ZOOLOGY. By Chas. H. O'Donoghue, D.Sc., F.Z.S., University College, London.

Protozoa.—An interesting account of the "General Biology of the Protozoan Life Cycle" is furnished by Calkins (*Amer. Nat.* May 1916). Experiments appear to show a gradual chemical differentiation of the protoplasm with age. This senility may be overcome by nuclear reorganisation, conjugation, encystment and even cell division. "The Structure and Development of a Myxosporidian Parasite of the Squeteague *Cynoxion regalis*" is described by Davis (*Jour. Morph.* vol. xxvii. 1916). The parasite, *Sphærospora dimorpha,* is abundant in the urinary bladder, and has two forms: one is disporous, and the other polysporous. The spores are apparently the same in each case. As is well known, the alimentary canals of goats and sheep have a rich protozoan fauna, some of which pass through it in a resting, encysted condition, undergoing their active phases in moist dung. These forms are dealt with at length by Woodcock (*Phil. Trans. Roy. Soc.* vol. ccvii. B) in "Observations on Coprozoic Flagellates; together with a suggestion as to the Significance of the Kinetonucleus in the Binucleata." It is pointed out that there is a close relation between the binucleate condition and the absence of syngamy. Incidentally too the author

is led to the conclusion that Flagellates and Amœbæ are not the factors inhibiting bacterial activity in well-manured soils.

Invertebrata.—" Some Nemertinea, Free-living Nematoda, and Oligochæta from the Falklands " collected by Mr. Vallentin are recorded by Baylis (*Ann. Mag. Nat. Hist.* April 1916). In " The Development of *Paravortex Gemellipara* (*Graffilla gemellipara*, Linton) " (*Jour. Morph.* vol. xxvii. 1916) Ball states that when two or more embryos are found in a capsule, it is due to the inclusion of two or more eggs. Three germinal regions are recognisable in the blastula, a meso-ectoderm, a primary entoderm (whose cells act as vitellophags) and a secondary entoderm.

The question of the influence of the yolk in development has been investigated by Grave (*Jour. Morph.* vol. xxvii. 1916) " *Ophiura brevispina*—II. An Embryological Contribution and a Study of the Effect of Yolk Substance upon Development and Developmental Processes." The great increase in the yolk content of the egg of *Ophiura brevispina*, to which its large size is due, has not disturbed its early developmental processes. The manner and rate of its segmentation and the structure of the blastula remain practically the same as in the egg of *Ophiocoma*, which is $\frac{1}{78}$th the size of the egg of *Ophiura*.

Bagnall gives " Brief Descriptions of new Thysanoptera " (*Ann. and Mag. Nat. Hist.* May 1916), " New *Tipulidæ* from the Malay Peninsula " and " A Third Species of the Genus *Elporia* Edw." are recorded by Edwards (*Ann. and Mag. Nat. Hist.* April and May 1916). Champion describes a series of new Coleoptera in " A new Genus of *Pythidæ* from the Falkland Islands " (*Ann. and Mag. Nat. Hist.* April 1916), " A new Genus of Anthicidæ from the Islands of Mysol and Waigiou " (*ibid.* May 1916), and " New Species of the Genus *Platomops*, Reitl. [= *Spithobates*, Champ.] from Tropical South America " (*ibid.* June 1916). Cockerell continues to pile up " Descriptions and Records of Bees " (LXXI. *ibid.* April 1916, LXXII. *ibid.* June 1916) and Turner " Notes on Fossorial Hymenoptera " (XXI. *ibid.* April 1916, and XXII. *ibid.* June 1916). A list with brief descriptions of the " Ants from British Guiana " comes from Crawley (*ibid.* May 1916) and " Notes on the Apidæ (Hymenoptera) in the Collection of the British Museum, with Descriptions of new Species " from Meade-Waldo (*ibid.* June 1916). Lepidoptera appear in the *Ann. and Mag. Nat. Hist.*

as follows: "Descriptions of New Species of Lepidoptera" and "Notes on the Synonymy of the Genus Ogyris" (May 1916); "Some new Lepidoptera from Siam and Africa" by Lord Rothschild and "A new Sphingid and little-known Butterflies from Africa" by Joicey (June 1916). In the April number of the same periodical are to be found "On a new Species of *Solpuga* from the Belgian Congo" and "On a new Variety of European Tick (*Dermacentor reticulatus var. aulicus*, var. nov.) both by Hirst and "Rhynchotal Notes—LIX." by Distant. Further links in the chain of evidence showing that the chromosomes furnish structures sufficient to serve as a morphological basis for the phenomena of heredity, variation, and evolution are provided in "Taxonomic Relationships shown in the Chromosomes of Tettigidæ and Acrididæ: V-shaped Chromosomes and their Significance in Acrididæ, Locustidæ, and Gryllidæ; Chromosones and Variation" by Robertson (*Jour. Morph.* vol. xxvii. 1916). The author finds the "number fundamental and constant, not only for all the cells of an individual, but likewise individuals of a species, the species of a genus, the genera of a sub-family, and if they are sufficiently closely related, even the sub-families of a family." Various collections of Crustacea are described in "Edriophthalma from South America" by Walker (*Ann. and Mag. Nat. Hist.* April 1916), "A new Species of the Amphipodan Genus *Hyale* from New Zealand" by Chilton (*ibid.* May 1916), "A collection of Freshwater Entomostraca made by Mr. G. W. Smith in Ceylon in 1907" (*Proc. Zool. Soc.*[1] April 1916) by Gurney, and "The Apterygota of the Seychelles" by Carpenter (*Proc. Roy. Irish Acad.* vol. xxxiii. 1916).

Vertebrata.—Specimens of *Tilapia nilotica* with an increased number of anal spines were described by Boulenger (*Proc. Zool. Soc.* April 1916). The apparatus for poisoning the wound inflicted by the large serrated spine at the base of the whip-like tail of the Sting-Ray has been investigated by Evans in "On the Poison Organ of the Sting-Ray (*Trygon pastinaca*)." A well-marked epithelial follicular gland is present, opening to the outside by small nipples. In his "Form and Growth in Fishes" Hecht (*Jour. Morph.* vol. xxvii. 1916) concludes that weight $= a \times (\text{length})^3$ where $a =$ condition of fish inde-

[1] In the case of papers in the *Proceedings of the Zoological Society* the date given is that when the paper was read, not when it was published.

pendent of sex. Comparison with other data indicates that in animals with indeterminate growth, the external form is established early in post-embryonic life, and is adhered to more or less therefrom ; but in animals with determinate growth, the external form changes during the growth period ; as soon as the form becomes constant growth ceases.

The third and last instalment of Boulenger's account of "On the Lizards allied to *Lacerta muralis*, with an Account of *Lacerta agilis* and *L. parva*" is given in *Proc. Zool. Soc.* (April 1916). A full discussion of the evolution of the colour pattern in these forms is given, and the author agrees in the main with Eimer, but carries the original pattern back to a much more primitive one. Moodie (*Jour. Morph.* vol. xxvii. 1916) finds that "The Structure and Growth of the Plesiosaurus Propodial" is essentially similar to that of mammalian limb bones. I. B. J. Sollas and W. J. Jollas (*Phil. Trans. Roy Soc.* vol. ccvii. B) have examined another skull of Dicynodon, and are enabled to add supplementary details regarding the occiput, palate, ethmoidal and otic regions.

Bate (*Proc. Zool. Soc.* May 1916) deals mostly with bird remains in her paper "On a Small Collection of Vertebrate Remains from the Har Dalam Cavern, Malta, with Note on a new Species of the Genus *Cygnus*." An incomplete sternum of a gigantic carinate bird from the Eocene of Nigeria is described by Andrews (*Proc. Zool. Soc.* May 1916).

Pocock writes "On some of the External Structural Characters of the Striped Hyæna (*Hyæna hyæna*) and related Genera and Species" (*Ann. and Mag. Nat. Hist.* April 1916), "On some of the External Characters of Cryptoprocta" (*ibid.* June 1916) and again "On the External Characters of the Mongooses (*Mungotidæ*)" (*Proc. Zool. Soc.* April 1916). "A new Rat from Tenasserim" is recorded by Thomas (*Ann. and Mag. Nat. Hist.* June 1916), who also discusses "The Races of *Dremomys pernyi*" (*ibid.* May 1916). In the same number of the *Annals and Magazine of Natural History* Blackler has a note "On a new Species of *Microtus* from Asia Minor. The Sitatungas (*Limnotragus*) of the Sesse Islands are divided into two races by Meinertzhagen (*Proc. Zool. Soc.* April, 1916), one on Bugalla Island similar to the form on the mainland, and another subspecies on Nkose Island. An interesting account of the skull of *Chrysochloris hottentota* and *C. asiatica* is given by

Broom (*Proc. Zool. Soc.* May 1916). Certain bones are described which are possibly the homologues of the tabular of the Therapsid reptiles, and also of the reptilian surangular. At the same meeting Woodward read a paper on the ramus of a marsupial mandible from Alberta, Canada. It obviously belonged to an opossum-like animal, and is termed *Cimolestes cutleri*. The old theory that the chin is developed in relation to articulation is rejected by Waterman in the "Evolution of the Chin" (*Amer. Nat.* April 1916), who regards it as due to the reduction of a once large lower jaw. By means of the reconstruction method, Esdaile has carried out an investigation "On the Structure and Development of the Skull and Laryngeal Cartilages of *Perameles*, with notes on the Cranial Nerves" (*Phil. Trans. Roy. Soc.* vol. ccvii. B). This contains also a comparison with the skulls of Cynodonts, Prototheria, *Trichosurus*, *Dasyurus* and the higher mammals.

General.—"An Experimental Determination of the Factors which cause Patterns to appear conspicuous in Nature" was conducted by Mottram (*Proc. Zool. Soc.* May 1916). The most conspicuous object was found to be a circular disc of black with a white centre. The remaining papers occur in the *American Naturalist*, so that it will only be necessary to give the month of publication. Muller has a series of three papers on "The Mechanism of Crossing Over" (Part I. April; Part II. May; and Part III. June 1916), the concluding part of which is still to come. As is now well known, the study of a large number of factors in the same animal has shown that these characters run in series, a phenomenon known as linkage. However, if a sufficient number of such linked series be taken it will be found that a certain group of one series changes over with a group from another series, this being termed crossing over. The object of these papers is to review the outstanding evidence relating to this phenomenon, and to describe a new experimental method of studying such separation. The results of the experiments give a demonstration of the fact that the factors behave as though they are joined in a chain. When interchange takes place, the factors keep together in sections, according to their place in line, and are not interchanged singly. By means of mazes, Bagg has investigated "Individual Differences and Family Resemblances in Animal Behaviour" (April 1916). It is found that there is a marked difference in individual

behaviour, but apparently no sex differences. A certain amount of resemblance is noticeable amongst individuals of the same litter, and considerable difference among different strains. Castle (June 1916) throws "New Light on Blending and Mendelian Inheritance" and deduces that the whole population will gradually resolve itself into relatively constant self-fertilising lines. But because of the slow but continuous blending which occurs, these pure lines will in a few generations form a complete gradation of forms connecting one parental mode with another. In the course of a series of breeding experiments, Little has to record "The Occurrence of Three Recognised Colour Mutations in Mice" (June 1916). It is the mutation from a grey-bellied agouti pattern to a white-bellied agouti. Firstly it arose independently three times in a hybrid race of mice in which there had been no selection in the direction of the mutation. Secondly it arose once in an inbred race, and in exactly the opposite direction from that in which selection was being made. Another mutation involving the disappearance of the black-producing factor arose in a stock of inbred wild mice, causing the appearance of agouti young. This race was related to the hybrid race in which the white-bellied agouti appeared three times, and it is suggested that this germinal instability may have been introduced by a male ancestor of both races.

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc.

THE *American Anthropologist* for October to December 1915 (vol. xvii. No. 4) maintains the high standard of excellence usually reached by this periodical. An admirable characteristic of this magazine is that it habitually caters for every class of reader. Some of the contributions are so very popular and so simply written that the ordinary newspaper-reader, making no pretence of anthropological knowledge, could peruse them with interest and profit, whilst other articles go so deeply into special problems that professed students of those problems may find in the essays new thoughts and original suggestions. The editors of the *American Anthropologist* are doing most valuable work for their science. In the category of popular articles in this number is an essay by G. Grant MacCurdy of Yale entitled "Race in the Pacific Area, with special reference to the origin of the American Indians: Antiquity of Occupation." The

article does not deal with the existing races of Polynesia, but with fossil types, and we are interested to see that the author thinks that the American Indians did not reach their continent until the close—or at least the decline—of the last great glaciation. It is, of course, generally believed that they entered *viâ* the Bering Strait. The immigrants presumably found the Western Hemisphere uninhabited, and the virgin prairies must indeed have been happy hunting grounds. If it be true that the arrival of the Red Men was as recent as Mr. MacCurdy believes, the peopling of the Americas must have taken place contemporaneously with the end of the Old Stone Age and beginning of the New Stone Age in Europe. Mr. Charles Peabody contributes an article on Prehistoric Palestine and Syria, and W. D. Wallis, of the California University, writes a learned discourse on "Individual Initiative and Social Compulsion," in which, amongst other matters believed to be psychologically cognate, the phenomenon of "messiahs" in North American and other savage tribes is discussed. Mr. Earl H. Morris, of Oklahoma University, describes some original investigations in a paper entitled "The Excavation of a Ruin near Aztec, San Juan County, New Mexico." The book reviews published in the *American Anthropologist* are of the nature of what in Science Progress are called "Essay-Reviews." Thus A. A. Goldenweiser contributes what is in reality a fairly long article around the subject of Emile Durkheim's book, *Les Formes Elémentaires de la vie Religieuse: Le Système Totémique en Australie.* Amongst the other books reviewed in this manner is Hose and McDougall's *Pagan Tribes of Borneo.*

It is notorious that science in general does not receive adequate recognition in the British Empire, and this is unfortunately true of anthropology in particular, but it is satisfactory to be able to record that in respect of anthropology Canada is less backward than other parts of the Empire, possibly owing to the educative influence of the great republic: for in anthropology (though not I believe in most sciences) America is ahead of England. In the April number of *Man* there is a contribution by A. C. Breton describing recent anthropological work in Canada, and having regard to the fact that the Canadians are a small nation, one is impressed with the large amount of work which is being done. The article includes a summary of the important recent work on the Eskimos. In

the May number of *Man* Prof. F. G. Parsons writes " A Reply to Mr. Pycraft's Plea for a Substitute for the Frankfort Base-line," a plea to which I referred in these notes last January. Parsons points out that any such change as Pycraft advocates would need to possess very marked advantages before it could be considered justifiable, as a change would render much recent craniometric work useless for purposes of comparison; moreover, he thinks Pycraft's proposed substitute would not be an improvement, even apart from the unavoidable nuisance of any change of standards, but rather the reverse. Prof. Giuffrida-Ruggeri, of Naples University, contributes to the June *Man* an interesting though extraordinarily digressive article on Egyptian ethnology. The Italian anthropologists (or most of them) divide *Homo sapiens* into a large number of sub-species or primary races, and the terminology of Italian essays will therefore be unfamiliar to most English readers. It appears probable that the number of sub-species made by this classification is at once too large and too small; too large because these sub-species are capable of being grouped into a few more comprehensive racial divisions; and too small because ultimately it will probably be possible to classify mankind on the basis of Mendelian unit-characters, which will involve the creation of numerous new sub-species or types.

The July *Edinburgh Review* contains a popular article by Prof. Boyd Dawkins on " The Antiquity of Man," which will not be criticised for its novelty. It should be pointed out that the table given on page 84 conveys a seriously erroneous impression of the geological history of the Primates. The word " Lemuroids " is printed against the Eocene and Oligocene periods, and the expression " Anthropoid Apes " against the Miocene and Pliocene periods. Now not only the Anthropoidea, but the Simiidæ themselves, are known to occur in the Egyptian Oligocene. The existence of an Oligocene ape, the creature named *Propliopithecus*, is a highly significant fact which tells against Prof. Boyd Dawkins' special theories, and the slip is therefore unfortunate.

A Neandertaloid mandible discovered near the town of Banolas, in northern Spain, about thirty years ago has now been fully described by Profs. Hernandez-Pacheco and Obermaier in *Comisión de Investigaciones Paleontologicas y Prehistoricas*, memoria numero 6 (Madrid).

CORRESPONDENCE

To the Editor of Science Progress

BACTERISED PEAT

From PROF. W. B. BOTTOMLEY, Ph.D.

Dear Sir,—In view of the interest now being taken by the Board of Agriculture in bacterised peat I am reluctant to criticise the strict accuracy of some of the statements made by Sir Sydney Olivier in your April issue, but in fairness to my work a more detailed account of some of the "facts" he mentions is necessary.

In April 1914 an application was made by King's College to the Board of Agriculture for a grant in aid of research on bacterised peat. In June a representative from the Board called at King's College and informed me that the attention of the Advisory Committee had been drawn to the fact that I had patented the process of manufacture of bacterised peat, and they could not recommend the grant unless I gave up my patent rights. This I refused to do. Eventually it was suggested that I could retain my patents and that a grant might be recommended for a purely scientific investigation if I wrote a letter disclaiming any intention of using the grant for experiments with a patented substance.

On July 23 I wrote: "With reference to our conversation yesterday I quite understand the position of your Advisory Committee in being unable to make a grant for research in connection with a patented substance. I beg, however, to state that the grant for which I have applied would not be used for any research in connection with bacterised peat. . . . It is to this purely scientific investigation—the presence and work of accessory food substances in germinating seeds and soil organic matter, and their interrelationship—that any grant made would be applied. As regards the method of manufacture of bacterised peat being the subject-matter of a patent, I would willingly grant permission for its preparation and use for scientific investigation, and supply as far as possible all necessary cultures

of bacteria to any person or persons nominated by the Board of Agriculture."

On August 21 the King's College authorities received an intimation that a grant of £150—£100 towards my assistant's salary and £50 for apparatus—had been sanctioned "in aid of research on soil bacteria and the probable accessory food substances in bacterised peat."

Surely in view of these circumstances I was justified in stating that the Board of Agriculture had refused to make a grant for experiments with bacterised peat.

I have never made the admission "that no experiments on a commercial scale have been carried out successfully." On the contrary, I maintain that the value of bacterised peat for horticulture has been proved on a commercial scale in numerous nurseries and market gardens during the last three years. Could one require better testimony on this point than that given by Mr. Watson, the curator of Kew Gardens, in the *Gardeners' Chronicle*, October 30, 1915? After experimenting with bacterised peat for three years Mr. Watson writes: "I have not seen a single case of failure where plants treated with humogen (bacterised peat) have had ordinary attention. . . . In a properly constituted soil humogen is capable of working a change in its productivity which, after a long experience with plant soils and plant foods, I am in a position to say is very extraordinary. I have never seen anything to equal it."

One wonders why Sir Sydney Olivier did not mention in his letter these results of the experiments made at Kew, "which," as he says, "is under the Board's control." Is it because he attaches more importance to "the doubts that many of my scientific colleagues still maintain as to the value of my discovery" than to the results of three years' investigation at Kew?

I am rather surprised to find that Sir Sydney Olivier considers that the results of the Wisley experiments afford evidence "that there is still room for doubt whether bacterised peat can be used agriculturally on a commercial scale." This is evidently not the opinion of Mr. Chittenden, who conducted the experiments, for in the "summary and conclusions" of his report he definitely states: "The results on the whole show that when prepared under the best conditions bacterised peat is capable of acting as a very effective manure."

Profs. Wood and Biffen, after criticising the experiments

on barley, potatoes, mangolds, and wheat given in Mr. Knox's book, say that "on this inconceivably flimsy foundation statements are made that our food supply can be doubled by the use of bacterised peat." Neither Mr. Knox nor myself have ever made any such claim for bacterised peat, for, although we consider its value has been demonstrated for horticulture, we realise that field experiments have been as yet too few to warrant any specific statement or claim regarding its value for agriculture.

They quote from the report of the field trials with bacterised peat at the Midland and Dairy Institute, which shows that a dressing of 7 cwt. per acre applied to wheat and seeds hay produced no result in either crop. They do not mention that the same report states that similar negative results were also obtained with other manures. Seeds hay plots were treated at the rate of 1½ cwt. superphosphate, ½ cwt. muriate of potash, and 18 lb. of nitrogen in various forms per acre, and "none of the manures gave an appreciable increase in the crop." This does not prove that phosphates, potash, and nitrogen have no manurial value, but rather, as the report points out, "shows that on land in high condition one must exercise judgment in the use of manures." One cannot expect bacterised peat, which is essentially a humus manure, to give results on land already rich in humus. Judgment must be exercised even in the use of bacterised peat.

The farmer who farms for profit will be duly impressed with the calculations of Profs. Wood and Biffen who, after saying that "the price of bacterised peat appears to be about £10 per ton," assume that it will have to be used in the open fields in the same proportion as in the experiments at Kew, and then give the cost of application as about £1,000 per acre!

The Manchester Corporation are now selling bacterised peat at £4 per ton, and experiments with an improved drying and sterilising plant at Entwistle indicate the probability that this price will be reduced eventually to about £2 per ton.

The suggestion that farmers should use only 5 to 10 cwt. per acre is objected to because it is made "without a shred of evidence to show that such proportions may be expected to produce in his fields results comparable with those obtained by the use of 10 per cent. in a flower-pot." Plenty of evidence on this point has been received since Mr. Knox wrote his book.

May I be allowed to quote one result taken from the "Report on Potato Experiments" published by the Hants County Council in December 1915? These experiments were carried out at the Farm Institute, Sparsholt, Winchester, on a ⅛-acre plot under field conditions. Bacterised peat was applied at the rate of 15 cwt. per acre to one section of the plot and gave an average increase, with seven varieties of potatoes, over the control section of 2 tons 12 cwts. per acre. The exact figures per acre are given in the following table:

Variety.	Without bacterised peat.				With bacterised peat.				Gain per cent.
	tons.	cwts.	qrs.	lbs.	tons.	cwts.	qrs.	lbs.	
British Queen	9	0	3	6	12	12	1	8	39·5
King Edward VII.	4	16	1	20	9	1	2	12	88·3
Up-to-date	9	17	2	20	11	8	3	20	15·8
Arran's Hope	8	12	3	2	10	8	3	20	20·8
Arran Chief	7	7	0	6	9	0	0	0	22·4
Golden Wonder	5	8	1	26	10	7	1	8	91·0
Langworthy	5	4	1	24	5	12	2	0	7·6
Total	50	7	2	20	68	11	2	12	—
Average	7	3	3	22	9	15	3	21	—

The report states: "The effect of the humogen (bacterised peat) was very marked on all varieties, and if further trials bear out these results this substance should prove of great value to the potato crop, if the price is a reasonable one." Evidently the question of price will be an important factor as regards the extent of the application of bacterised peat. It is this consideration which caused me to welcome the generous aid which has been extended to me by the Manchester Corporation and the owners of the Entwistle Peat Estates, in spite of the unfortunate newspaper notoriety, for it is only by manufacturing the material on or near the peat bog itself that the price can be reduced to a minimum.

As regards nitrobacterine the only "discovery" in connection with this was the use of sterile soil as the best medium for distributing pure cultures of nitrogen-fixing bacteria. This "failure" has proved so successful in America that the United States Department of Agriculture has adopted the method for all cultures in connection with seed and soil inoculation for leguminous crops.

The question now is—can the results which have been

obtained in horticulture be extended to agriculture, especially in the direction of enhancing the productivity of poor land? A poor starved soil must be supplied with humus to render it fertile, and in peat bogs we have an unlimited supply of insoluble humus which can be rendered available by bacterial treatment. The practical bearing of bacterised peat on crop production rests on a much surer foundation than "a few haphazard trials carried out under unsuitable conditions by experimenters who do not possess sufficient acquaintance with farming to enable them to interpret the meaning of their own results." Three years' experiments at Kew Gardens have shown that bacterised peat contains certain organic growth-promoting substances. Water cultures with a water plant, *Lemna minor*, have demonstrated that the presence of these substances is necessary for the assimilation of nitrates, phosphates, and potash. This theory of plant accessory food substances is so important from both scientific and practical standpoints that it is now being further tested in an extended series of experiments at the Imperial College of Science and Technology, South Kensington. One welcomes the valuable assistance of such experiments, and it is difficult to understand the attitude of certain scientists who condemn the work of other investigators without even taking the trouble to ascertain its true nature.

I can assure Profs. Wood and Biffen that my object is neither to exploit the farmer nor to handicap "the efforts now being made by a considerable body of scientific men throughout the country to improve the position of British agriculture." British horticulture and agriculture are at present handicapped by a serious shortage of stable manure, and it is on the problem of finding an efficient substitute for this material that I have been working for the last few years. That bacterised peat may provide such a substitute is the opinion of most of those who have personally experimented with it, and one might have expected that my attempts to solve this problem would have met with sympathetic assistance rather than hostile and destructive criticism from some of those who profess to have the interests of British agriculture at heart.

W. B. Bottomley.

Botanical Laboratories,
King's College, London,
May 31, 1916.

P.S. (July 28).—The experiments with *Lemna* plants at the Imperial College of Science are proving the importance of accessory food substances for plant nutrition. Plants growing in Detmer's culture solution to which was added a small quantity of the water-soluble organic substances from bacterised peat have multiplied much more rapidly than those grown in Detmer's solution alone. They have also increased in size and retained their vigour, whilst the control plants have become much smaller and are in a dying condition. The results after five weeks' growth are:

	No. of plants originally.	No. after 5 weeks.	Weight of 100 plants originally.	Weight after 5 weeks.
Detmer's solution (5,500 p.p.m. mineral salts)	20	211	12 mg.	5·9 mg.
Detmer's solution (5,500 p.p.m. mineral salts) + 367 p.p.m. organic matter	20	3,065	12 mg.	16·7 mg.

ANCIENT KNOWLEDGE OF PARASITE-CARRIERS

FROM EM. PROF. H. A. STRONG, LL.D., LIVERPOOL

SIR,—May I venture to notice with reference to the most interesting article[1] on the subject of the knowledge of the ancients of animal and insect pests, and their influence on health, that the Romans do not seem to have had any knowledge of the rat, and indeed possessed no word for that animal. Even in Italy at the present day *topo* is used by most people indifferently for a mouse or a rat. There seems no doubt, as mentioned by Hehn (*Kulturpflanzen und Hausthiere*), that at some early period *after* the great migration of the Aryan tribes to the west, an immigration of rats occurred from Asia (the *Mus rattus*). The various tribes composing the Aryan people must have formed themselves into nationalities before the advent of this animal, for we find that it bears different names among the different Aryan nations. Thus the Polish word for rat differs from the Russian, and the Slavs of the Balkans have a different form again. The Irish called the rat the Frankish mouse, and the modern Greeks still call it the Pontic mouse: showing that they regarded it as coming from the East. The Italian *topo* is simply a variant of talpa,

[1] SCIENCE PROGRESS, April 1916, by Joseph Offord.

which is said to be connected with Gk. τολύπη = the mound-thrower. Of course the great invasion of the *Mus decumanus* or Norway rat only occurred in the beginning of the eighteenth century. The name *rato* occurs in old German vocabularies, and this name in different forms is common to all Teutonic countries. It is most probable, then, that the rat was not known to the Romans.

With respect to the knowledge of the Romans of the baleful influence of mosquitoes, Varro (*de re Rustica*, i. 12) remarks, as a reason for avoiding *loca palustria*—" crescunt animalia quaedam minuta, quae non possunt oculi consequi, et per aera intus per os ac nares perveniunt atque efficiunt difficiles morbos "—a striking passage.

Yours, etc.

HERBERT A. STRONG.

MALEDUCATION AND MALPRONUNCIATION

FROM SIR H. BRYAN DONKIN, M.D., F.R.C.P.

SIR,—The author of the " Note " on " Maleducation and Malpronunciation " in the July issue of SCIENCE PROGRESS seems to have forgotten that all Greek and Latin scholars in England (with the probable exception of many of those who are now under fifty years old) were taught to pronounce these languages in the English way; and that to such scholars the sound and rhythm of the poetry of Homer and Virgil have given, and still give, as exquisite a pleasure as that enjoyed by classical scholars of any other nation. It surely matters not how the ancients pronounced Latin or Greek, when this question is looked at from the æsthetic point of view. It may be true that by an English student of other languages the rhythm of Homer, as rendered by a modern Greek, is quite appreciable; but it is certainly true that to an immense majority of English scholars of middle age both the sound and rhythm of Homer's lines, pronounced in English fashion, are immeasurably more delightful.

Why the author of the " Note " should drag in the word " Cockney " when he contrasts the modern Greek with the English tongue is a puzzle as insoluble as " What song the Sirens sang." But " what are we to think " of his quoting and stigmatizing as " doggerel " the well-known and touching lines :

Qui procul hinc, the legend's writ—
  The frontier-grave lies far away—
Qui ante diem periit,
  Sed miles, sed pro patria.

What, moreover, is wrong in the rhyming of " away " with "patria," unless the writer contends either that " away " is correctly pronounced as " awy," or that " patria " (in the *ablative* case) should be sounded as " patrier " ?

I agree with the author of this " Note " that " poor Science lives in the kitchen like Cinderella " ; but I don't think that his attribution of her domicile to the death of Poetry is likely to expedite her enfranchisement. For Poetry in England isn't dead.

Your obedient servant,
H. BRYAN DONKIN.

LONDON, *July* 21, 1916.

REPLY

SIR,—Are not these elementary matters of text-book phonetics? The first and second *a* in the ablative *patria* were probably the short and long *a* of modern Italian, Spanish, French, German, etc.—somewhat like the first and second *a* in *mamá* and *papá*. The *ay* in *away* is really the diphthong correctly spelt in *veil*, which cannot possibly rhyme with the last syllable of *patria*, properly pronounced. Even the first *a* in *patria* is often wrongly given the sound of *a* in *cat*, as pronounced in what are called the " cockney dialects " of English to distinguish them from some north-country and Scottish dialects (in which I have heard *cat* pronounced nearly as a Londoner would pronounce *cart*). The ablative of *patria* was certainly never sounded either *pat-rier* or *pat-riay*. Sir Bryan Donkin's argument appears to be that we are justified in adopting a wrong course if it is easy and pleasant. The following passage, however (which I have not seen quoted before), shows what Milton thought on the matter. Thomas Ellwood describes in his autobiography how when he read Latin books to Milton in the days of his blindness, "At my first reading to him, observing that I used the English pronunciation, he told me, if I would have the benefit of the Latin tongue, not only to read and understand Latin authors, but to converse with foreigners, either abroad or at home, I must learn the foreign pronunciation. To this I consenting, he instructed me how to sound the vowels; so different from the common pronunciation used by the English, who speak Anglice their Latin, that . . . the Latin thus spoken seemed as different from that which was delivered, as the English generally speak it, as if it were another language." Probably Milton's æsthetic sense in versification was better than that of most of us. Regarding the survival of poetry in England, we may doubt whether, excepting reprints of classical works, sold chiefly for prizes and presents, and " topical verses," as many as five thousand books of poetry are disposed of annually to the forty odd millions of people in the country.

Yours faithfully,
THE WRITER OF THE NOTE.

NOTES

Sir William Ramsay, K.C.B., F.R.S. (F. A. Mason, Ph.D.)

By a somewhat grim coincidence each of the three divisions of chemical science has, within the last eighteen months, suffered in succession the loss of one of its most distinguished exponents.

First we have had to mourn the death of that doughty warrior in the cause of organic chemistry, Prof. Raphael Meldola; then the death of Sir Henry Roscoe robbed the province of pure inorganic chemistry of its doyen; and now the third division, physical chemistry, has to bear the loss of a man who indeed may be looked upon as one of its founders.

No British scientist in recent years has enjoyed so large a measure of public confidence as Prof. Ramsay, and both the country he served so well and the science to which he devoted his life will feel that his death has left a great and ineffaceable gap alike in the ranks of scientists and of public men.

Sir William Ramsay was born in Glasgow on October 2, 1852. On his father's side the family had been for seven generations practising the art of dyeing, and on the distaff side a number of physicians in the family history showed a certain bent towards science, which was to manifest itself so strongly in a later generation.

Prof. Ramsay has described himself in his early days as a dreamy youth of somewhat unconventional education. He does not appear as a boy to have shown anything more than average ability, either at the academy or at Glasgow University, which he entered at the age of fourteen; nor, in fact, does he seem at first to have shown that precocity and single-minded pursuit of one definite ideal which so often characterises the youth of genius.

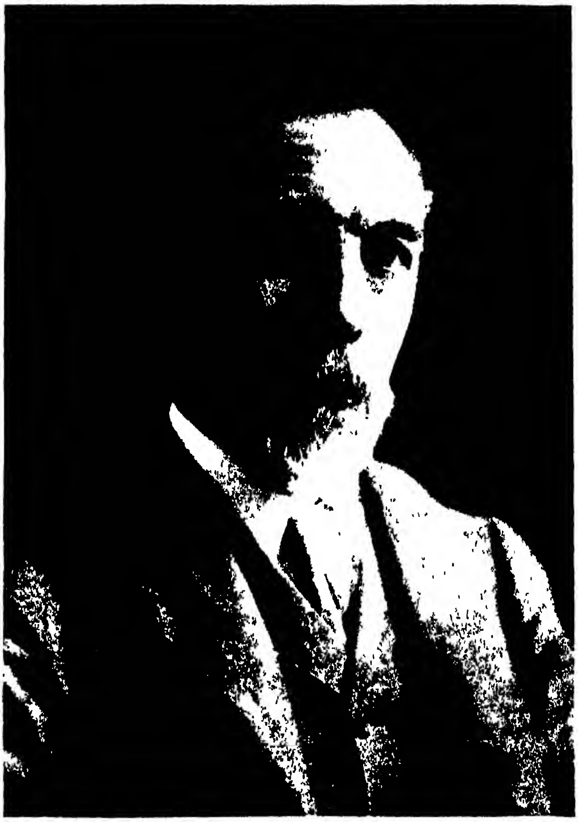
His first introduction to chemistry arose as the result of an accident at football, when, being confined to bed with a broken leg, his father presented him with a copy of Graham's *Chemistry* and a few chemicals to while away the time, and combine "instruction with amusement."

According to his own confession his chief attraction towards chemical science was the desire, not uncommon perhaps, to know how to make fireworks, and certainly with little idea of devoting his life to the study of chemistry.

At the University, however, he came under the eye of William Thomson, a genial, sympathetic, and unconventional teacher with a great power of inspiring enthusiasm among his pupils. Here, perhaps, it was that Ramsay began to find his feet and to throw himself with enthusiasm into the examination of the facts of chemistry. Later he studied under Tatlock also in Glasgow.

At eighteen years of age he had imbibed all the chemical knowledge obtainable in Glasgow, and decided to migrate to Germany to complete his chemical training. This was the time of the *année terrible*, the war of 1870, and at first it seemed impossible to carry out the project. Later, however, the difficulties were overcome, and after studying for some time under Bunsen at Heidelberg, he went to Fittig at Tübingen, where he graduated after presenting a dissertation on toluylic acids.

After these "Reisejahre" he returned to Glasgow, and for some years acted as



Lafayette, Manchester.

SIR WILLIAM RAMSAY, K.C.B., F.R.S., NOBEL LAUREATE.

assistant at his old University. Here, in spite of the strong prevailing trend towards organic chemical research, his natural leanings towards physical chemistry began to show themselves, and one of his earliest researches was on the determination of vapour densities by the use of pitch-pipes, in carrying out which his musical ear was turned to good account.

In 1880 he accepted the post of Professor of Chemistry at Bristol College, where, in collaboration with Prof. Sydney Young, he carried out those important researches on vapour pressures which have become classical.

In 1887 the Chair of Chemistry at University College having fallen vacant through the resignation of Prof. Alexander Williamson, Prof. Ramsay accepted the post. At University College those researches on the rare gases of the atmosphere were undertaken which, more perhaps than any others, brought Prof. Ramsay's name into the very front rank of chemical investigators.

Lord Rayleigh had noticed whilst examining the density of nitrogen obtained from various sources that the samples of atmospheric nitrogen were consistently slightly heavier—by about one-thousandth part—than those obtained by other methods. Cavendish in 1784 had noticed that after removing the chief components of air, oxygen and nitrogen, there was always a small residue of less than a per cent. which could not be removed by chemical treatment. There the matter had remained for a century, but taken in conjunction with later observations the suggestion offered itself that there were present in the atmosphere small quantities of an unknown inert gas.

Sir William Ramsay, working quite independently of Lord Rayleigh, had found that atmospheric nitrogen after absorption by red-hot magnesium always left a residue unaffected by the metal. The two great scientists, becoming aware of each other's work, decided to pursue the investigation conjointly, and as a result of their researches they were able in 1894 to announce to the meeting of the British Association the discovery of a new gas in the atmosphere, which, on account of its chemical inertness, they named Argon.

Later, working with Dr. M. W. Travers, Prof. Ramsay was able to show that even "Argon" obtained from the air was not homogeneous, but contained a whole family of five inert elements—helium, argon, neon, krypton, and xenon.

When it is considered that there is only one part of xenon in seventy million parts of air, one begins to realise the immense difficulty of the research, but Prof. Ramsay's unique skill in glass-blowing and his almost uncanny powers of working with microscopic quantities of substances enabled him to carry out his researches successfully, even with quantities of such nightmare-like minuteness as would appal most men.

It is, perhaps, worthy of note that to-day certain firms are in the habit of selling compressed argon, obtained as a by-product from liquid air distillation, as an ordinary article of commerce: a far cry indeed from the infinitesimal amounts obtained at first by Ramsay and Rayleigh in their work—sometimes alluded to as "the triumph of the third place of decimals," as indicating the minute difference in the densities which gave rise to the research!

Of the other work of Sir William Ramsay the most widely known is, perhaps, that connected with radium and its emanation, in the course of which he was able to prove that helium is produced from radium, as a product of atomic disintegration, and so for the first time that chimera of the alchemists, the transmutation of the elements, was shown to be an actual fact.

It would take too long to enumerate the full course of these discoveries, but here also Prof. Ramsay's skill as a manipulator and his almost miraculous power

of working with minute quantities were tested to the utmost. In his research on radium emanation he obtained after two months' work about as much of the gas as would fill a hollow pin's head, yet this sufficed in his hands to prove that helium is produced from radium.

As a teacher Sir William enjoyed the greatest popularity, and his keen perception, cleverness, and genial temperament made it a very great pleasure for those who were fortunate enough to work under him.

During the last few years he realised how dangerously careless, unscientific, and inefficient was the national treatment of many vital matters, such as the position of science, the utilisation of the national coal resources, and the like, and he did yeoman service in bringing the matter before the public notice.

One of his last public actions was the campaign during the present war to force the Government, ignorant alike of science as of the operations of modern warfare, to declare contraband the cotton needed by the Germans for their propulsive explosives.

He was a linguist of a high order, and as a musician he frequently delighted his friends by his performance on the violin, or occasionally at a college chemical dinner by some humorous parody written and composed by himself.

Prof. Ramsay was made a Knight Commander of the Bath in 1902, one of the very few distinctions conferred on chemists in this country, and he was the recipient of numerous orders from many Governments. British and foreign universities conferred on him their degrees, whilst the Nobel Prize for Chemistry was also his.

Unfortunately for the country he was stricken down in the midst of his activities with a mortal sickness, and despite every care he gradually sank and passed away at his home in Hazlemere, High Wycombe, on July 23, 1916.

Alike as a chemist and a patriot he deserved well of his country, and his name will be a source of inspiration for many a generation to come.

FREDERICK A. MASON.

Élie Metchnikoff (C. H. O'Donoghue, D.Sc., F.Z.S.)

Scientists the world over heard with deep regret that Élie Metchnikoff died on July 15, at the age of seventy-one. In him they have lost an outstanding figure in which great simplicity and charm of personality were combined with untiring industry and extraordinary intellectual powers.

Elias Mecznikow was born in the Russian province of Kharkoff in 1845. His father, of an old Moldavian family, attained some eminence in the Russian army and when he retired from the Imperial Guard was holding the rank of major-general. To his mother, who was of Jewish extraction he appears to owe much of his wonderful mental equipment. He entered the university of his province at the age of seventeen and two years later graduated, having by this time already published two papers, one on *Vorticella* and the other on *Diplogaster*. After graduation and a short stay in the then British colony of Heligoland he went to study with Leuckart first in Giessen and then in Göttingen. Little is to be gained by quoting in extenso a list of his published works, though it is a very impressive and formidable one, but it is interesting to note the general trend of his studies. His first work, as has been noted, was on what may be described as general invertebrate zoology. We soon find, however, particularly during a stay in Naples, that he gradually transferred to invertebrate embryology. He married his first wife in 1868 and two years later he was appointed professor ordinarius in the University of Odessa, and three years after that he lost his first wife, who died of pulmonary tuberculosis. In 1875 he married again a young and accomplished

lady, Olga Belecoyitoff, who survives him after being his constant help throughout much of his subsequent work. With his friend and compatriot Kowalewsky he shares the honour of practically founding the modern study of cellular embryology from which cell lineage has grown. In the course of his researches he was led to deal with the activities of various cells and particularly to the intracellular digestion in the wandering amœboid cells of various animals to which he gave the name phagocytes. These activities he almost at once correlated on the one hand with the microbes of Pasteur, and on the other with the Darwinian concept of the struggle for existence. This quickly developed into the well-known theory of "phagocytosis" which brought its propounder before a wider public and opened up such vast fields of research that he forsook zoology to break the new ground.

As opportunities for this work were not forthcoming in Odessa, he left in 1888, and perhaps only naturally, but certainly fortunately, came to work in the old Pasteur Institute where he remained until the new one in rue Dutot was built, and in this Elie Metchnikoff, as he was now called by his French friends, settled down and actually died on the building. His subsequent studies were marked by two striking books. The first was *The Comparative Pathology of Inflammation* in 1892, which contains a series of lectures setting forth his view that inflammation is brought about by a local stoppage in the blood flow induced by the nervous system, so allowing a large number of phagocytes to leave the capillaries. These phagocytes, attracted to the injured spot by chemiotaxis, form an army through whose agency obnoxious substances and harmful bacteria are got rid of. As might be expected, these views were opposed by many of the medical profession, but although involved in controversy he never indulged in wordy disputation, but answered his critics by publishing further researches on crucial points.

After a further period of brilliant research by himself and his pupils he published in 1901 the second book, *Immunity in Infectious Diseases.* This involved the discussion of a large number of the branches of the modern theories of immunity including antitoxins, opsonins, bacteriotropins, etc., many of which owe their origin to, and almost all were rendered possible by work done in, his laboratories. He subsequently published in 1903 a more popular book, *The Nature of Man*, and it is this that is best known to the general public. In dealing with old age, he advanced the view that senility and early death may be induced by the poisonous substances produced during certain fermentation processes that sometimes take place in the large intestine. Experiment showed that such fermentation could be stopped by the addition of soured milk to the diet. This was of course an opportunity too obvious for journalists to let slip, and it is about the one thing that is prominently associated with Metchnikoff's name by the public.

Here in brief is the outline of a great life's work. It is a life that provides an answer to the people, unfortunately too numerous, who constantly demand to know what "use" science is going to be and regard with intolerance the study of science for its own sake. It would have been impossible from his first papers to predict the course of Metchnikoff's work, yet what more natural sequence could be found? Starting with pure microscopical zoology, the transition to embryology is quite an obvious one and so also is the change from this to the study of the activities of the cell layers of the lower animals. Here the amœboid cells fixed his attention and from that he became a pathologist, and few men have had so wide an influence on the theory and practice of medicine.

His brilliance was recognised during his lifetime, for he was awarded the

Copley Medal of the Royal Society, of which he was also a foreign member; he was a member of the Institute of France and the Academy of Sciences of Petrograd. His researches on immunity brought him the Nobel Prize, and only a fortnight before his death he was awarded the Albert Medal of the Society of Arts to mark the benefit he had conferred on mankind by his researches. It is hard to measure in true perspective the value of a great man so recently among us, but it would appear that much as he was honoured in his life, posterity will accord him a still higher place in the list of those who have given of their best for their fellow men.

To the memory of a great genius and a large-minded man we wish to pay a humble tribute of respect.

CHAS. H. O'DONOGHUE.

Dr. R. H. Scott, F.R.S.

The first holder of a new and special post is frequently hard to replace, and the problem faced by the Board of Trade in 1865, on the death of Admiral FitzRoy, the "father" of British Official Meteorology, was further complicated by the fact that, during the ten years since his appointment by Mr. Cardwell as Director of the new Meteorological Department of the Board of Trade, FitzRoy had far exceeded his instructions. Gifted with energy, enthusiasm, and imagination, he had allowed the original programme, the compilation of marine meteorology from ships' logs, to fall into arrear, while he initiated daily weather reports in 1860, and forecasts and storm warnings in 1861, these last depending principally on empirical rules of his own. On his death the Government requested the Royal Society to report on the matter, the forecasts being continued meanwhile by Mr. Babington, the senior clerk in the department, who was familiar with FitzRoy's methods. The Royal Society appointed a committee with the President, Gen. Sir Edward Sabine, as chairman, and by December 1866 they had decided that the forecasts, not being founded on a truly scientific basis, should cease for the time, and that a better scheme for the Office would be to extend land meteorology by the foundation of several more observatories in the British Isles, after the model of Kew, at the same time continuing the Marine branch and the daily weather report. The Board of Trade accepted the decision and arranged to hand over the department, with an annual subvention of £10,000 for expenses, to the control of an unpaid Committee of the Royal Society. The forecasts were stopped at once, and Mr. Babington was immediately transferred to another department. In January 1867 the new Meteorological Committee met and appointed Robert Henry Scott as Director, Captain Toynbee as Marine Superintendent, and Prof. Balfour Stewart (Director of Kew Observatory) as Secretary to the Committee. Mr. Simmonds, next in seniority to Mr. Babington, and the only man left in the Office with any experience of FitzRoy's methods, remained in charge only until the new Director arrived. Both of these clerks naturally preferred to continue in the Civil Service. It had thus been left practically to Sir E. Sabine to find a successor for FitzRoy, and he, obviously desiring an effective contrast, might be expected to choose a methodical man of little or no imagination.

Scott was born in 1833. His father was James Smyth Scott, Q.C., and his maternal grandfather the Hon. Charles Brodrick, Archbishop of Cashel. One of his brothers was headmaster of Westminster, and another Vicar of Bray and Archdeacon of Dublin; so that his family was by no means obscure. He was educated at Rugby and Trinity College, Dublin, where he was Classical Scholar in 1853, and graduated as Senior Moderator in Experimental Physics in 1855.

Before taking his M.A. degree in 1859, he spent a couple of years in Germany, where he studied at Berlin and at Munich, principally Chemistry, Physics, and Mineralogy, and incidentally attended a set of lectures by Prof. Dove, of Berlin. In 1862 he was appointed Keeper of Minerals to the Royal Dublin Society and during the five years that he held this post he contributed several articles to scientific journals on the minerals of various districts, especially Donegal, in addition to a manual of *Volumetric Analysis*. Being desirous of keeping up his German, he also published in 1863 a translation of the second edition of Dove's *Law of Storms*, knowing something of the author's views and style. It is interesting to note that his last contribution to scientific literature was also a translation from the German of Lenard's article on "Rain" in the *Meteorologische Zeitschrift* for 1904. To the translation of Dove's work, dedicated to Admiral FitzRoy, who had translated the first edition, is generally attributed Scott's appointment to the Meteorological Office; but, though Sabine probably used it as an argument in his favour, it is likely that something was due to his work in Dublin at a subject in which method is almost everything and imagination all but useless; moreover, Sabine most likely had previous knowledge of Scott, a T.C.D. man like himself, and whom he afterwards appointed as his executor. An autograph letter of Sabine, preserved at the National Portrait Gallery, proves that he, at any rate, felt sure that Scott's appointment would be a success.

Henceforth Scott's main interest lay in his new work, though he did not quite abandon mineralogy, and did, in fact, serve a term as President of the Mineralogical Society. But the work of the newly constituted Meteorological Office was rapidly growing. Storm warnings were soon resumed in deference to the pressure of public opinion, and before the end of 1868 six new observatories were in operation, the triangle for England being completed by the addition of Falmouth and Stonyhurst, while Scotland was represented by Glasgow and Aberdeen, and Ireland by Armagh and Valentia. Moreover, the growth of the observatory work soon induced Balfour Stewart to give up to Scott the duties of Secretary to the Meteorological Committee. In 1874 a new inquiry was held, and as a result the responsibility for the Office was vested in the Meteorological Council, Scott being now called Secretary to the Meteorological Council, and this arrangement remained in force until after his retirement in 1900. Forecasts were reintroduced in 1879, the arrears of work in the Marine Department were gradually cleared off, the number and value of reports—weekly, monthly, quarterly, and annual—steadily increased; and these with many subsidiary investigations, for which, in a great measure, Scott was personally responsible, form the history of the Office and the result of Sabine's policy. But much of the progress made in modern meteorology, which now seems ancient history, took place too late for Scott to have had any real share in it.

In 1870 he was elected F.R.S., and in 1871 he joined the Meteorological Society, and was almost immediately appointed its Foreign Secretary, a post which, except for the short intervals 1880-1, when he served as Secretary, and 1884-5, when he was President, he held until the day of his death. He attended the International Meteorological Congress at Vienna in 1874, and for a quarter of a century, until his retirement, acted as Secretary of the International Meteorological Committee then formed. Dublin University honoured him in 1898 with the degree of D.Sc., and he was a corresponding member of several foreign academies. His *Weather Charts and Storm Warnings*, published in 1876, which explained the use of Synoptic Charts in forecasting, and his *Elementary Meteorology*, published in 1883, are his best-known works, the latter having attained to five

English editions, and also been translated into French and Italian. Most of his contributions to scientific literature, though covering a wide range, were of a technical nature, either arising out of the routine of the Office, such as the occurrence of heavy rainfalls at the various observatories, or the classification of fogs, or dealing with some exceptional phenomenon reported to the Office for explanation. His two Presidential Addresses to the Meteorological Society, dealing respectively with the Climatology of the Globe and of the Ocean, were very thoughtful and complete studies of the state of the science at the time.

The need for a more extensive set of stations, and the inelastic nature of the Government grant, made the help of volunteer observers of great importance, and Scott's official connection with the Royal Meteorological Society led to a determined effort by the Society in that direction, of which the Meteorological Record, 1881 to 1911, is a memorial. Its recent discontinuance, on the sound ground of wasteful duplication, may be regarded as evidence that, greatly as Scott differed in essentials from his predecessor, he was almost equally removed from his successor. In fact, it would appear that he was by temperament inclined to evade personal responsibility if work could be delegated to others, so that it is not surprising that matters did not always run smoothly between him and his subordinates, who failed to find the kind of support that a fag expects from his master. An apparent lack of appreciation by a new superintendent of the work of those who are already in possession, and in whose appointment he has had no voice, is, however, too familiar a phenomenon to be emphasised.

In private life Scott is said to have been of a kindly and humorous disposition, though somewhat opinionated. He retired at the end of February 1900, and soon afterwards had the misfortune to lose his wife, a daughter of the Hon. W. Stewart, Island Secretary of Jamaica, and in January 1903 he had a serious fall on the staircase leading to the Royal Meteorological Society's rooms, and suffered an injury to his head. He still continued to be a regular attendant at council, committee, and society meetings, but frequently left before the close, and took less and less active part in discussion. He died on June 18 last, and was buried at Peper Harrow, near Godalming, the seat of the Brodrick family, to which his mother belonged.

The Education Problem (Sir Harry Johnston, G.C.M.G., K.C.B.)

The Editor of this review has kindly permitted me to cross the t's and dot the i's in the writings and speakings of myself and several other people on this subject.

As occasionally happens when a person is much interested in a discussion, he receives unfair treatment at the hands of both reporters and printers, his argument being weakened by Press errors or reporters' mistakes not always due to carelessness on his own part.

When invited to attend the meeting summoned by Sir E. Ray Lankester on May 8, I prepared in typewriting the substance of what I wanted to say, and was careful that my sentences should contain no inaccuracies of statement. But the excessive number of speeches arranged for and other conditions of the assembly made it impossible for me to read what I had to say. I had, therefore, to make a hurried statement based on a recollection of the contents of my typoscript. Consequently, according to the reviewers and listeners, I was wrong on two points (not consciously, because I knew better): (1) as to compulsion in classical studies, and (2) as to the total amount of marks which could now be obtained in certain Government examinations for Greek and Latin studies. I was made to

overestimate this amount by some hundreds, though the correct figure had been published by myself before and since this speech. I also appeared to say that Greek and Latin were *compulsory* subjects in the present-day Sandhurst and Woolwich examinations. They are not, they are optional, though heavily baited with marks. What I had intended to say was that at the time when most of our general officers were educating themselves the classics were compulsory. Latin, at any rate, remained a compulsory subject down to the period before the war (1914).

I endeavoured to set these matters right in my *Nineteenth Century* article for July, but here again there was mischance. In my MS. I stated the marks allotted at Sandhurst and Woolwich for Freehand Drawing at 400. My figure 4 was mistaken by the printer for 2 (200). For various reasons the Editor was unable to send me proofs, and so the article has gone forth with that mistake—200 marks instead of 400 (the correct figure).

On the strength of these three mistakes, in reality of no importance to the arguments advanced, certain writers in the Press attempted to pour scorn on the whole of my presentation of this subject—the subject being, "Are those persons of the middle and upper classes from whom are drawn, with very few exceptions, all the important servants of the State in Military and Civil Service careers, properly educated in their boyhood and youth for the work they may be called upon to perform in such careers?" Although the question has only interested the public since the beginning of 1915, it is, as a matter of fact, one on which I have written in books and articles at different times between 1903 and the present day.

Then, if the allusion to these three inaccuracies in my statements were not thought sufficient to confound my arguments, certain schoolmasters proceeded to rate me indignantly or loftily for having any opinions on education at all. I was informed that not having been a schoolmaster by training, still more, not having passed through Oxford or Cambridge as a student and graduate, I was quite incapable of forming any opinion as to how my fellow-countrymen and women should be educated. I am not content to sit down under such an unfair attempt at disqualification. The assets as educationalists possessed by the clerical or lay schoolmasters who thus attempt to brush my opinions on one side, wherever I have been able to look them up in books of reference, appear to be the conventional attendance at inefficient preparatory schools and inefficient public schools followed by the stereotyped courses in Classics, Mathematics, Greek, Roman and miswritten English (not British) History at one or other of the great universities, together with some dabbling in a few really useless subjects, such as Logic. In most cases it has seemed, even, as though their Mathematics (which as a subject for education I have no wish to attack) have been inapplicable on their part to any useful purpose. As regards their own country they have bicycled or, in later years, motored over a good proportion of England, they have seen something of Scotland, and perhaps had a peep at Wales. But as a rule they know nothing of the Channel Islands or of Man, and scarcely any one of them (not of Irish birth) has been to Ireland. Their foreign travel has been limited for the most part to visiting Belgium, the Rhine, Switzerland, North Italy, and Paris.

I, though I have studied both at Oxford and Cambridge at different periods of my life, have never been through those universities as a student. Yet both have helped me in different ways from an early period of my life onwards in the sciences on which I have chiefly written; and one of them—Cambridge—conferred on me the honorary degree of Doctor of Science as far back as 1902. I have had the usual education at small and large preparatory schools, and have

been a student at the King's College branch of the University of London. It so happened that at the principal preparatory school I attended, and at King's College, I was encouraged to take up modern languages somewhat extensively. By the time I was nineteen I could speak, read, and write with comparative ease French, Spanish, Italian, and Portuguese. In later years I added to these languages Arabic, Hindustani, Swahili, and a general knowledge of many of the Bantu languages of Central Africa. Whilst I was a student at King's College I was equally a student (after passing the necessary examinations) at the Royal Academy of Arts, and there, as at the Zoological Gardens under the late Prof. A. H. Garrod, I went through courses of both human and comparative anatomy, studies which also took me to the museum of the College of Surgeons (of which I am now one of the trustees), where I was much helped by Sir William Flower. The outcome of these studies in biology and painting obtained for me very early in life offers of employment first in West and next in East Africa in connection with scientific expeditions. My year's work as leader of the Kilimanjaro expedition, organised by the Royal Society and the British Association, brought me into relations with the late Lord Salisbury, who gave me a commission in the Consular Service, so that I might embark on political work in Africa. In the course of this I had to learn all the essentials of, at any rate the simpler, military arts, so as to be able to lead or to accompany small armies against Arab slave-trading sultans or powerful native chiefs engaged in the slave trade. I have had much experience at sea on the vessels of our Navy; I have gone through courses of Botany so as to be able to collect intelligently and to describe the forests and their contents; my services to Zoology were acknowledged by the Gold Medal of the Zoological Society; to Geography by other gold medals and foreign distinctions. I have similarly plunged deeply into Ethnology; I have become an expert photographer; I have framed the budgets and looked after the revenue and expenditure of considerable States in Africa; have written a great many consular reports; have learnt, at any rate, enough Law to deal with the intricacies of consular business, the exercise of magisterial functions at home, and the framing of laws and land settlements in African protectorates.

I have travelled at different times over a great deal of Southern Asia, perhaps more over Africa than any contemporary Briton, I have seen a great deal of North America and of the West Indies, and something of South America, and have travelled in nearly every European country except Russia. Moreover, unlike so many of my fellow-countrymen, I have made it my business to know Ireland thoroughly, as well as Wales, Scotland, and every portion of England and the Channel Islands.

And so on, and so on.

And yet I—and this egotism is only excusable because the kinds of life and experience I have been describing cover hundreds of colleagues and of those travelled men of science who have been leading in this Education argument—am told by a few schoolmasters, professors, and politicians whose knowledge of the Empire by any actual visual experience is practically nil, and whose reading is very limited, and by certain members of Parliament chiefly and perhaps solely educated in political jobbery . . . not geography—that I am not qualified to offer an opinion as to how the youth of this country should be educated.

On the other hand, the problem we are trying to face and solve as regards national education may be put thus: We are not attacking, at least *I* am not attacking, the two Universities of Oxford and Cambridge. I have pointed out repeatedly that they are in the forefront of modern knowledge; though I deeply regret that

the over-endowment of the played-out Classics in their colleges robs them of even greater powers for good than they at present possess. Still, you can get the best of modern education at either Oxford or Cambridge, and no doubt at Edinburgh, Dublin, Glasgow, Birmingham, and Manchester. But the false note in Education is set by our Government, especially by the Treasury. They lay down or cause to be laid down the curricula of the examinations for entering the Army and all the important branches of the Civil Service, and to meet these out-of-date curricula the preparatory and the public schools continue to maintain an out-of-date type of education no longer applicable to the needs of the twentieth century. In order to pass these Government examinations the boys who leave the great schools of the country pursue similar out-of-date studies at the Universities; and thus our Army and our Civil Service is staffed with people who, unless they are extraordinary individuals (and fortunately a proportion of them may be so styled), are sealed up for all their working lives with poorly furnished minds unable to rise to their great opportunities. We have only got to induce our Government to adopt a curriculum at these examinations answering to our most modern needs, and the schools and universities will shape themselves to meet this enlightenment.

H. H. JOHNSTON.

A Lost Aeroplane (Lord Montagu of Beaulieu)

(At the request of the Editor, Lord Montagu of Beaulieu, whose claim to speak on aerial matters is admitted, has furnished the following note on one of the most remarkable events of the war—the loss of a new and valuable aeroplane F.E.2D., fitted with a Rolls-Royce engine.)

It is natural that the recent case of the loss, under unusual circumstances, of one of the F.E.2D. (A.5), a very modern type of areoplane fitted with one of the new 250 h.p. Rolls-Royce engines, should have excited a good deal of public interest.

This aeroplane started from the aerodrome at Farnborough on June 1, and two and a half hours later was in German hands at Lille. Another pilot who happened to be on the ground at the time when this aeroplane took its departure gives the following account, which was quoted in a question put by me in the House of Lords on June 27.

My correspondent writes:

"The War Office sent down and asked for two F.E. pilots (the machines being F.E.'s in which the engines were placed) to fly them over-seas. Owing to an error in the delivery of the message, it was understood that the pilots were to fly ordinary F.E.'s, with the result that in one case an inexperienced pilot was sent."

After describing some technical points my friend continues:

"I was at Farnborough at the time and saw the pilot, who complained that he was not an experienced F.E. pilot; also that he had never been overseas and was not sure of the way. The authorities there who heard all this took no notice of his complaint, and told him to take the machine; not only was he inexperienced as regards the machine, but he also had never been overseas before and therefore did not know the way.

"Later a report came through that an F.E. had crossed the lines at Armentières, and had disappeared in the direction of Lille. That evening the German wireless communiqué stated that an F.E. had landed intact south-west of Lille, the pilot having lost his way.

"Thus it will be seen that within three hours of its having turned out of the factory our newest and latest machine was handed over intact to the Huns. I

should think if you wrote and told . . . about this he would have somebody's blood—as it is the second time our beauties at the War Office have lost a brand new machine in the same way—as when the B.E.2E. had just come out, the fourth or fifth machine we sent over to France, was also flown over by a chap who had only just got his wings, and who also did not know the way; and he landed right on the Lille aerodrome and handed the machine to the Huns. To say it is nobody's fault is rot."

The pilot, Lieutenant Littlewood, was only gazetted a flying officer on June 9, eight days after this incident occurred. He is said to have been considered a capable pilot, and there is no ground for the least suspicion of his loyalty. It was clearly an error of judgment on his part, but the real fault would seem to lie with those responsible for sending him over. The pilot, now a prisoner in Germany, has since complained in a letter to his father of "having rotten maps," and has also stated that he descended near Lille thinking that it was St. Omer. There is some excuse for this, as there is a similarity owing to the fact that a canal leads to each place. Lieutenant Littlewood having never flown in France before, was ignorant of the geography of the Front. The full details of the incident have not been really explained so far, and probably will never be explained so far as the public is concerned.

As regards the weather, the day in question seems to have been one with a slight east wind and comparatively good visibility.

The observer, Captain Grant, was not a member of the Royal Flying Corps, and was apparently merely getting a passage back for some reason unknown. Whether his flight was authorised or not has not yet been stated. He had no knowledge of flying, is said never to have been in an aeroplane before, and to have asked the pilot before he started not to indulge in any tricks or to do any spiralling—a name given to the spiral descents practised by pilots as the quickest way of descending to a giving spot.

In the Navy, in the case of the loss of a ship an inquiry or court-martial is always held. In the Army similar procedure is the rule when there has been a serious incident of any kind. But so far there is no information of any special inquiry or court-martial having been made into the loss of this valuable and novel machine. Neither, according to the evidence given before the Judicial Committee sitting under the chairmanship of Judge Baillache, was there any inquiry into another former case—namely, the loss of seventeen out of twenty-nine De Havilland Scouts fitted with Monosoupape Gnome engines which were damaged or "crashed" at the beginning of March while flying between Gosport and another aerodrome and various places on the Flanders Front.

At the moment of writing (July 15) it is pleasant, however, to be able to put on record that the position of our pilots at the Front has greatly improved owing to better machines arriving in considerable numbers to take the place of the now out-of-date B.E.2C.'s with 90 h.p. R.A.F. engines, which, though good machines eighteen months ago, have long been out of date compared with the aeroplanes of the Germany Army.

Open-Air Sleeping (Sir William Lever, Bart.)

(This is a question of considerable interest to every one. Those who have ever lived in the tropics know how pleasant and beneficial it is to sleep in the open air during the warm weather—under mosquito nets of course; and many have also had much experience of sleeping in tents—which is almost the same thing owing

to the easy percolation of the air through the meshes of the fabric. It might be well if persons who can afford to do so would sleep in the open air in England also, and in order to inquire whether this can be done, not only with impunity but with advantage to health, we have asked Sir William Lever, who adopts the practice, to give us his experiences for the benefit of our readers. He writes to us as follows.)

You have asked me to give in as few words as possible my experience of sleeping in the open air. It is now some ten or more years ago, at the time of a very hot spell in the summer, when, noting the delicious coolness of an open-air bathroom and gymnasium that I had built on a flat portion of the roof at Thornton Manor in such a position as to be entirely free from any possibility of being overlooked (one end being roofed over in such a way as to protect it from sudden showers whilst in no way taking away from its open-air character), my wife and myself decided to have the bed put out there and to try sleeping in the open-air. We found it so full of comfort and so beneficial to our health that this open-air bedroom has continued ever since, and in arranging my bedroom at The Hill, Hampstead, I was able to make a similar open-air bedroom—as also at a bungalow I have on the moors near Bolton.

Sleeping in the fresh air, with ample but not excessive bedclothes, I find the sensation of warmth is greater than with a similar amount of bedclothes in an ordinary bedroom even with the window as wide open as possible. I am convinced that it is the fact of passing the night in the open air that enables me to manage to maintain health, though passing the daytime in close confinement at business; and I am convinced, seeing that it is impossible for me to spend much time in the open air except on rare occasions, that it is essential for me to sleep in the open air at night, and that the latter is a most excellent substitute when the former is impossible. I am wonderfully free from colds and I attribute this to sleeping in the open air.

I know that the comic artist has turned himself loose in ridiculing open-air sleeping, and generally depicts the face of the sleeper and the bedclothes covered with flakes of soot, with music coming from a glee-party of cats, and, occasionally, with the head of the sweep's boy coming up out of a neighbouring chimney. And in these war times he varies the fun by depicting a G.R. Volunteer training-man practising sleeping in the open air for the sake of military efficiency, and being called for breakfast by the housemaid in a pouring shower of rain. He sometimes extends this idea and depicts a sleeper in the open air some wet night shouting for whisky without water as he had plenty of water near the bed. But any one who has enjoyed the luxury of sleeping in the open air will not begrudge this ridicule; and after all it is better to laugh than to frown, and are we not told that "he laughs loudest who laughs last," and this last laugh will be with the open-air sleeper.

(It would be quite possible to put open-air bedrooms on the roofs of many houses in England. Indeed, we think that much house-reform is required in this country, and have long been of the opinion that the upper stories might be built almost entirely of glass to allow as much light as possible within the rooms. It would be an excellent thing if some society were to take up the whole subject of house-building. The public and the architects are very slow to make changes, and even the best modern houses might probably be greatly improved in the interests of the public. Windows are still often made too small; hot and cold water are seldom laid on to all the bedrooms—a practice which would save much domestic labour; and hot-air ventilation is rare.)

Ventilation and Chills

The question of ventilation is treated very differently in different countries. We in England are, or pretend to be, inured to chills and not very apprehensive of draughts, and think that this is a manly attitude. But on the Continent people often insist upon shutting railway-carriage windows when the Briton bares his brow to the wintry blast with complete (or pretended) indifference. Hence certain quarrels we have witnessed. Which is the right attitude?

Our theory appears to be based upon the view that we do not get chills when walking in the open air, and indeed feel the benefit of such exercise. But a person who is sitting in a draughty room or railway-carriage is not taking the exercise which warms him against the cold of the atmosphere when he is walking. During a long railway journey we may become thoroughly chilled and open to the attacks of the numerous germs which are living upon our skin, throat, nose, or elsewhere, waiting for an opportunity to enter the deeper blood or tissues, or of other germs which may be hiding in the carriage cushions. The cold air may perhaps impede the attacks of the latter germs, but will tend, by chilling the patient, to encourage the former ones. On the whole, then, the case for sitting in draughts may be questioned; and there are numbers of subsidiary factors which are usually ignored—such as excessive meat-eating, alcohol, previous illness, and so on. Our theory that a close room means much poisoning with exhaled matter has been rather exploded by the work of Prof. Leonard Hill and others. It may be that the danger of being thoroughly chilled is much worse than the danger of infection by germs floating in the air of a close room. At all events, many people declare that they get colds by sitting in draughty places, while others say they get them by going to crowded assemblies in ill-ventilated halls.

Our mode of life is based upon our theories. Our sash windows are probably the most irrational things in creation. They appear to have been first invented in Holland, and brought over to England towards the close of the seventeenth century, in place of the rational French windows. It must have been a curious person who invented the former, which are often dangerous owing to the breaking of the cords, while a draught is always pouring in between the two sashes. What their advantage is, no one can understand, and we are glad to see that they are now being replaced again in new houses by the French windows. On the other hand French windows, as will be easily seen anywhere on the Continent or in Egypt or elsewhere, may be closed to exclude either draught or noise, and to our mind, look much better. Moreover, seldom if ever do we have double windows such as are used throughout Northern Europe.

Our open fires do not warm the rooms, while they flood the atmosphere with particles of carbon, and are the most wasteful method of producing artificial heat. On several occasions the writer has left England in the depth of winter for Russia, Germany or Sweden, and has never been cold until he returned here. On one occasion a Canadian told him that England is the coldest country that he was ever in; and most foreigners make the same exclamation. In Russia the double windows are kept closed nearly throughout the winter; the house is ventilated by proper arrangements of pure heated air brought up from the basement, with a great saving in cost and with the effect of enabling all the inmates to keep warm all day and night. In fact, in the middle of a Russian winter people wear only thin under-vests and sleep at night with a single blanket: while we shiver and shake before smoky fires and dress in thick semi-arctic clothing, besides having the privilege of wasting a large part of our income on coal. To be brief, Swedes,

Germans, Russians, and Canadians laugh at our arrangements. The writer has inquired whether colds are very frequent in Northern Europe and has generally been told that such is not the case, but that colds begin only in the spring when the windows are first opened. As an experiment he once spent five days in Stockholm in December with windows closed (and remained perfectly well), but one comparatively warm night opened the window and heaped blankets on his bed as in England. The next evening a bad cold commenced.

Englishmen complain that rooms in Northern Europe are stuffy, but this may possibly be a mere auto-suggestion. Personally the writer prefers the Continental system—one is warm all the time in the house, and when one goes out one can brave the outside cold in a thick coat without having been previously thoroughly chilled in the house. But these few notes do not by any means conclude the arguments on either side. The matter ought to be much more carefully discussed, especially in view of the fact that the British domestic coal-bill is something enormous, probably equal to that of the British liquor bill, and the war ought to compel us to pay proper attention to such details. We are by no means certain that the British method is the correct one, but neither are we certain that the foreign view is better. At all events, the British nation is at present in this matter in a minority of one. There is no denying the fact that the open English fire is more cheerful than the Continental grate; but just look at the price we have to pay for it!

The Bread and Food Reform League

On July 4 this League held a Conference in the Queen's Hall on the National Importance of Utilising Whole Cereals in Time of War, with Sir James Crichton Browne, M.D., D.Sc., F.R.S., in the chair. The Secretary, Miss May Yates, opened the meeting by a few able introductory remarks in which she stated that the League has been in existence for thirty-five years and has made progress to the extent of inducing the Blue Coat Schools, amongst other institutions, to adopt the principle of feeding their children on unadulterated bread—considerably to the improvement of their health. The Chairman then set himself to prove that the white bread so largely consumed in Britain is both deleterious and wasteful. The millers, in abstracting the outer coating of the grain and resorting to bleaching processes, really remove the health-giving properties of the flour, as it is just this outer covering that contains the vitamines, the germ, and what is technically known as "patents." Nothing more is gained by this bleaching than a pleasing appearance, while the loss in nutrition is considerable. But wheat, he said, is not the only cereal that suffers degeneration at the hands of the millers, for barley and rice also share the same fate. The tropical disease of beri-beri, he stated, had been proved by men of science to be due to the consumption by the natives of polished rice, and had been largely reduced when coolies were induced to nourish themselves on the unpolished variety. The only cereal that had defied the arts of man was oats, as it was found impossible to remove anything from it owing to its hard and gritty nature; so that he deplored the fact that oatmeal, in his opinion, largely responsible for the hardiness of the Scottish peasant and the quality of the English racehorse, should be so neglected in England as a human food. He also denied the truth of the popular ideas that oatmeal is heating and fit only for the poor. Now in time of war, when the general need for economy was felt, it was still more incumbent on the country to insist on the best use being made of its foodstuffs. If the source

of life could be drawn from whole wheat meal, whole wheat flour, oatmeal, unpearled barley and unpolished rice, less meat, eggs, milk and other foods would be needed, thereby effecting a true economy. The only reform possible must come from the Government which had this question already under consideration, and he hoped that a law would be passed to standardise flour and other cereals. Although the rich have the option of making up for the loss of vital properties in the flour by the substitution of other foods, the poor, who live largely on the staff of life, are condemned to buy this deleterious product now on the market, and this League is doing a good work in striving to prove to the poor that their ideas of the value of white bread must be abandoned.

The three following resolutions were moved and carried at the meeting:

1. It is resolved that the Government be urged to direct attention to the importance of utilising during war time whole cereals, especially whole wheat meal (100 per cent.) (finely ground), whole wheat flour (about 80 per cent.), containing less indigestible woody fibre, oatmeal, unpearled barley, and unpolished rice, as they retain essential nutritive substances removed from ordinary cereal foods. Their adoption will reduce the consumption of meat as advised by the Board of Trade and help to promote the healthy economical nourishment of the people. The general use of finely-ground wheat meal would be an increase of over 25 per cent. to the bread supply of the nation in addition to providing a more substantial and nutritious food for the people.

2. It is resolved that the Government be asked to take measures to prevent the abstraction, without notification to the public, from whole meal, wheat meal, and household flour of the germ of wheat and of the strong gluten found in flours commercially designated "patents," and also to prevent the sale, without notification to the public, of flour which has been bleached, "improved" or otherwise adulterated.

3. It is resolved that these resolutions be sent to the Prime Minister, the Presidents of the Local Government Board, the Board of Agriculture, the Board of Trade, the Board of Education, the Chancellor of the Exchequer, and the National War Savings Committee, appealing to them to lead public opinion to recognise the importance of this subject; and that education authorities all over the country and women's societies be asked to co-operate in this educational campaign and thus help their country in this time of national need.

Dr. S. Rideal, F.I.C., J.P., in moving the third resolution, gave a valuable piece of information when he showed up the fact that our only statute regulating the sale of good bread was formulated as far back as 1836, and that, although this Act stated of what ingredients bread might consist, it made no provision whatever for the punishment of any persons who might use these ingredients in an adulterated form.

The Proportional Representation Society

This Society held its annual meeting on July 21 at the Westminster Palace Hotel, London, with the Rt. Hon. Lord Parmoor of Frieth, K.C.V.O., in the chair. It was evident from the Report and the general tenor of the speaking that the movement for proportional representation is steadily gaining ground; and Mr. Humphreys, the Secretary, testified to the warm appreciation which he had found for the scheme during his tour in Australia, New Zealand, the United States, and Canada. In several places, he said, it had been actually put in practice with very satisfactory results. Col. Anderson, in supporting the first resolution, said that the present party system was "an invention of the Devil" to keep people away from each other who ought to be working together; that Parliament should

be a reflex, not of the majority of the people, but of the whole people; that minorities should have a share of responsibilities instead of being driven into mere irresponsibility; and that the views of all, however mistaken, should have a hearing, for after all the best way to overcome error is to answer it with argument and destroy it. The Rt. Hon. Lord MacDonnell, G.C.S.I., amongst others, upheld the view that the present system of representation, which is proving itself to be a bad policy for England, is still more fatal when applied to Ireland, and that if all parties in that country felt sure of some representation much of the present dissension would abate. As the present electoral system shows signs of being scrapped in the near future, it surely behoves those at present unacquainted with this scheme to examine carefully these ideas of the "single transferable vote" and the "quota" which, if put into operation, would go far to bring governments on their path towards the ideal democracy.

The Programme of the British Science Guild

The Guild has issued a very important Memorandum on what it thinks should be done for the betterment of Science in Britain—proposing a National Statutory Board of Science and Industry. The signatures of a large number of influential persons are appended. We regret that we have not got sufficient space for the publication of it; but we recommend our readers to obtain copies from the Secretary, 199, Piccadilly, W. The matter will be considered together with other reports in our next issue.

This and That

Men of science will welcome the appointment of the Marquess of Crewe, K.G., to the high office of President of the Board of Education. He is President of the Imperial College of Science and Technology and an accomplished poet, and has commenced well by establishing committees to inquire into the position of science and modern languages respectively in our system of education. Sir J. J. Thomson, P.R.S., is the chairman of the former committee, Profs. H. B. Baker, F.R.S., and E. B. Starling, F.R.S., being among the members. We wish that Sir Ray Lankester had been one also, as he has done much for the movement which principally led to this result.

Our modern education has created a widespread culture which is humane, just, and accomplished, but also casuistic, and therefore almost entirely non-productive; and it is tending to become effeminate or even effete. At its best it generates types like the hero of Tennyson's *Princess*, and at its worst, ineffectuals like Herrick in Stevenson and Osbourne's *Ebb Tide*, who starved with a Vergil in his pocket. More science and modern languages, joined with Sir Baden Powell's admirable doctrines, will put life and backbone into the product. An ideal is attempted in the following Essay.

Most people have concluded by now that the war—the greatest disaster which has ever befallen Europe—was due to the misgovernment of the world by essentially wrong types of persons, who rule in the interests of themselves rather than of humanity or of their nation. The problem of the future is how to replace these persons by higher types; and the notes by Sir Harry Johnston and Lord Montagu, and Dr. Hodgson's review of M. Faguet's book, should be considered from this point of view. The notes furnish examples of M. Faguet's theory of *misrule by ostracism.* So long as we continue to believe that persons who have never seen

or done anything are as good as men of the widest experience and achievement, so long shall the world suffer from wars, massacres, injustice, destitution, insanitation, and crime. Let us hope that this war will abolish not only German militarism but the whole lying and canting democratic hypothesis of equality. The best men at the top is the only sound rule; and it remains to find how to put them there.

The *Glasgow Herald* of July 19 had a most able article on "Science as Cinderella," and this was followed on the 25th by a letter from Prof. Soddy, which suggested that a large part of Mr. Carnegie's endowment of Scottish universities for science had been appropriated by these bodies to other purposes.

All the British universities should be subjected, we think, to a fiery renovation. Remarks were published in SCIENCE PROGRESS of January 1914 and January 1915 on the doings of the Bristol University as regards the giving of honorary degrees and the subsequent dismissal of a professor who had objected to those doings. We now hear that this learned body has disembarrassed itself of another member of its staff who objected to both of these actions. The Board of Education is the proper body to look into these matters. It is paid for such work: why does it not do it? Is it because, as a correspondent has assured us, the whole country is managed by time-servers and invertebrates?

The Croonian Lecture of this year was delivered at the Royal Society on June 22 by Prof. S. J. Hickson, F.R.S. (Manchester), and dealt in an interesting manner with the two forms of symmetry—namely, radial and bilateral—found in animals. The gist of the lecture was that those classes of animals now sedentary or floating in habit tend to show a radial symmetry, while those that are actively motile tend to show the bilateral symmetry of form. The lecturer also noted that there is a far greater range of variability in radially symmetrical animals, so much so that he believes that in some radially symmetrical genera there is no such discontinuity of structure as would justify their division into specific groups; while, on the other hand, such a discontinuity certainly exists in the bilaterally symmetrical genera.

A ton of coal for half-a-crown! For a considerable time past the advertisement sheets of our newspapers have been informing readers that they can greatly increase the heat of their fires by adding certain remarkable agents to the coal. We once procured a sample of this stuff, carried out the instructions, and were crushed to find that there was no result at all! We had scarcely expected anything else, since if the mere adding of an ounce or two of powder to a mass of coal will so greatly enhance its heating capacities, the world would at once be in possession of an invention of quite a revolutionary character. How cheap it would then be to run our railway trains and men-of-war! The saving to the British taxpayer alone would amount to millions a year. In fact, we wonder that the purveyors of this kind of thing are driven to advertise their wares in the Press at all, for the whole world would be using the powders by this time. But what an extraordinary thing it is that such frauds should be allowed to be advertised for the express purpose of deceiving the ignorant public, and is there any other country in the world where such a thing would be permitted? On the other hand, the clever persons who do the advertising have probably succeeded in netting many thousands of pounds' worth of half-crowns. Doubtless the ingenious inventor of the idea, whoever he may be, will soon be made a baron and be given a seat on the Privy Council. We may heartily congratulate him; but what about the state of education of a people who are ignorant enough to be deceived by such advertisements? Could there ever be a better plea for more science in education?

The *Dublin Express* complains that SCIENCE PROGRESS "cannot be obtained

from any Dublin bookseller except to order," and wonders why this should be the case "in a city that is the seat of two universities." Alas, if Irish people, who are certainly the most charming and in some respects the most capable people in the world, had paid more attention to great science and great art and less to the most contemptible and fraudulent politics, Ireland would not now have been the "distressful country" they say she is. And if England had done the same, she would not now have been losing her sons at the rate of a thousand a day. Our rights are insignificant compared with our duties. Man may forgive, but nature never.

What a magnificent idol is the German Lie-God! In July the Kaiser is reported to have said that "the English built up during the years before the war the combination of countries which at the given signal fell upon us and attacked us, the most peaceful and most peace-desiring people in the world." What—we proposed to take Berlin with "our contemptible little army"! No, the world will not believe it. The *Times* says that the German casualties actually reported during the two years of war amount to 3,135,177, of whom 735,866 were killed. That is the price which the Germans have paid for worshipping Baal. And yet we read in the July papers that, when this man was at Cologne, "rich and poor uncovered their heads, and women without distinction of class greeted the War-Lord with waving handkerchiefs." Yes, men may forgive—but what about God?

The other day we asked a soldier in high position on leave from the front what his opinions were upon our capacity as a fighting nation. He replied that the individual British soldier is acknowledged to be worth two of the Germans, to be simply heroic as regards self-sacrifice, and to possess extreme cheerfulness under all circumstances. "One would think," he said, "that men who are living under the appalling conditions of the trenches would be morose or even despondent. Not so, however. The men are really wonderful in their cheerfulness." He had asked one senior man who had been through the war and was going home for a few days' leave, whether he was not glad to get out of the trenches. "Well, sir," said the man, "them trenches are not so bad as they are painted." Our informant also said that the junior officers are beyond all praise; and we must remember that these officers and men have been in no wise compelled to undergo these hardships, but have done so quite voluntarily. It is in fact a sublime spectacle such as has seldom been presented in the world before; and the advocates of free enlistment are right in this respect, that the picture shown by our armies is in a sense more noble than that shown by the Germans—though they are equally wrong in supposing that free enlistment is a proper business proposition for the conduct of a great war. On the other hand, however, our informant said that there his praise ended; that the people who managed our affairs in Parliament had failed to foresee almost everything and had prepared for nothing. In fact, he said, we as a nation possess a *genius for disorganisation*! That is clear from many other things besides the conduct of the war. But the Battle of the Somme shows that we are improving!

The British Medical Association has appointed a Committee to work with the Science and State Committee of the British Science Guild regarding the position and pay of pathologists.

ESSAY

Education from the Parents' Point of View (the Editor)

THE war has undoubtedly engendered a widespread feeling that all is not well with public education in Britain; and this feeling seems to have been brought to a head by the meeting and memorandum of the Neglect-of-Science Committee referred to in the July issue of SCIENCE PROGRESS; while, to judge from the press, the need for some reform appears to be admitted by many educationists themselves. On July 12, 19, and 26, a prolonged discussion, inaugurated by Lord Haldane, took place in the House of Lords on the subject. On July 6, a deputation representing the parents of boys at public schools (with Lord Desborough as chairman) waited upon certain members of the Headmasters' Conference in London in order to express their personal views;[1] and the Educational Supplement of the *Times*, *Nature*, and other organs have published numerous letters on the theme. It is clearly felt, as Lord Haldane stated, that now is the time when the business must be set in order.

A perusal of these discussions will probably lead the unbiassed reader to the conclusion that while there is very general agreement as to the list of items in the bill of fare, there is some difference of opinion as to the amount of each dish which is to be provided at the educational banquet. Briefly it comes to this, that the Reformers desire less Greek and Latin grammar and more science, modern languages, and history. Compromise might be thought to be easy, but unfortunately the modern educationist appears to be taking the ground that it is he alone who should decide the point. For instance the *Times* (July 28) says: "There are two pitfalls . . . the first is the danger of confusing reform of organisation with reform of curriculum. . . . The only compulsion which the educationist supports to-day is the compulsion to learn something. What that something shall be, in individual cases, beyond the elements common to all subjects, it is the first duty of the schoolmaster to decide in accordance with the aptitudes of the individual." In other words it is the schoolmaster who shall decide as to the boy's future career.

On the other side, the Report of Lord Desborough's committee gives the most decided contribution to the discussion from the parents' point of view. The committee first published in the *Times* of June 5 a memorandum urging reform, and sent copies "to the parents of all boys at Eton with a request for a reply either in favour of the suggestion therein contained or against it." The result was that "536 replies were received from Eton parents, of which 512 were in favour, 12 against and 12 neutral. Letters in favour of the movement were also received from a number of parents of boys at 26 other Public Schools, and from a large number of other parents and sympathisers. A précis of a few of the letters is subjoined." Undoubtedly, therefore, the voting of the parents was overwhelmingly in favour of reform; and a consideration of the abstracts from 37 letters shows

[1] The Report of Proceedings is issued by the Honorary Secretary, Public School Reform, St. Clere, Kemsing, Kent.

that most of the opinions are very strongly held. For example, with regard to the schoolmasters' demand voiced in the extract from the *Times* quoted above, one parent writes that he was "advised not to put the boy into Army Class, the reason given being that Army Class, in the Lower Division, is chiefly composed of undesirables and slackers, though the real reason is that the boy's tutor is a Classical Master and he does not wish the boy to give up Latin for a modern language." Unfortunately, before we trust our boys' careers entirely to their schoolmasters we must remember that even schoolmasters are sometimes human. Another parent writes that he, "as a business man with long experience, has felt strongly that the average Briton commenced his career very inadequately equipped for the struggle for the maintenance of our supremacy in trade—a struggle in which, prior to the war, we were, speaking generally, being slowly but steadily worsted." Another parent says that he himself "suffered in youth from classical education, which has helped him in no way"; and another that his "own education at Public School taught him the highest ideals in duty and discipline, but almost nothing else that was useful in the struggle for existence, or public duties." This correspondent also "protests most emphatically against antediluvian syllabus of instruction, and bitterly denounces arrogance and bigotry of scholastic staff of great Public Schools, who know nothing of the work-a-day world, especially overseas." Another complains that he must pay extra fees for the tuition of his son in such an elementary subject as geography; another that mathematics and French teaching at * * * are ridiculous, and that nothing taught at * * * is of any use in the Navy; another that many posts abroad cannot be filled by English boys as they have not had the necessary grounding; another complains of boys having to "grind at Latin verse, while being taught no history, geography or literature, and no colloquial French"; another objects to having his boy's "time wasted, as mine was, on a smattering of useless Greek and Latin. Since the stock-in-trade of most Oxford and Cambridge dons and schoolmasters is knowledge of Classics, they will fight hard against any change"; another says that he "has served a year in France; had over one hundred Public School boys to deal with; helplessness of boys greatly struck him; few had any idea of work or real devotion to irksome task . . . many French Tommies ahead of them in knowledge—literary and practical"; and another declares that he is forced to send his three sons to Public Schools "not in the hope of their receiving a profitable education, but because the curriculum happens to be bound up with advantages which cannot be secured in any other way."

We have seldom observed such unanimity in any series of independent opinions; and the worst of it is that any one who is acquainted with the world will recognise that these opinions are not confined to parents of Public School boys, but are common everywhere, except perhaps in scholastic and ecclesiastical circles. Rightly or wrongly, many people think that our boys are taught, not what they should be taught, but what the present type of educationist is generally able to teach—that our system of education is designed in the interests rather of the schoolmasters than of the boys. What such people wish to see reformed is, precisely, the curriculum, and not so much the organisation.

In order to study the point further, let us inquire what are the qualities and accomplishments which most parents would like to see in their sons—say in the ideally-trained young man of twenty. Whether, in Roger Ascham's sense, he is by nature *euphues* or no, he should still, we surmise, be brought up "in the book and the bow." His *outdoor qualifications* should comprise (1) military training; (2) athletics; (3) walking and running; (4) riding, bicycling, rowing, sailing,

shooting, fishing, motoring; (5) football, cricket, tennis; (6) endurance of fatigue, heat, cold, hunger, and thirst. His *arts* should comprise (7) reading, writing (legibly), arithmetic, mensuration, logarithms; (8) sketching, singing (or a musical instrument), dancing, photography, carpentry, camp-grooming, camp-cooking, road-engineering, map-drawing and map-reading; (9) intelligible talking and writing; (10) good personal manners. His *foreign languages* should comprise (11) the *first elements* of Greek, Latin, French, Italian, German, and Russian, none excepted; (12) the translation of easy Greek and Latin into English, but not necessarily the converse; (13) a colloquial efficiency in one or two modern languages. His *knowledges* should comprise the *outlines* of (14) modern geography; (15) general history; (16) mathematics (enough to understand its objects and uses); (17) mechanics, astronomy, physics, chemistry, geology, physiology, natural history, botanical and zoological classification and evolution, pathology, epidemiology; (18) agriculture, manufactures, economics. His *general reading* should comprise (19) poetry and the ancient mythologies; (20) histories of the great arts; (21) histories of law, politics, war, invention, science, ethics, and literature; (22) abstracts of philosophies; (23) all the greatest books in verse or prose, in original or in translation; and (24) the Scriptures. And, finally, he must hold (25) a sufficiently low opinion of his pleasures, privileges, and rights, and a sufficiently high one of discipline, honour, work, duty, and the other virtues.

A large programme, even if we use the words "first elements" and "outlines" in very narrow senses—but we have seen young men who have nearly achieved it, even after a "classical education." But these were probably exceptionally apt pupils, and the real question is, by what curriculum can we raise the ordinary boy to such a high level? There can be only one answer—by taking him to the top of the hill, so to speak, and showing him the whole great landscape of human knowledge, effort, and duty spread out before him. Then, when he leaves school, he will be able to judge better in which particular field he himself is fitted to work for the rest of his life, and will also be able to form some idea of the fields in which others are toiling, and to accommodate himself to any sudden calls which fortune may make upon him. The alternative course is to plunge him from the beginning into a deep mine, out of sight of heaven and earth, and there keep him grubbing for special knowledge which he may have no desire to possess and no natural aptitude to acquire. This is the admitted error of too early specialisation. Whether the mine be the classical mine, the theological mine, the mathematical mine, or the scientific mine, the result is much the same—not a man at all, but a Latin dictionary, a commentary, a demonstration, or a test-tube!

The grammarians have objected to science in education because, they say, it is a case of too early specialisation; but, as Dr. Mercier pointed out in the July number of Science Progress, the teaching of Greek to the young is early specialisation. So is the too-elaborate teaching of any one branch of knowledge. It is like exercising one arm or leg to the exclusion of the rest of the body; and every limb of the mind should be trained. Early education should be wide, not deep; for the depths must be reached later when the mind is fully grown. But parents have the right to take still more serious exception to the over-elaborate teaching of knowledge which will not be required in after life, because this occupies the time which might be used for giving a wider field of vision, or, if elaboration is demanded, for the elaboration of useful knowledge. Now science and languages are useful to many, but the composition of Greek and Latin verses to few; and this precisely is the justification of the complaints mentioned above. After all, a public-school education costs from £500 to £1,000 or more, and is therefore a

"business proposition." It is the parents who pay the piper, and they will be wise, we think, if they insist upon calling the tune.

Very amazing arguments have been heard during these discussions. Thus, some have maintained that the admirable voluntary recruiting in Britain has been due to our "humanistic" system of education, and that the wickedness of the Germans has been due to the scientific tendency of theirs. They forget that universal service, which is national voluntary recruiting, has been in force in Germany for fifty years, whereas it was introduced in Britain only after eighteen months of actual war, and then not without much disgraceful opposition. Moreover, no one denies that the Germans as a nation have at least been efficient, whatever their rulers may have been; whereas many think that our attitude towards the German menace has constituted ever since the cession of Heligoland in 1890 one of the most colossal pieces of stupidity known in history. How poor science can have stimulated German brigandage it is impossible even to conceive, and the German militarists who produced the war were probably as ignorant of it as the British politicians who gave them an opportunity. It is more likely that the war was caused by the false ideals engendered in both nations by unreal and unscientific methods of education.

Another common pretence is that the grammatical education stimulates love of literature—the truth being that the two things are quite distinct, and that boys who have been through the grammatical mill have often never been through a single masterpiece in the language which they have learnt at such cost. That it specially stimulates a sense of duty is another hypothesis for which no evidence is given or is apparent. Yet another pretence is that a scientific education makes a man a "brutal materialist." This is a meaningless term of opprobrium which A hurls at B when B happens to criticise the particular dogma or superstition by which A gets his living or advancement; and the use of it is evidence only of obtuseness or senility in the user.

But few will be misled by such arguments, and we believe that most men of the world will summarise the discussion as follows: (1) That the first elements both of Greek and Latin are necessary for every intellectual profession or employment; (2) that a complete classical education is necessary for very few of such occupations; (3) that an exclusive classical education is not sufficient for any such occupation; (4) that a knowledge of one or more modern languages is more useful than and just as educative as a similar knowledge of a dead language; and (5) that a man who is entirely ignorant of science can scarcely be considered educated.

But popular criticism is directed not only against the curriculum but also (1) against the want of discipline which often prevents masters from being able to compel their pupils to learn anything; and (2) against the system by which nearly all the better-paid scholastic and academical posts are given to men who belong to an out-of-date order of things. On the other hand, every one recognises that many educationists are themselves trying to introduce reforms. Parents must, however, remain on guard. We do not wish the schoolmasters, however excellent they may be, to take complete charge of our boys (and of the future of our country), just as some centuries ago the priests took charge of our souls.

REVIEWS

MATHEMATICS

Logarithms

Improved Four-Figure Logarithm Table. Multiplication and Division made Easy. By GEORGE C. MCLAREN, Fellow of the Faculty of Actuaries, Scotland. [Pp. 27.] (Cambridge: at the University Press, 1915. Price 1*s.* 6*d.* net.)

Interpolated Six-Place Tables of the Logarithms of Numbers and the Natural and Logarithmic Trigonometric Functions. Edited by HORACE WILMER MARSH, Head of Department of Mathematics, School of Science and Technology, Pratt Institute. [Pp. xii + 155.] (New York, John Wiley & Sons; London: Chapman & Hall, Ltd., 1916. Price 5*s.* 6*d.* net.)

Five-Figure Mathematical Tables. Together with an Explanatory Introduction and Numerous Examples. Compiled by E. CHAPPELL, B.SC., A.C.G.I., Assoc.M.Inst.C.E. [Pp. xvi + 320.] (London: W. & R. Chambers, 1915. Price 5*s.* net.)

THE first of these books was reviewed briefly in our Book List for July 1915. The little book contains only twenty-seven pages and is furnished with marginal thumb index. As we stated, the principle is to simplify by leaving the determination of the characteristic to ordinary arithmetical consideration—and, indeed, this is often the most troublesome part of a logarithmic computation. Another innovation is to denote the remainder of the logarithm after the first four figures by the use of the full stop for a fraction of about one-third and by a colon for a fraction of about two-thirds. The pamphlet is of course limited only to logarithms of numbers, but is so well arranged that the reader will find it useful for small current calculations, such as may be used in classes, and other rough statistical work.

In the preface to Mr. Marsh's book he says, "Why a six-place table? The teacher of conventional mathematics who endeavours to impart a knowledge of theory through the use of the artificial problem whose data are fictitious or 'contrary to fact,' can have no answer to this question. In problems of this type the results obtained by solution have no part in human industry, and it is immaterial whether they are expressed in three figures or in more. If interpolated alike, a three-place table is therefore as good as a five or greater place for such computation, and even better, because necessitating the use of fewer figures. In industrial and technical problems, however, the result is of supreme importance and the number of figures is fixed by standard practices of manufacture. In simple

logarithmic computation which does not involve the log or transitions from non-tabular to tabular, it is a well-known fact that the result is correct, in any event to within one less significant figure than the number of places in the table. If, therefore, as frequently is the case, a result must be exact to the fifth significant figure, a six-place table, and no less, is a necessity. In the preparation of these tables, no pains have been spared to have them accurate. Some two hundred students have assisted in the reading of the proofs, and no page has been read by less than ten different persons, with six-, seven-, and eight-place tables. The table of circular arcs was computed by over one hundred students and checked until all results agreed." A heavy ruling is used to distinguish the change in leading figures when these occur in the same line, in place of the light ruling which is sometimes employed, as, for instance in Chambers's "Mathematical Tables." Another advantage is that differences are given at the bottom of the page. On the other hand, we think that the type should have been blacker; but as the paper is fairly thick, the present light type is still easily legible. Some of the trigonometrical tables, such as those on pages 38 and 39, look very crowded. The preliminary instructions for the use of the tables are clear enough for the most ignorant reader, and there are additional tables giving United States and English measures, decimal equivalents of some of these measures, specific gravities of materials, etc.

Mr. Chappell's book contains several innovations. He suggests a number of new abbreviations—namely, Cologs, Illogs (Antilogs), Lologs (Logs of Logs), Illologs (Antilologs). We find it rather difficult to trace explanations for each set of tables in the preface, and wish that references had been given on the title page of each such set of tables. For instance, it takes one some time to ascertain what is the meaning of the figures printed red. Trigonometrical functions and their logarithms are given at the end of the book. The printing is very clear.

It is unfortunate that in all these books, good tables of natural logarithms are not given. In the last-named work the author says specifically that he has not included such tables because these logarithms are "readily calculated by means of lologs." But nevertheless even such ready calculation is often very troublesome to a fatigued mind or to a busy worker. Yet natural logarithms are being used more and more owing to the frequency of exponential functions in statistical work, and we really wish that new books of logarithms would deal with them better. We have personally actually had to fall back for these tables upon such books as Dr. A. S. Percival's *Practical Integration* (Macmillan), where there is a one-page table.

Dr. W. P. Workman's admirable *Memoranda Mathematica* (Clarendon Press, Oxford) contains small but good logarithmic tables, including trigonometrical ones; and we cannot resist mentioning this fact because the book is so generally useful for all kinds of mathematical work.

We have received the Prospectus of an entirely new publication, the *Control Table of Logarithms* (Four to Twenty-one Places) for the use of Mathematicians and Calculators, Constructed by Percy Ramsey-Kent (C. & E. Layton, 56 Farringdon Street, London). We are informed that this "being a control table, the calculator can by its contents prove its own figures with singular ease, absolute certainty, and, therefore, absolute conviction to himself"; but the methods of the table are not clear. Apparently, from an accompanying letter, the author wishes to sell the invention to which he refers.

(1) **Fundamental Conceptions of Modern Mathematics.** Variables and Quantities, with a Discussion of the General Conception of Functional Relation. By ROBERT P. RICHARDSON and EDWARD H. LANDIS. [Pp. xxi + 216.] (London and Chicago: The Open Court Publishing Company, 1916. Price $1.25 net.)

(2) **Numbers, Variables, and Mr. Russell's Philosophy.** By ROBERT P. RICHARDSON and EDWARD H. LANDIS. [Pp. 59.] Reprinted from *The Monist* of July 1915. (London and Chicago: The Open Court Publishing Company, 1915.)

ON first reading these books, one is not at all favourably impressed by them. It appears remarkable that nearly all the leading mathematicians should so often be in the wrong, while Messrs. Richardson and Landis are always in the right—especially when we observe several small possible flaws, such as split infinitives. We do not know whether split infinitives have ever been absolutely proved to be vicious, but they are a kind of vegetable which grows largely in this particular garden. A greater fault appears to be that in the first book the whole matter is not set out in well-contrived compartments, but the authors ramble on indefinitely from the first page to the last. Indeed the first book is only the first part of the large subject stated at the top of the title-page; and at the end of the book the authors give us brief accounts of no less than twelve other parts which they project. As we said, it ought in our opinion to have been arranged in a more formal manner. There is art as well as science in the writing even of books of science. Perhaps the final test of such books is the test which Euclid has withstood for two thousand years. That poor discredited volume still remains with us. By the universal consent of modern mathematicians the opening postulates, definitions, and axioms are far from perfect: but nevertheless the work proceeds in an orderly manner from proposition to proposition, and the result is that the work has been done and finished once and for ever. We cannot say the same of this book. On reading one page we are driven often to hunt up at random the results of previous or future pages, and the final impression is therefore confusing. Still worse, we are personally doubtful whether the authors have really seen to the bottom of the bucket; and in science it often happens that the most important facts lie there. As Byron remarked, "When Bishop Berkeley said there was no matter, and proved it—'twas no matter what he said"—because the Bishop had entirely overlooked the fact that the mind possesses a kind of muscular sense which enables it to distinguish between real and ideal cognitions. Similarly the writer of this review thinks that the authors have failed to draw the very important distinction between operations and the arguments to which they may be applied, and he therefore believes that the whole of the work will ultimately have to be rewritten. The omission would probably have never occurred if the authors had adopted the wise exemplar of Euclid.

Now let us haste to say that our first impression was by no means altogether sound. The books are both very interesting, and the strictures on the errors of the great mathematicians indicate only the enthusiasm which the authors have brought to their studies; and we gladly admit that their criticism lies well within the concluding paragraph of their Preface. Indeed many people might think that a rambling naturalists' excursion such as this will gather more specimens than will a formal search; and many of the authors' criticisms, distinctions, expositions, and labellings appear to be of distinct value for future work in the philosophy of mathematics—subject, however, to the necessity that the final book upon this subject does reach nearer to the bottom of the bucket. For example,

we doubt whether the discussions on negative quantities, on real and imaginary quantities, and on quaternions are any more than skimmings from the surface. In reality signless numbers, negative numbers, and neomonic numbers belong to three entirely different arguments, the first to the arithmetical one, the second to the linear one, the third to the rotational one.

Thought is continuous, but words are discontinuous. One might as well try to express a continuous curve by a series of isolated points as to express the whole truth in any book. This is the perpetual trouble of philosophy. And this is also the reason why our authors are able to find so much in the great mathematicians to criticise. The great mathematicians recognised the difficulty and avoided it entirely by ignoring it. That is, they trusted the mind of the reader to understand more than was written. Now we are quite certain that Hamilton understood as much about quaternions as our authors understand; but he had other things to do than to endeavour to plot out his curve in infinitesimal intervals. Indeed this is at the bottom of the feeling that the philosophy of mathematics is not really productive. We can spend a volume in proving that two and two make four, and indeed we write volumes on the subject; but somehow they appear to be unnecessary. Not only the world, but, we think, many mathematicians look on with some amusement as they watch philosophers trying to put infinity into boxes. Nevertheless such works as these are at least interesting—though we scarcely think that they are so fundamental as the authors would appear by their Preface to believe.

The second book or rather pamphlet is mostly an attack upon the tenets of Mr. Bertrand Russell. When two philosophers attempt the task mentioned above, their boxes are apt to be of very different sizes, so that their measurements often disagree. We suspect that really there is much more agreement than might be imagined, and that the trouble arises from little more than the finity of the words they use. We cannot understand the authors' contention that there is more than one number one. Should we say that if two billiard balls have the same redness, each has a different redness? The attributes may be the same, though the things which possess them are different.

AMATEUR.

Euclid's Book on Divisions of Figures (περὶ διαιρέσεων βιβλίον): with a Restoration based on Woepcke's Text and on the *Practica Geometriæ* of Leonardo Pisano. By RAYMOND CLARE ARCHIBALD, Ph.D., Assistant Professor of Mathematics in Brown University, Providence, Rhode Island. [Pp. viii+88.] (Cambridge: University Press, 1915. Price 6*s*. net.)

OF the nine works which Euclid is known to have written, approximately complete texts, all carefully edited, of four are in our possession. Of four of the others we only possess more or less fragmentary knowledge from mention or comment by other Greek writers. The remaining work is the book *On Divisions of Figures*, and Proclus alone among the Greeks made an explanatory reference to it: "For the circle is divisible into parts unlike in definition or notion, and so is each of the rectilineal figures; this is, in fact, the business of the writer of the *Elements* in his *Divisions*, where he divides given figures, in one case into like figures, and in another into unlike" (p. 1 of the book under review). A copy in Latin of a treatise, *De Divisionibus*, by one "Muhammed Bagdedinnus," was given in 1563 by John Dee to Commandinus, who published it in Dee's name and his own in 1570. The original manuscript from which Dee's copy was made was, for all useful purposes, destroyed by fire, and though the Dee manuscript is referred to by many eminent historians of Euclid's works, the conclusions of Heiberg followed by Heath are:

"The Arabic original could not have been a direct translation from Euclid, and probably was not even a direct adaptation of it; it contains mistakes and unmathematical expressions, and, moreover, does not contain the propositions about the division of a circle alluded to by Proclus. Hence it can hardly have contained more than a fragment of Euclid's work" (p. 9). But in an Arabian manuscript found by Woepcke at Paris, and translated by him into French and published in 1851, we have apparently a translation of much of Euclid's work. "It is expressly attributed to Euclid in the manuscript and corresponds to the description of it by Proclus. Generally speaking, the divisions are divisions into figures of the same kind as the original figures—*e.g.*, of triangles into triangles; but there are also divisions into 'unlike' figures—*e.g.*, that of a triangle by a straight line parallel to the base. The missing propositions about the division of a circle are also here: 'to divide into two equal parts a given figure bounded by an arc of a circle and two straight lines including a given angle,' and 'to draw in a given circle two parallel straight lines cutting off a certain part of a circle.' Unfortunately the proofs are given of only four propositions (including the two last mentioned) out of thirty-six, because the Arabian translator found them too easy and omitted them" (quotation from Woepcke on p. 9). It is on Woepcke's text and on a manuscript in the Vatican Library, written by Leonardo Pisano in 1220 and named *Practica Geometriæ*, that Prof. Archibald's restoration of Euclid's work (pp. 30-77) is based. As Prof. Archibald says (p. vii): "A score of the propositions are more or less familiar as isolated problems of modern English texts, and are also to be found in many recent English, German, and French books and periodicals. But any approximately accurate restoration of the work as a whole, in Euclidean manner, can hardly fail to appeal to any one interested in elementary geometry or in Greek mathematics of twenty-two centuries ago." Thus it is easy to see that this restoration is of great value as a source of elementary geometrical problems. But Prof. Archibald's work has a value far beyond this: in the introduction on the various manuscripts which come into consideration (pp. 1-18), in the account of writers prior to 1500 who have dealt with propositions of Euclid's work (pp. 19-28), and in the selection from the very extensive bibliography of the subject since 1500 (pp. 78-85), Prof. Archibald has shown so much research, critical power, and profound learning that his little book will take a permanent place in the literature of the history of mathematics.

PHILIP E. B. JOURDAIN.

METEOROLOGY

Atmospheric Circulation and Radiation. A Meteorological Treatise on the Circulation and Radiation in the Atmospheres of the Earth and of the Sun. By FRANK H. BIGELOW, M.A., L.H.D. [Pp. xii + 432, with 78 figures.] (New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1915. Price 21*s*. net.)

METEOROLOGY is a science in which an enormous mass of observations have been collected, but in which little has been done in the direction of theoretical discussion of the observations leading to definite results which could be of use in what is essentially the final aim of meteorology—to understand the causes which are operative in producing the circulation in the atmosphere and to apply the information so gained to foretell the probable weather. So much is this the case that a correspondent in *Nature* could comparatively recently assert, not without some justification, that a mere rule-of-thumb mechanical "prediction" would be at least as often correct as the official predictions. Prof. Bigelow, who from

1891–1910 was Professor of Meteorology in the U.S. Weather Bureau, and since 1910 has been in charge of the Argentine Meteorological Office, ascribes this lack of theoretical progress to two reasons. Firstly, that the observations are usually recorded in practical units, and these not in one system only, so that considerable reduction is necessary before they can be used in formulæ. Thus, for example, the barometric pressure is expressed usually in inches or millimetres of mercury. Reductions and intercomparisons of observations would be enormously simplified if they were always recorded in one definite system of absolute units, say the centimetre-gramme-second-absolute temperature units. The importance of this step to meteorological science cannot be overestimated, and, in this country, Sir Napier Shaw, Director of the Meteorological Office, is striving to secure the general adoption of such a system.

The second reason advanced by Prof. Bigelow is that theoretical discussion has always proceeded on the assumption that the conditions in the atmosphere are strictly adiabatic, and that the specific heats are constant throughout. A little reflection will show that these conditions are not likely, in general, to be satisfied, and investigation shows that they cannot be as long as there is any circulation or radiation in the atmosphere. The greater portion of this volume consists therefore of the derivation of a system of equations in which the specific heats and gas-constant are not assumed to remain constant (although the ratio of the specific heats does so), and of their applications to the explanation of many meteorological phenomena. The volume thus gives in outline many of Prof. Bigelow's own researches, which have proved very successful in forecasting weather conditions in the United States.

Prof. Bigelow claims in this way to have given solutions of the following problems, which have not previously been satisfactorily solved :—

1. The diurnal convection and the semi-diurnal barometric waves, with the radiation.
2. The pressure and temperature in cyclones and anticyclones, with the circulation and radiation.
3. The thermodynamics of the atmosphere from balloon ascents to great altitudes.
4. The thermodynamics of the general circulation.
5. The distribution of the radiation in all latitudes and altitudes up to 20,000 metres.
6. The "solar constant" of radiation and the conflicting results from pyrheliometers and bolometers.
7. The discrepancy in the absolute coefficient of electrical conduction as derived from the several apparatus for dissipation and for the number and velocity of the ions.
8. The diurnal magnetic variations in the lower strata of the atmosphere.
9. The non-periodic magnetic variations in their relation to the solar radiation.
10. The magnetisation and electrical terms in the sun at very high temperatures.

The above ten headings will give some idea of the wide range covered by the book. The treatment is not uniformly good, particularly in the mathematical part. The practice of numbering equations, many of which are merely steps in an argument or transpositions of other equations, consecutively through the book up to a total of almost 800 is wearisome. Also the writing of Cv and Cp instead of C_v and C_p for the specific heats, although perhaps convenient for the printer, is very

confusing in some formulæ. The number of typographical errors which we have noticed throughout the book is very large. On the other hand, the greater part of the application of the formulæ to the observational material will be found to be of considerable interest. The portion dealing with simple vortex systems shows how the results have been used in weather prediction with considerable success. The material provided by one of the international balloon ascents is fully worked out and the method of deriving the radiation and circulation at different levels explained. It appears that progress in theoretical meteorology is now largely dependent upon a considerable extension of balloon observations taken with self-recording instruments up to as high an altitude as possible. The value of a knowledge of the prevailing conditions in the upper atmosphere which may be thus derived will be of great value in weather forecasting.

One of the subjects very fully discussed is the value of the "solar constant" of radiation. Various authorities claim that the value approximates to 2·00 calories, others that it is 4·00 calories. Which of these values is correct depends upon whether there is an inappreciable loss of radiation in the very high cirrus layers of the atmosphere or whether about half the radiation is there reflected back into space. Prof. Bigelow inclines to the latter view and the higher value of the constant, but the matter cannot yet be regarded as at all settled. Much more work needs to be done by self-recording balloon pyrheliometers reaching to very high altitudes. Valuable work is being done in this direction by C. G. Abbott of the Smithsonian Institution, who strongly supports a value slightly less than 2 calories for the solar constant.

We could wish that Prof. Bigelow had been a little more appreciative of the work of other investigators, even when they differ in their conclusions from him. We can excuse this if we consider this book simply as a summary of his own researches, and as such can recommend it to meteorologists, especially those who are interested in the theoretical side of the subject, both as a volume for reference and as indicating lines along which research may profitably be pursued in the future.

H. S. J.

PHYSICS

The Physical Properties of Colloidal Solutions. By E. F. BURTON, B.A., Ph.D. (Monographs on Physics, edited by Sir J. J. THOMSON, F.R.S., and FRANK HORTON, D.Sc.) [Pp. vii+200, with 18 illustrations.] (London: Longmans, Green & Co., 1916. Price 6*s*. net.)

THIS monograph contains a clearly written account of the colloidal state as far as it is understood at the present time. The subjects dealt with are: preparation, classification, ultra-microscopic observations, Brownian movement, optical properties of colloidal solutions, the size of ultra-microscopic particles, cataphoresis, coagulation of colloids, stability of colloids, some practical applications of colloidal solutions. The treatment is largely from the physical standpoint, considerable space being devoted, for example, to the theory of the ultra-microscope. The author early draws attention to the incorrectness of speaking of colloidal substances as a particular class. Recent work, more especially the work of von Weimarn, has shown that almost any substance which exists in the solid state can be produced as a colloid or as a crystalloid, it being largely a question of concentration and the nature of the medium. Consequently one ought to speak of the colloidal *state* just as one speaks of the liquid or gaseous states. The various methods of preparing colloidal solutions are very clearly summarised. The question of their possible classification is one of considerable difficulty. Indeed,

at the present stage it is doubtful if classification has any particular value. In connection with Brownian movement, which has in recent years served to bring the colloidal and emulsoidal state into close relation to physics, a very useful table is given of the values of the molecular constants (*e.g.* the Avogadro number and the electronic charge), determined by very different methods. The list, however, should have contained the results of Millikan's oil-drop method. The phenomenon of cataphoresis, to which Dr. Burton himself has contributed much, is particularly well treated. It includes a comprehensive table of the mobilities of suspensions, suspensoids, and emulsions, an interesting comparison being drawn between these mobilities and those of electrolytic ions. The still unsolved question of the source of the charge carried by a colloidal particle is discussed both from a physical and a chemical point of view. The author himself evidently favours a chemical explanation, and much of the research recently carried out points strongly in this direction. It must be confessed, however, that the data are still too meagre to allow one to come to a definite conclusion on the matter. As the author says in concluding the monograph: "In spite of the immense amount of work that has been done during the last fifteen years, it is quite apparent, that both from a theoretical and a practical point of view, there still is need for much additional research before one would be able to suggest with any degree of satisfaction a general theory of the colloidal state." This is somewhat disappointing, but its realisation ought to be a strong incentive to further research in this great branch of physical chemistry.

W. C. McC. Lewis.

Text-Book of Practical Physics. By H. S. Allen, M.A., D.Sc., and H. Moore, A.R.C.Sc., B.Sc. [Pp. xv + 622, with 297 diagrams.] (London: Macmillan & Co., 1916. Price 8*s.* 6*d.* net.)

There is unquestionably an opening for a standard work on Practical Physics for students who are not sufficiently advanced to use Watson's well-known text-book, and Dr. Allen and Mr. Moore's book is admirably designed to fill the gap. It has been based on the manuscript instruction sheets used in the Physical Laboratory at King's College, London, and includes all the experiments likely to be required in a Pass or Honours Intermediate Course. No previous knowledge of practical work is assumed; but it would not be suitable for those whose theoretical knowledge is much below matriculation standard. The theoretical as well as the practical side of the experiments is dealt with at considerable length, with the inevitable result that the book is rather bulky and expensive; it is, however, also issued in three separate parts. These divisions are: I, Properties of Matter, including an excellent course in Practical Mechanics intended, in the first instance, for the benefit of Engineering Students in the University of London; II, Sound, Light, and Heat; and III, Magnetism and Electricity. Save in a few unavoidable cases the apparatus described is all of that simple description which, calling for a certain amount of care and skill to give accurate results, is best suited for students at this stage of their work.

The book is very thorough and very complete, but on examining it closely a certain sense of disappointment is produced by the fact that it is also rather ordinary. There are none of the special refinements either in manipulation or in modes of calculation that might be expected from authors of such experience; there is nothing, or very little, that is not well known to every experienced teacher; on the contrary there are just a few things which all those intent on the best

possible methods will feel to be rather weak. To give three examples out of perhaps a dozen: no reference is made to any simple difference method for measuring time periods, the student is only told to determine the time taken for fifty swings; again, in the calibration of a thermometer it is never desirable to detach a mercury thread by applying a flame—a suitable length can always be obtained quite safely from ordinary thermometers by jerking them; finally, in the magnetometry no hint is given as to the limits within which the approximate cube law of magnetic force may be applied, and there is no mention at all of the more accurate calculation which can be made if the deflections are taken at two distances instead of one.

Hardly sufficient stress has been laid on the accuracy needed for the individual measurements in an experiment in order to obtain the best possible result. The beginner never thinks of this for himself, and it should be brought to his notice whenever possible. A notable opportunity occurs in calorimetry, where the accuracy of the final result is usually limited by that with which the small rise of temperature in the calorimeter can be determined. In spite of this the specimen data quoted as an example of a latent heat of fusion of ice experiment on p. 351 are as follows:—Masses of calorimeter, etc., 40 gm., 240 gm., and 262·9 gm. Temperatures 10° C. and 20° C. Such figures would call for a good deal of red ink if presented in a laboratory note-book! And even intermediate students should be provided with a thermometer divided into tenths for measuring the calorimeter temperatures.

Space does not permit of a detailed criticism of the separate parts, but a few words of praise must be given to the section on current electricity which occupies a quarter of the whole book. It is not overloaded with a great number of unimportant experiments, representing all the freakish combinations likely to be devised by an examiner out for "something different" at all costs; while on the other hand it does contain every important one described with painstaking care and clearness. Many descriptions are followed by notes, as, for example, concerning the conditions under which the method may be employed with advantage. This information is generally left to be imparted by the demonstrator; it is only too often forgotten at once and ought to be given permanent record in the textbook. A few simple experiments with dynamos and motors are included for those proceeding to a course on electrical engineering, and the section closes with a very excellent collection of notes on electrical apparatus.

The book contains a collection of useful data, a selection from Castle's Mathematical Tables and an adequate index. The line diagrams and printing are all that could be desired, but the binding is rather light, and the paper seems a little soft to stand the hard wear that such a book is destined to meet with.

D. Orson Wood.

CHEMISTRY

Chemistry in the Service of Man. By Alexander Findlay, M.A., D.Sc., F.I.C. [Pp. xiv + 255, with three portraits and 23 diagrams in the text.] (London: Longmans, Green & Co., 1916. Price 5*s*. net.)

It will be the task of some cynical historian of the future to point out that it needed the greatest and most devastating war the world has ever seen to force the British Empire to acknowledge the existence and uses of the Sciences on which its welfare and security ultimately depended. The publication of the present work is, however, a sure symptom of that national awakening to the manner in which

Chemical Science and Industry have been neglected in this country in the past, and a hopeful augury of a fundamental reorganisation of the educational and industrial forces of the country which it connotes.

In the preface Prof. Findlay explains that at the end of the past year he was invited to deliver the Thomson Lectures before the United Free Church College, Aberdeen, and chose as his subject the indebtedness of mankind to Chemistry. These lectures have now been expanded into book form, and form the basis of the present work.

Prof. Findlay begins with a general introduction giving the salient features of chemical theory, such as Dalton's Atomic Theory, and so on. He then proceeds through the consideration of the phenomena of combustion to the examination and explanation of the chemistry of illuminants, and thence to explosives, etc. Later chapters deal with cellulose, catalysis, and fat-hardening, the fixation of atmospheric nitrogen, soap, molecular structure, and synthetic organic chemistry.

The chapter on the fixation of atmospheric nitrogen is of particular interest at the present moment, when the Germans are dependent almost entirely on these methods for the production of nitric acid for their high- and propulsive-explosives. Prof. Findlay's words on page 130 are worthy of note: "But when we contemplate the position which our own country occupies in this matter (the fixation of atmospheric nitrogen), we must confess to a feeling of great disappointment. Norway, Germany, Austria, Italy, France, the United States, Canada, India, Japan, in fact practically every civilised country but our own, is actively engaged in developing the nitrogen industries." It is surely a national disgrace that we alone of all civilised powers are so devoid of appreciation of scientific facts that such words can be written.

As regards the general structure of the book there is little to criticise, but on one point one may venture a suggestion—namely, that there should be far more illustrations. The man in the street—and for such the book is written—is not thrilled by portraits of dead celebrities. Photographs of modern works and plant would excite his interest far more. One may quote in this connection the words of Dr. E. James at the inauguration of the new Chemical Laboratories of the University of Illinois: "We cannot expect the average man to appreciate the importance of certain phases of scientific work. . . . I have come around quite strongly to the view that the most direct method of educating such people as to the importance of certain things is to let that importance be shown in some visible structure of steel and iron, of concrete and brick, which impresses his senses and may impress his imagination . . . the existence of (such buildings) creates *prima facie* a case for the importance of the subject," and, by parity of reasoning, a more lavish supply of illustrations dealing with the technical side of chemistry, such as the Odda Nitrogen Works or those at Niagara for instance, a few pictures of the inside of a soap works or paper factory, would at once whet the appetite of many a casual reader and persuade him to delve more deeply in it, and, further, the pictures would impress the theories in his mind.

Perhaps when the second edition of the book is issued it may be possible to bear this suggestion in mind.

Prof. Findlay is to be congratulated on having produced so interesting and readable a book.

FREDERICK A. MASON.

A Senior Experimental Chemistry. By A. E. DUNSTAN, D.Sc.(Lond.), F.C.S., and F. B. THOLE, D.Sc.(Lond.), F.C.S. [Pp. xiv + 522, with 120 diagrams by E. D. GRIFFITHS, B.Sc.] (London: Methuen & Co., Ltd., 1916. Price 5*s*.)

IT is somewhat difficult to criticise the appearance of yet another text-book of chemistry when, as in the present case, no explanation is afforded as to its filling the usual "long-felt want" or in what respect the existing text-books fail to satisfy the needs.

At the same time, whether or not the urgency was sufficient to warrant the appearance of a new text-book of inorganic chemistry, there can be no doubt that the present work has been carefully compiled, and should serve its purpose well—namely, that of providing a suitable book on inorganic chemistry for the use of "boys in the upper forms of Secondary Schools and students in Technical Institutions."

The tables comparing the chief properties of the elements of the various groups of the periodic system are certainly to be commended, and the directions for performing various experiments interspersed through the text ensure that theory and practice go hand in hand.

The volume also has two appendices on qualitative and volumetric analysis respectively.

The type is clear, and the diagrams, with which the text is fairly liberally supplied, have the merit of having been apparently drawn from actual laboratory experiments.

FREDERICK A. MASON.

GEOLOGY

Notes on the Fenland. By T. MCKENNY HUGHES, M.A., F.R.S., Woodwardian Professor of Geology, with a description of the Shippea Man, by ALEXANDER MACALISTER, M.A., F.R.S., Professor of Anatomy. [Pp. 35.] (Cambridge University Press, 1916. Price 6*d*. net.)

IN this pamphlet the author concludes that the peaty ("turbiferous") beds of the East Anglian fens are sharply divided from the gravels of the preceding or "areniferous" series, and that there was "an unrepresented period of considerable duration" between their epochs of formation. The mammalian faunas of the two series are distinct, and the Fen Beds include nearly the whole of the Neolithic stage. The human remains found at Shippea Hill, near Littleport, are referred to the Bronze Age. The gravels that occur on the margins and floor of the Fenland are believed by the author to record an epoch of uplift "out of the Glacial Sea," and the fens accumulated in the estuaries caused by a subsequent submergence. A reference to Dr. Holst's papers in the *Geological Magazine* would have been of interest; but their recency may have rendered this impossible.

The Gravels of East Anglia. By T. MCKENNY HUGHES, M.A., F.R.S., Woodwardian Professor of Geology. [Pp. 58.] (Cambridge University Press, 1916. Price 1*s*. net.)

IT is not clear why this essay appears as a separate work, to be received coldly by librarians, and why it is cut off from its many kindred in the *Geological Magazine* or the *Quarterly Journal of the Geological Society*. It contains scarcely any references to the abundant literature of the East Anglian gravels, and is evidently addressed to specialists to whom a century of observation is familiar.

It may be valued locally for its sections of pits which are notoriously liable to change or destruction. The author supports the view that the boulder clay and the contorted beds of the Cromer coast are products of floating ice. His main thesis, however, is that, "whatever their origin, the beds of the Norfolk cliffs can be traced up to the East Anglian peneplain, and that the Boulder Clay and Plateau Gravel are only part of one and the same series." Owing to the irregular and intermittent nature of earth bulges and depressions, he does not hope to trace any type of deposit in East Anglia as contemporaneously formed over a wide area, though the succession of types in different areas may be the same. We gather that the indications of climatic change in our Pleistocene beds, including those of the Glacial epoch, are ascribed to comparatively small oscillations in the level of the land and in the relations of land and water.

Methods in Practical Petrology. By H. B. MILNER, B.A., F.G.S., F.R.G.S., and G. M. PART, B.A., F.G.S., F.R.G.S. [Pp. 68, with 6 illustrations.] (Cambridge: W. Heffer & Sons, Ltd., 1916. Price 2*s*. 6*d*. net.)

THIS little book purports to be a practical companion to standard works on petrology. Its practical methods, however, resolve themselves into a somewhat trite series of directions for the preparation and examination of thin sections of rocks, the use of micro-chemical methods (staining), and the mounting of sands and crushed rock material. These hints will no doubt be useful to the student in elementary petrology; but in a book of barely 20,000 words it would have been wise to have severely limited the matter to practical methods, and not have tried to cram in ineffective notes on rock composition and classification, with tables of refractive indices and birefringences of minerals. There is an appendix on methods of preparing stains which is unnecessary, for even now it is easier for the student to procure stains than to make them himself. Moreover, micro-chemical methods are rarely used in comparison with the more exact optical, and are useless save in expert hands. The authors state that very little literature on these practical methods is available to the student, but they have omitted to give a reference to the best, biggest, and latest work on this subject—Johannsen's *Petrological Methods*. There are misprints on pp. 8, 13, and 39, and "siliceous" is misspelt throughout on the latter page. The book is most likely to be useful in the sections devoted to the mechanical preparation of material for petrological examination.

G. W. T.

ZOOLOGY

The Rat. Reference Tables and Data for the Albino Rat (*Mus norvegicus albinus*) and the Norway Rat (*Mus norvegicus*). By HENRY H. DONALDSON. [Pp. v + 278.] (Memoirs of the Wistar Institute of Anatomy and Biology, Philadelphia, 1915. Price $3.)

THOSE who use either the albino rat or the brown rat for the purposes of experimentation will find a mine of useful information in this volume by Mr. Donaldson. The author, whose own works on the subject and those of his collaborators are now widely known, has brought together all the information concerning these two varieties of *Mus norvegicus* that has been or is capable of being treated statistically. It is essentially a book of reference and as such is arranged in a very convenient way and has a full index. The bibliography of 52 pages is not only extremely valuable in itself, but gives an indication of the

thorough way in which the author has carried out his task and the extent of the ground that has been covered. A short introduction, note on classification, and account of "Early records and migrations of the common rat" are quite interesting to the general reader.

It is a book that should certainly be in all laboratories and in the hands of all who are working not only on the rat but on the general questions of the relations of different parts of the body in the adult and in the young animal.

C. H. O'D.

Diseases of Poultry: Their Etiology, Diagnosis, Treatment, and Prevention. By Raymond Pearl, Frank M. Surface, and Maynie R. Curtis. [Pp. xi + 342, with 72 illustrations.] (New York: The Macmillan Company, 1916. Price 8*s*. 6*d*. net.)

The recognition of the various diseases of poultry, together with their prevention and cure, is of special importance at the present time, when it is necessary to increase the available food supply. The present volume is therefore welcome, for it supplies an account of applied poultry hygiene, the commoner diseases of poultry, and treatments appropriate for the same. Twenty-one chapters are devoted to these topics, and a glossary, which unfortunately contains some rather poor definitions, is appended.

The book is frankly described as a compilation, but it is to be regretted that few sources of information, other than American publications, appear to have been consulted. The result is, that in several cases successful treatments are ascribed to workers who did not originate the treatments. Thus, the use of catechu for coccidiosis was set forth by Fantham and applied in England in 1910, but is ascribed to Salmon, who published it in 1913. Also, the account of spirochætosis in birds might have been brought more up-to-date. *Spirochæta gallinarum* and *S. marchouxi* are synonyms, but appear to be considered as separate organisms (see pp. 191, 230).

The chapter on poultry hygiene is very clear and concise. The sanitation of the poultry house, the problems of hygienic feeding, provision of runs, means of exercising birds, and the disposal of dead birds all receive adequate treatment. Much of the mortality among poultry could be prevented by attention to the principles laid down in this chapter. A short, practical *materia medica* for poultrymen is given in the fourth chapter, and should be of service. It contains an account of the principal drugs used, and the methods of their administration.

The various maladies affecting poultry are dealt with chiefly according to the organs of the host most concerned. Diseases of the liver, alimentary tract, respiratory, circulatory, nervous, excretory, and reproductive systems are discussed, while chapters are also devoted to the external and internal parasites occurring among birds. The various tumours of poultry and hints on simple surgical procedures conclude the book.

Probably the best chapter in the book is the one dealing with poultry hygiene, which has already been outlined. When dealing with diseases of the alimentary tract, an account is given of simple diarrhœa, enteritis or dysentery, constipation, indigestion, coccidiosis, peritonitis, and ascites. Poisoning due to salt, sodium nitrate, lyes, arsenic, zinc, lead, mercury, and ptomaines are discussed and suitable treatments are indicated. Diseases of the liver, fowl cholera, typhoid, plague, and tuberculosis are considered in some detail.

The chapter on the internal parasites contains an account of the commoner tapeworms of poultry, and of the best means of administration of medicaments for

exterminating them. The introduction of medicines into the crops of birds by means of a catheter is neat and practical. The round worms and flukes of the alimentary tract are also described. Maladies of the respiratory system, including catarrh, bronchitis, croup, influenza, the various forms of roup (which cause much loss), canker, aspergillosis, and the air-sac mite disease are all discussed. Some of the treatments for roup in use in England, such as that by hydrogen peroxide, however, are not noted.

Very practical hints on the prevention and treatment of diseases due to external parasites—bird-lice, mites, ticks, fleas and eye-worms—are given, and the chapter is well illustrated. The same applies to the chapter dealing with skin diseases. To those poultrymen concerned chiefly with egg production, the interesting accounts of the maladies of the reproductory organs will appeal. Other maladies and sundry deformations are noted, so that a comprehensive survey of the various affections of poultry is comprised in the book.

The book can be recommended as a very practical contribution to the well-being of the poultry industry.

F.

Memoirs of the Indian Museum. Vol. V. Fauna of the Chilka Lake. No. 2, October 1915. (Calcutta: Published by order of the Trustees of the Indian Museum, 1915. Price Rs. 3–8.)

THE *Memoirs of the Indian Museum* are now sufficiently well known to those interested in the Fauna of India. The fifth volume is devoted to a consideration of the Fauna of Lake Chilka. In this, the second part, the Mysidacea (Dr. Tattersall), the Mammals, Reptiles and Batrachians (Dr. Annandale), the Aquatic insects other than Coleoptera (Dr. Annandale and Mr. Kemp), the Odonata (Mr. Laidlaw), and the Stomatopoda (Mr. Kemp) are dealt with. One of the most interesting features of this part is the bionomics, the notes for which are furnished by Dr. Annandale and Mr. Kemp. Of the sections under review the non-professional reader will turn naturally to the account of the mammals, reptiles and batrachians. No new species are described in these groups, but there is much information about their habits and distribution within the lake area. The snakes represent an essentially estuarine element in the fauna of the lake. Of the crocodiles both the common crocodiles of India (*Crocodilus palustris*) and the "gharial" (*Gavialis gangeticus*) are present; the latter record, if correct, marks an extension of the previously recorded southern limit (the Mahanaddi river-system) of this species in the Bay of Bengal. On the whole the herpetological fauna of the lake is estuarine. Most of the species are of wide distribution and there is no endemic element. The fauna is Peninsular, as opposed to Indo-Gangetic. Apart from the Coleoptera, the aquatic insects of the lake include at least twenty species, the majority belonging to the Rhynchota. Only a very small minority of these insects can be regarded as anything but casual visitors; they are for the most part immigrants from fresh water and drift or fly into or on to the lake from neighbouring tanks or rice-fields. The chief factor influencing immigration is the periodic growth and decay of a species of Potamogeton that forms dense submerged thickets during the dry season in sheltered bays of the main area of the lake, dying down almost completely in the rains. The Stomatopoda of the lake are represented by two species and one variety of the genus *Squilla*.

There is an immense field for bionomic studies in India, and it is to be hoped that what Annandale has done to stimulate interest in this subject may lead to more extensive and intensive work not only in Lake Chilka but in other selected

localities in India. Much of this work might appear to the layman to be absolutely useless, judged from the severely practical standpoint, but a little reflection shows us that even apparently unpromising fields of research are worth undertaking solely from a utilitarian standpoint. Take, for instance, in the part under review, the group of aquatic insects. The Culicidæ include the mosquitos, which have such an important relation to public health in Bengal. In a recent report on the public health of that province we read: "Villages are built on the very borders of the lake, and though most of them have a few patches of rice cultivation, the vast perennial mosquito population comes from the lake itself. In situations sufficiently protected by weeds and algæ from the attacks of fish, anopheline larvæ and nymphs are in veritable swarms." Annandale and Kemp do not confirm this. They say that in spite of careful search in many localities the only mosquito larvæ they were able to find in the lake were were those of *Anopheles rossii*, and they conclude that most of the mosquitos breed in small pools of water near the edge rather than in the lake itself.

It is obvious that if prophylactic measures are based on insufficient or inaccurate evidence there is a risk of waste of public money. Consequently we are compelled to insist on the value of the field zoological work done by the Indian Mission staff under the able direction of Dr. Annandale and to hope that its sphere of usefulness will be considerably extended in the future.

J. T. JENKINS.

Memoirs of the Indian Museum. Vol. V. Fauna of the Chilka Lake. No. 3, December 1915. (Calcutta: Published by order of the Trustees of the Indian Museum, 1915. Price Rs. 9.)

THIS, the third part of the volume of the *Memoirs of the Indian Museum* devoted to Lake Chilka, is of less general interest than the preceding, since it is confined to a description of the Crustacea Decapoda inhabiting the lake area. Mr. Kemp, who is responsible for this group, is a worthy successor to Dr. Alcock, whose researches into the Indian Crustacea brought great credit to Indian zoology.

It must be obvious that a group such as this can only be adequately dealt with by a trained zoologist who has collected and made field observations in India. We would even go so far as to say that it is impossible to do the work properly in any other manner.

Mr. Kemp writes not only with the knowledge of the laboratory-trained zoologist, but with the advantage of a first-hand acquaintance of the habits of the creatures he is writing about.

The Crustacea Decapoda are of some economic interest since they form the basis of an important seasonal fishery by the Ooriyas who inhabit the borders of the lake. The fresh-water prawns (*Palæmon malcolmsii* and *P. rudis*) are trapped in large numbers in September when the water is fresh or very slightly brackish.

Generally speaking the Decapod lake fauna shows a much greater resemblance to that of the Madras backwaters than to the Gangetic delta. Of the brackish water prawns *Penæus indicus* is caught in large numbers by the Ooriya fishermen and it occurs at all seasons of the year both in the main area of the lake and in the outer channel communicating with the Bay of Bengal. These prawns are dried and exported to Burma.

There is, in Bengal generally, evidence of an annual migration of the prawns (Penæidæ) to the sea in the winter months, and this apparently coincides with

the commencement of the breeding season. Mr. Kemp finds no confirmation of this in Lake Chilka.

Certain species of amphibious crustacea, such as the Ocypod crabs, a new species of *Dotilla*, and *Cardiosoma*, seem to inhabit the banks of the outer channel during the whole year in spite of the great change in salinity; whereas others, such as *Gelasimus annulipes*, are only met with when the water in the channel is of the salinity of the open sea.

Fifty-four species of Decapoda are found in the lake, and these belong to thirty-eight genera. The Paguridea have already been described by Dr. Henderson in the *Records of the Indian Museum*. About 60 per cent. of the whole number of species exist through the full range of salinities. Some of the forms are well-known brackish water creatures. Of others some are marine types whose presence in fresh and brackish water is unexpected. Two species of *Caridinia* are the only fresh-water representatives that can be classed as permanent inhabitants.

J. T. J.

Memoirs of the Indian Museum. Vol. VI, No. 1. (Calcutta: Published by order of the Trustees of the Indian Museum, December 1915. Price Rs. 7-8.)

THE first part of the sixth volume of the *Memoirs of the Indian Museum* contains two important papers on widely different representatives of the fauna of India, namely one on the Tunicata in the collection of the Indian Museum by Dr. Oka of Tokyo, the other on Indian Oligochæta by Dr. Stephenson.

The Tunicate collection described by Dr. Oka, though a small one, is of quite exceptional interest since it contains five specimens of the very rare genus *Hexacrobylus*. This genus was constituted by Sluiter for a curious deep-sea Ascidian dredged by the Siboga expedition, which was so unlike ordinary Ascidians that it was impossible to guess its true nature until the only specimen was dissected. Dr. Oka gives a full description of this new species, *Hexacrobylus indicus*, which was dredged at a depth of 1912 fathoms south of Ceylon. A new genus *Monobotryllus* is constituted for a form which connects the families Styelidæ and Polystyelidæ.

Dr. Stephenson, whose work on the Oligochæta is well known, contributes a paper on two collections of Oligochæta, one made by Mr. Kemp in Ceylon, the other by Mr. Graveley in Cochin. There are also references to other Indian Oligochætes. Although the earth-worm fauna of India has been worked at to some purpose during the last few years, it is practically certain that much remains to be done. In the collections under review, of thirty-seven definitely named forms there are two new genera, twenty new species, and five varieties. The discovery of the first terrestrial species of *Pontodrilus* is noteworthy. Although this paper of Dr. Stephenson covers new ground, we fear that much more extensive collections are necessary before a volume in the *Fauna of India* can be devoted to the earthworms.

J. T. J.

The Involuntary Nervous System. By WALTER HOLBROOK GASKELL, M.A., M.D., F.R.S. [Pp. ix + 178, with 9 figures, 8 of which are coloured.] (London: Longmans, Green & Co., 1916. Price 6*s.* net.)

THIS book possesses a two-fold interest apart from its subject-matter. In the first place it is the first of a new series of "Monographs on Physiology" being

published under the editorship of Prof. E. H. Starling; secondly, by the sad fact that the death of the talented author occurred soon after the completion of the MS.

Walter Holbrook Gaskell was an eminent physiologist whose work was always marked by a wonderful breadth of view, by originality and by courage. His writings, which invariably aroused interest and have formed the stepping-off stones for many subsequent researches, will be much missed. Throughout a long life he has been working at many problems connected with the sympathetic or, as he prefers to call it, the involuntary nervous system, and the present work forms a fitting summary to that life's work. The book is an intensely interesting one from beginning to end.

We are in entire agreement with the author that the solution of physiological problems needs the co-operation of morphology to be satisfactory, and conversely morphology must call to its aid physiology. It is unfortunately true that nowadays it is extremely difficult to keep abreast with the enormous amount of work that is being done in these two great branches of knowledge, and the result is that one side or the other suffers. Indications of this are not lacking in the present volume. To take an example, one of the sub-headings of a chapter is "Motor nerves of muscles surrounding the segmental duct." Segmental duct is a definite term in embryology, and it is somewhat bewildering to the biologist with any acquaintance with vertebrate embryology to find that when the author uses this term he does not mean the same as the anatomist or embryologist. As a matter of fact it is about the one part of the uro-genital ducts he does not mean. Again, after describing in a general way the Pronephroi and their derivatives, he concludes when dealing with the metanephros: "The Wolffian body now takes no part in urinary excretion, only the generative gland remaining, whose duct is the Müllerian duct." This is a loose kind of statement. Why *only* the generative gland, as if it were either a part of the Wolffian body, or took part in urinary excretion? Furthermore, this applies to the female alone, for as is noted in another connection some lines lower down, in the male it is the Wolffian duct that is the genital duct. Apart from these matters and the drawing comparisons between vertebrates and Limulus or other arachnid, which are certainly not to be regarded as generally accepted, the book is a valuable contribution to our knowledge of the Involuntary Nervous System. Whether future research will endorse the conclusions set out—and it probably will many of them—is not so important as the bearing the work will have on future work along similar lines. It gives the reader a good account of our present knowledge of the problems with their difficulties and limitations in such a way that it is bound to stimulate interest in a fascinating branch of physiological and morphological research. The diagrams help to make clear some of the difficult points, and the whole book is clearly and concisely written.

C. H. O'D.

MEDICINE

Cerebro-Spinal Fever. By MICHAEL FOSTER, M.A., M.D., Capt. R.A.M.C. (T.F.), and J. F. GASKELL, M.A., M.D., Capt. R.A.M.C. (T.F.). [Pp. viii + 222.] (Cambridge: at the University Press, 1916. Price 12*s*. 6*d*. net.)

ALTHOUGH outbreaks of cerebro-spinal fever have occurred in recent times in Ireland and Scotland, the year 1915 first witnessed this disease on an epidemic scale in England. Physicians and pathologists have thus had the opportunity of studying it at first hand, and this admirable monograph is the fruit of such a study,

based on some forty military cases from the Eastern Command under the observation of the authors at Cambridge. So much has been learned during the last decade as to the nature and epidemiology of this once mysterious malady that there was real need for an authoritative work embodying recent researches, and the writers are to be congratulated upon the thorough way in which they have dealt with the subject, upon its literary presentation, and upon their admirable coloured illustrations.

After an historical summary, the clinical features of cerebro-spinal fever are fully and accurately described, and it is at once apparent that the descriptions are drawn from actual experience. The substantial identity of posterior basic meningitis in infants with the epidemic disease is assumed. Diagnosis occupies a whole chapter, special stress being laid on Kernig's sign and on the importance of lumbar puncture. In the chapter on treatment the authors pin their faith on persistent drainage of the spinal theca by lumbar puncture, a method of proved value which in their hands gave better results than any other. We think, however, that they underestimate the value of serum treatment, though it is true that every one found it disappointing in the 1915 epidemic. We know from American experience, and from the results obtained by Robb at Belfast with Flexner's serum, that a marked reduction in the death rate has at times followed its use by the intrathecal route. It would seem that we had not at our command in 1915 any serum sufficiently potent against the particular strains of meningococcus then prevalent—a defect now largely remedied, if we may judge from the improved results already recorded in 1916. The writers' unfavourable verdict may thus demand reconsideration.

It is perhaps invidious to point out any one chapter in the book as the best, but if we had to make such a choice we should name that on the pathology of the disease, on account of its admirable presentation of the structural arrangements of the meninges and the physiological effects which follow their disturbance by inflammatory changes. Of the two paths by which the meningococcus has been believed to gain access from the nasopharynx to the meninges, the authors are inclined to favour the direct route by the olfactory nerves as more probable than that by the blood. There is an excellent account of the cerebro-spinal fluid and its changes in epidemic meningitis.

The spread of the disease by the healthy carrier is accepted as the chief mechanism of an epidemic, and the whole carrier question is fully discussed in the chapter on epidemiology. Little is said, or can, indeed, be said, as to the conditions which determine the occurrence of the disease itself in the carrier.

The final chapter is devoted to bacteriology. It contains a clear account, not only of the meningococcus, but of the allied organisms with which it may be confounded. Cultural tests are fully considered, but we note the omission of any reference to the value of egg-medium: in sealed tubes of this medium it will live for at least three months—far longer than in the starch-agar stabs recommended by the authors.

In discussing the difficult question of the subspecies of the meningococcus the serological results at present available are duly considered, including those obtained by Gordon in last year's epidemic. The authors are wise in regarding this matter as at present unsettled, though we think they underestimate the value of agglutination tests in diagnosis.

The book is so free from misprints that it is only kind to mention the few which we have noted. On p. 92, line 21, "dura mater" is printed where "pia mater" is evidently intended. "Cornflower" is a misprint for "cornflour" on

p. 160, and on the same page the name of Hiss is spelt with one s. On p. 162 Ghon's name is twice misspelt.

We have already alluded to the excellence of the coloured illustrations, which exhibit the cutaneous rashes, the morbid anatomy, the histology and the bacteriology of cerebro-spinal fever. A full bibliography and a good index are added, completing a volume which should prove of great value for many years.

F. W. ANDREWES.

An Inquiry into the Statistics of Deaths from Violence and Unnatural Causes in the United Kingdom. By WILLIAM A. BREND, M.A., M.D., B.Sc. [Pp. 80.] (London: C. Griffin & Co., 1915. Price 3*s*. 6*d*. net.)

THE common saying that there are three kinds of lies—ordinary lies, damned lies, and statistics—is unfortunately a witticism which contains a large amount of truth. This is, however, no reflection on the statistician, whose services are not usually employed in recording the data. The erratic way in which statistics are supplied is well illustrated by the author of the work under review. There is nothing in the whole work—in fact, there could be nothing, with the returns at the disposal of the author—to enable the reader to say whether or no we had attained or were approaching the irreducible minimum of deaths from violence. There are indications, but indications only, that the number of deaths from unnatural causes are far in excess of what we might reasonably hope, with average care, to reduce very considerably.

The chief aim of the work would seem to be to show the chaotic state of our records. The records of unnatural deaths given in the Annual Report of the Registrar-General differ from those supplied by the Local Government Board, and also from those supplied by the coroners' returns; the difference being probably due to the various systems of classification employed. There seems to have been no attempt to bring uniformity into the methods of recording, even when such uniformity could easily be attained, such as the methods of geographical distribution, and arrangement in the same age groups.

The classification of the cause of death and the difficulties of nomenclature might certainly be considered more difficult problems; but a useful working basis might be arrived at if medical men would only consider the possibilities of statistics more fully. They would see that, while a medical practitioner might be justified in considering his own method preferable to those of his confrères, a persistence in his own classification and refusal to join in the general method, while not perhaps reflecting on his ability as a doctor, does certainly point to his incompetence as a Public Health officer.

It is curious to hear that officials, such as coroners, should ever consider their returns as their own personal property; and although they may not refuse information to those requiring it, the fact that they have the power to refuse, and are not obliged to send their records in a prescribed form, is a scandal. The figures given in the book are the records for the year 1912, taken from the sources mentioned in the introduction; but there is no indication as to whether they are larger or smaller than the average for the last few years, obtained from the *same* sources.

The author clearly points out the unreliability of the data, but does not help us to estimate the amount of that unreliability. As to whether the figures are 5, 50, or even 500 per cent. out, is left to our instinct. Now on purpose that statistics should be of value it is necessary to consider the following points: (*a*) the reliability of the sources; (*b*) their completeness or incompleteness; (*c*) the manner of grouping with reference to the taking of an average, or for correlation. Associated

with each of these points it is possible to obtain certain numerical measures by what is called the "Method of Statistics." These measures give some idea of the value to be attributed to the figures, and without these measures no argument based on them can be of any value. Dr. Brend has not attempted to give such measures. He has shown that the figures are unreliable and incomplete, and gives useful suggestions for improvement which might well be considered by those occupied in recording.

W. STOTT.

ENGINEERING

Specification and Design of Electrical Machinery. By Prof. MILES WALKER, M.I.E.E., etc. [Pp. xix + 648, with illustrations.] (London : Longmans, Green, 1915. Price 32*s*. net.)

THE chief interest of this book to the non-technical person lies in the evidence it gives of the progress that has been made in systematising the design of all kinds of electromagnetic machinery. The electrical designer, though he has been the last to come into the field, has always been the most scientific of all designing engineers. The criticism that has been levelled at him by engineers of an older school is that he is too scientific and that he does not pay enough attention to the practical conditions under which the machinery he designs has to operate. Though this criticism was possibly a just one as regards many designs of twenty years ago it is not a just one to-day, and this book is sufficient evidence that the designer of electrical machinery is now as alive to practical difficulties as any other member of his profession.

The early chapters of the book deal with the general question of electrical design, and Prof. Walker follows the method devised originally by Mr. B. G. Lamne in all the machines with which this book deals. The method is one of great interest, and it is thoroughly scientific because it is based on what Dr. Thompson has called the "specific utilisation of material."

The essential things in any electromagnetic machine are, firstly, the magnetic field in which the conductors which produce the electrical energy move, and secondly, the conductors which carry the current. The strength of the magnetic field depends on the density of magnetic flux that can be used in the iron; this flux passes through the iron circuit of the machine and across the air gap to the copper-carrying part of the machine which supports the conductors. The total flux passing across this air gap backwards and forwards is called the magnetic loading of the machine, and is evidently a quality depending principally on the qualities of the material of which the frame of the machine is built up and on its size. The power which the machine will develop, if it is a dynamo, depends on the voltage which the machine produces and on the current which it will carry. The voltage produced depends on the number of conductors which are cutting the magnetic flux, and the current which the machine gives will depend ultimately on the copper section of the conductors. The second factor in the design is thus arrived at, *i.e.* the current loading, which is the product of the current per conductor and the number of conductors carrying that current. The design of any machine is thus reduced to a consideration of these two factors, and the ultimate dimensions are determined by the limits of magnetic loading and current loading that are possible.

The first half of the book deals with the subject of design from this point of view. It deals with the magnetic qualities of the materials used in dynamo manufacture, and with the practical limits of flux density which can be employed. It

deals also with the current density that can be used in the conductors, and with the limits of heating that can safely be employed without injuring the materials which are used for insulation. This discussion necessarily involves the consideration of ventilation and cooling (a subject on which Prof. Walker himself has done a great deal of original work), and the chapters dealing with it are of great interest and value.

In the second half of the book the author deals with sixteen specifications of electrical machines, including alternators, both high and low speed, induction motors, rotary converters, direct current generators, boosters, and phase advancers. The method used is first to draw up a model specification for some prescribed running conditions, and then to work out from the manufacturers' standpoint the complete design. A detailed design is given, together with calculation sheets from which all dimensions can be obtained, with the least possible delay.

In the preface he has written to the book the author states he has had in mind as a model the *Conveyancing Precedents* of Prideaux, which are used so much by lawyers. He has endeavoured to produce a book which will take an analogous position in electrical engineering. Whatever the average engineer may think of Prof. Walker's precedent, and of his attempt to copy in a productive science the methods used in a profession essentially parasitic, one cannot but be impressed by the value of what he has done. The labour of collecting the material for this volume, and of drawing up and working out the various designs, is no light task for a designer engaged on practical work; that it has been done by such a man will add enormously to its value.

The latter part of the book is not confined, however, to design; it includes a complete and careful abstract of the work that has been published on such subjects as tooth ripples, besides covering a good deal of the theory of the parallel running of alternators, commutation in direct current machinery, and the starting of rotary converters and induction motors. The book is not a mere designer's handbook, it approaches more nearly a treatise on electrical machinery, and as such it will take its place on the shelves of all electrical engineers who wish to possess in a convenient form a comprehensive survey of electrical machine design.

The Telephone and Telephone Exchanges. Their Invention and Development. By J. E. KINGSBURY, M.I.E.E. [Pp. x + 558, with 170 figures.] (London: Longmans, Green & Co., 1915. Price 12*s*. 6*d*. net.)

THE early chapters of this book, in which there is presented in a most complete and interesting form the history of the telephone, provide a story which is without parallel in any other branch of applied science. The name of Alexander Graham Bell will always be associated with the telephone, and it is interesting to find that heredity once more has shown its influence, for both Graham Bell's father and grandfather were professors of elocution, and studied the science of word production at first hand. It is to Graham Bell's father that the invention of "visible speech" is due, and a most interesting account of the testing of this theory is given in the chapter on "The Spoken Word." Melville Bell taught his two sons Edward and Alexander Graham his sound alphabet, and "wrote down the queer and purposely exaggerated pronunciations and mis-pronunciations" of his friend Mr. Alexander Ellis, F.R.S., who describes the experiment "in such a manner that his sons, not having heard them, so uttered them as to surprise me by the extremely correct echo of my own voice."

Unlike many great inventions, the discovery of the possibility of conveying speech by electrical currents was the result of careful scientific study. In his early telephones Bell started with the idea of analysing sounds into a number of component parts, transmitting the components and recombining them, when they were received, into the spoken word. He regarded as one of his greatest discoveries the fact that one of the reeds in his "harp" transmitter produced a current of sufficient power to excite vibrations in another "harp" which had a reed tuned to the same note as that which produced the original current. The substitution of a membrane for the reeds was the next step, and led to the first practical telephone, through which he and his assistant, Mr. Watson, shouted to each other. Such is the story, in brief, told in the first chapters of Mr. Kingsbury's book. It is one of the most fascinating stories that is to be found in the whole history of discovery and invention, a story which brings out most clearly the genius of Bell and his extraordinary acumen and power of observation.

Next follows an account of the inception of the telephone exchange, which began with the application to telephonic purposes of the district telegraph system installed originally as a protection against burglary at Brooklyn Heights, New Jersey. Though this was the first practical telephone exchange, the idea of an exchange system had been put forward by Dumont of Paris in 1851. This story also is one of the greatest interest.

The following chapters describe another epoch in telephone development: the discovery of the microphone by Hughes, a musician by training, who "though of Welsh parentage, was born in London and spent the greatest part of his life in America, acting first as a professor of music and later occupying the chair of Natural Philosophy in the College of Bairdstown, Kentucky." The merit of Hughes's discovery, as stated by Lord Kelvin, mainly consists in this: "That variation in electrical resistance by pressure is a property of the contact of two conductors and that it is not confined to any one but to all conductors." Edison had discovered two years before "that carbon of various forms when moulded into buttons decreased the resistance of the passage of the electrical current by pressure." There was great controversy as to the priority of invention of the transmitter between Hughes and Edison, and the controversy ended in the law courts. The work of other discoverers is clearly described in the chapter on the microphone.

The work of Reis is given in another chapter; one interesting sentence deserves quotation: "Scientists learned with expressed surprise many years later" (*i.e.* after the account of Reis's inventions) "and after the electrical transmission of speech had been effected by other means, that variations in contact pressure would serve to transmit articular speech." Professional scientists are apt sometimes to regard themselves as the chief medium of scientific advance. This is a just conclusion in many cases; but it requires the natural genius of a discoverer or inventor to make great strides, and as often as not the natural genius arises outside the pale of the professional scientists. Summing up the results of Reis's discoveries, Mr. Kingsbury says:

> "Whether the Reis instrument ever conveyed from talker to listener a sound which could accurately be called speech can never with certainty be known. The probabilities against it are so great that overwhelming evidence is necessary to support the contention that speech was transmitted, and the contemporary evidence does not indicate more than that sounds suggestive of words were sent."

The remainder of the book will be of greater interest to the telephone engineer than to the man of science, though there is much in it that will interest every one.

It is remarkable to read the words of Sir William Preece in 1879 when asked by Lord Lindsay as member of a House of Commons Committee "whether he considered that the telephone will be an instrument of the future which will be largely adopted by the public?" "I think not," was the reply. Of how many of the great discoveries in science has this been said in England!

It would be impossible in a short review to do justice to this book. It is a monument to the industry and thoroughness of one who is a recognised authority in his subject. It is a book which should find its place in the library of every scientific man, though it contains much that will prove of absorbing interest to those who make no claim to that description, for it is the history of the development (perhaps the most marvellous that the world has ever seen) of a great idea, which has done more than any other to revolutionise the methods of human intercourse.

Land and Marine Diesel Engines. By GIORGIO SUPINO. Translated by ENG.-LIEUT.-COMMANDER A. G. BREMNER, R.N., and JAMES RICHARDSON, B Sc. (Eng.) Lond., A.M.Inst.C.E. [Pp. xvi + 310, with 19 plates and 380 illustrations.] (London: Charles Griffin & Co., 1915. Price 12*s.* 6*d.* net.)

THIS volume on the Diesel Engine is a translation of an Italian work. The translators in their Preface write of the author as follows: ". . . an Italian engineer of high repute, who died ere yet he had had time to enjoy the reputation he had won, made a special study, not only of the theory, but of the construction and running of oil engines, and such merits as this book possesses as a translation are due entirely to his engineering genius, erudition, and lucidity of exposition. The translators have sought only to interpret his ideas. The English edition, it is hoped, will be accepted as much for its recognition of Ing. Supino's work as for the stimulus and assistance it may bring to British workers. . . ."

We join with the translators in their eulogy of the author and his book. It is evidently the work of an engineer who has had the widest scientific and technical training, and has then specialised in one particular branch of engineering. The very arrangement of the book can be taken—now that this country is talking about reconstructing its educational and commercial methods—as an object-lesson to engineers and authors alike. It is divided into two parts. Part I. contains six chapters as follows: I. Diesel Engines for Stationary Plants. II. Marine Diesel Engines. III. Fuels for Diesel Engines, Heavy Oil Explosion Engines, and Constant-Pressure or Diesel Engines. IV. Thermo-dynamic Cycles. V. Efficiencies. VI. The Calculation of Cylinder Dimensions. Part II. discusses the more technical detail from Chapter VII. on "Bed Plates, Crank Cases, Engine Framing, Cylinders," to Chapter XVI. on "Tuning-up, Test Bench, and Acceptance Trials of Diesel Engines."

Thus the engineer who completed his training some years ago but now finds he has to face the Diesel engine problem—and there are many such, particularly, perhaps, in the Royal Navy—can master the essential parts in the first 73 pages of the book. And these essentials are regulated in suitable steps: first, what the Diesel engine is and how it works; secondly, the thermo-dynamical considerations; and thirdly, the calculation of the most essential part in any reciprocating engine, the diameter of the cylinder, and stroke of the engine—*i.e.* cylinder dimensions. Part II. can be used as desired. The engineer whose work is either the design or construction will need to study every page in detail. The engineer

who has to supervise the running of the Diesel engines will need to read certain portions of Part II., but the remaining portions can be referred to as occasion demands.

It is, however, interesting to note that the author expects all his readers to have been properly trained in the fundamental portions of an engineer's education. Taking random examples, we are told in Part I. (p. 60) that "to examine with accuracy the thermo-dynamic values of the various cycles, recourse must be had to the entropy temperature diagrams. . . . The entropy of the gas is—

$$E = \int \frac{d\phi}{T} = c_v \cdot \log_e pv^k + \text{const."}$$

while on p. 62, fig. 64 shows the graphic conversion of curve $T = f(v)$ into curve $T = f(E)$."

Further, in Part II. in the chapter on "Valve Actuating Gears," fig. 222*a* indicates a graphic method of drawing a polarsinoid cam where—

$$R_x = \left(r + \frac{h}{2}\right) - \left(\frac{h}{2} \cos \frac{2\pi}{a} \theta\right).$$

While " in fig. 223, the curve of valve movement is drawn with the abscissæ representing time and the ordinates the lifts, h, of the valve. From this curve another representing $\frac{dh}{dt}$ may be drawn, in which the ordinates represent the velocity v at any instant, and thirdly $\frac{dv}{dt}$, the curve of accelerations, a, may be obtained."

In a volume where each and every portion of the engine itself has been very thoroughly treated, both in the text and by illustrations, it may seem a little out of place to ask for more. But we feel the value of the book would be greatly enhanced by a chapter on accidents to Diesel engines. They *do* occur, and are generally serious. For instance, investigations into the causes of explosions have been published, but, in general, the reasons given have not inspired confidence that they were the true reasons. Again, troubles with the air compressors are fairly common, but we see no reference to this or to the very great care that is necessary in the lubrication of that portion of the plant and in the selection of the lubricating oil.

The translators could easily add a few pages on these points in the next edition. We have no hesitation in very strongly recommending this book as an excellent contribution to the science and practice of this most important branch of engineering.

J. WEMYSS ANDERSON.

Diesel Engines for Land and Marine Work. By A. P. CHALKLEY, B.Sc. (Lond.), A.M.Inst.C.E., A.I.E.E. [Pp. xviii + 368, with 172 figures.] (London: Constable & Co., Ltd., 1915. Price 8*s*. 6*d*. net.)

THIS is the fourth edition of this work, and indicates very clearly that the author has a good knowledge of the possibilities of the majority of English "technically trained" engineers.

The opening chapter is on the "General Theory of Heat Engines, with special reference to Diesel Engines." If this theory, as given, is sufficient for the author's purpose, then surely an Appendix should be added in which the term "entropy"

might at least be introduced, and the word "elementary" substituted for "general" in the title of the first chapter. But why not put the *complete* theory wholly into an Appendix, and start with the present Chapter II—"Action and Working of the Diesel Engine"?

Taking another important section, "Valves and Cams," we find the subject-matter wholly descriptive. It is as if an author on "Mechanics" was content to dismiss the common or garden pump with—

H is the handle,
S is the spout,
You heave up the handle,
And the water comes out.

It cannot be disputed that an engineer without a previous knowledge of the Diesel engine will have some idea of "Valves and Cams," but what will be the value, the depth, of his knowledge? Exactly on a par with the student's knowledge of the pump as described above.

As a record of the design and construction of the Diesel engine as it stood in 1915 the work is of distinct merit, and the many fine drawings will be of great value to designers and constructors alike. It is the very merit in this direction that makes the opening chapter grotesque.

J. Wemyss Anderson.

GENERAL

The Dramas and Dramatic Dances of Non-European Races in Special Reference to the Origin of Greek Tragedy, with an Appendix on the Origin of Greek Comedy. By William Ridgeway, Sc.D., F.B.A. [Pp. xv + 448, with 92 illustrations.] (Cambridge: at the University Press, 1915. Price 15*s.* net.)

It will be remembered that three or four years ago Prof. Ridgeway published a book on *The Origin of Tragedy*, in which he advanced the theory that Tragedy originated not in the worship of Dionysus, as had been hitherto supposed, but in the rites in honour of deceased "heroes." This new theory was subjected at the time to severe criticism by scholars who adhered to the older opinion. The present work is Prof. Ridgway's reply to his critics, being in fact, as he explains, an expansion of a short chapter in *The Origin of Tragedy*, in which he had briefly given "some evidence from the dramatic performances of Asiatic countries in support of his doctrine of the origin of Greek Tragedy." The body of the work is preceded by an introduction, defending his view and taking the offensive against the theory, started by Mannhardt half a century ago and made familiar to British readers by *The Golden Bough*, attributing the periodic celebrations in Europe and elsewhere to the observances of the agricultural seasons, the worship of trees, and of a more generalised "Vegetation-Daemon." After this preliminary reconnaissance in force, the distinguished author begins his serious attack by a lengthy but interesting account of the Passion Play of Hassan and Hussein, still performed by the Shiah Mohammedans of Persia and India. He goes on to argue that Adonis, Attis, and Osiris had, before entering on godhead, been as real human beings as Muhammad, Ali, Fatima, Hassan and Hussein, and that the quasi-dramatic representations periodically given in their honour were performances having reference to the adventures and sorrows of their earthly life. He turns to India and argues in like manner concerning Rama and Krishna, describing the Hindu drama and elaborately discussing its origin and development. After dealing with

Burmah and China, he devotes a long section to Japan, urging that the indigenous religion, called Shinto, was chiefly a worship of the dead, and "that the *No*, or serious drama, is the lineal descendant of the ritual which from remote ages had been used in the Shinto temples to propitiate the spirits of the dead, in the hope that they would vouchsafe all kinds of blessings to their descendants and worshippers, and especially that of abundant harvests." He concludes by reviewing the dramatic dances of other parts of the world, where, it need hardly be said, he thinks that evidence to a similar effect is to be found.

There is no room here to consider, as it deserves to be considered, a theory so far-reaching and revolutionary. Prof. Ridgeway's thesis, as originally enunciated in *The Origin of Tragedy*, was at least arguable: possibly it was sound. But he seems to have been stirred by criticism into going greater lengths than he intended, or than were at all necessary to support his argument. For if not asserted in so many words, it is implied that *all* dramatic performances grew out of rites at the graves of deceased chieftains and heroes, and that *all* gods are deceased human beings. It is true that the author does not commit himself in words to universal euhemerism, but, in asserting the universality of ancestor-worship, he assigns the cult of a dead man or woman as the origin of one great god after another, he resolves nature-spirits into deified human beings; and, forgetting how impossible it is for the human mind to overleap the boundaries of anthropomorphism, he continually interprets anthropomorphism as a proof that the divinity is regarded as having been literally once a man. We ask at length in bewilderment, Where then are the gods that were not once men, and to what origin would Prof. Ridgeway refer them?

Happily we need not accept his theories to value his book. It is written with all his verve and directness. Its impeachment of "the solar and vegetation theories of Kuhn, Max Müller, Mannhardt, and Sir James Frazer" is vigorous and effective, though the solar theory of Kuhn and Max Müller was already as dead as Antinous. By its masterly presentation of evidence it compels opponents to review and restate their own position. Thus it will be of material use in reaching definite results, which the present writer believes will not be those arrived at by the author, at all events in the extreme form here advocated. Meanwhile it is a delight to read; and the numerous illustrations from photographs and other sources enliven its pages, and many of them help to render the descriptions of dances and dramatic performances vivid and comprehensible.

E. Sidney Hartland.

Ancient Astronomy in Egypt and its Significance. By Frederick J. Dick, Professor of Astronomy and Mathematics, School of Antiquity, Point Loma, California. Point Loma: The Aryan Theosophical Press, January 1916.

Modern Astrology. Edited by Alan Leo. Published at *Modern Astrology* Offices, Imperial Buildings, Ludgate Circus, E.C. Price *6d.* monthly.

These two pamphlets furnish melancholy evidence of the credulity still prevailing among civilised mankind. The first appears to be issued in the interests of the superstition known as Theosophy. Although characteristically vague in style and purpose, its main thesis exhibits a desire to discredit modern astronomical science, and to suggest that true knowledge is to be found in the works of the ancient Hindus and Egyptians. The observations of the Hindus, we are told, extended over 850,000 years. By statements such as this, the doctrine of the pamphlet is maintained.

It is, indeed, not easy to follow the author's meaning. Like all works of "theosophy" or "occult science," vagueness is the chief characteristic. Clearness and lucidity are as fatal to mysticism as oxygen is to the microbe of tetanus.

Although we may regret the attitude adopted by Mr. Dick in this pamphlet on Ancient Astronomy, we may yet admit that he has taken the trouble to acquire some information on the subject of which be endeavours to treat. No such extenuating circumstance can be pleaded in favour of the editor of the second pamphlet on *Modern Astrology*; for that periodical (as it appears to be) contains no information and displays no knowledge of any kind whatever. The advertisement pages are filled with notices of publications on Astrology which may be purchased by the public for varying prices. Nearly all appear to be written by the editor, who is also prepared to give a course of lessons in Astrology for the sum of 10 guineas. One advertisement naïvely attempts to define the difference between an astrologer and a charlatan. To which category the editor of *Modern Astrology* belongs we are not informed. But to all who value civilisation and truth, it is a scandalous and depressing circumstance that there exist apparently a number of people in this country who are prepared to part with their money for the purchase of such literary trash, as is here presented, for the consumption of the weaker-minded of our compatriots. If any analogy is justifiable, it can only be with those vendors of patent remedies who fill an eight-ounce bottle with water from the tap, and sell it to the public for a guinea as a cure for cancer, tuberculosis, measles, and all other diseases great or small. But tap-water at least is harmless as a rule; whereas a perusal of these publications may well engender, among hysterical women and others, a form of psychological measles which causes crooked and unhealthy vision, and is a curse both to themselves and their associates. The effrontery with which such doctrines are set forth is shown by the fact that the editors of *Modern Astrology* actually venture to send a review copy to SCIENCE PROGRESS.

HUGH ELLIOT.

Changes in the Food Supply and their Relation to Nutrition. By PROF. L. B. MENDEL, Professor of Physiological Chemistry, Yale University. [Pp. 61.] (New Haven: Yale University Press; London: Oxford University Press, 1916. Price 50 cents.)

PROF. MENDEL reprints an essay dealing with the greater facilities afforded by improved transportation, for wider variations of diet, which will meet physiological necessities. Year by year enormous importations of foodstuffs from sparsely populated regions where they are raised, to the great centres of population where they are consumed, have been steadily growing, with the result that a solution is found of the problem of fully and completely supplying the physiological needs of those large populations. Freezing, chilling, drying, as well as methods of preservation by heat, have contributed to this end, and immense quantities of readily perishable foodstuffs, such as milk, fish, eggs, etc., are transported in this way. Not only that, but fresh fruit, notably bananas, are brought long distances in suitably fitted vessels in a very high state of fitness for consumption. The author alludes to the exaggerated views as to the necessities for meat diets.

The present European War affords special opportunities to study the relationship of the food supplies in unexpected economic and territorial conditions. Prof. Mendel points out that the household is the ultimate agency in the distribution of economic wealth to individuals. What the wage earner really secures, and what

the wife and children secure, depend upon the efficiency with which the household turns the wage income into economic good. The home can become responsible for malnutrition and insanitary living by improper preparation and waste of food. The little book is interesting and instructive.

E. W HOPE.

Microscopy of Vegetable Foods, with Special Reference to the Detection of Adulteration and the Diagnosis of Mixtures. By DRS. ANDREW L. WINTON, JOSEF MOELLER, and KATE WINTON. [Second Edition. Pp. xiv + 701, with illustrations.] (New York: John Wiley & Sons; London: Chapman & Hall, Ltd. Price 27*s*. 6*d*. net.)

THE importance of food-microscopy is fully recognised, and the second edition of the comprehensive work on the subject by the present authors is evidence of the value of their previous work. The book is intended primarily for the student and worker, as well as the public analyst. The account of the method of preparing the materials for examination, and the description of the chief histological elements will be useful, but the volume presupposes some preliminary training in botanical science. The microscopical characters of the principal vegetable foodstuffs, as well as those of every common adulterant or impurity, are given in full detail, which makes their recognition easy. The excellent character of the drawings throughout the volume should further assist in countering and limiting the scope of the perverse ingenuity of the adulterant. The volume should be in the hands of every public analyst.

E. W. HOPE.

Catalogue of the Serial Publications possessed by the Geological Commission of Cape Colony, the Royal Observatory, the Royal Society of South Africa, the South African Association for the Advancement of Science, the South African Museum, and the South African Public Library. With an Appendix containing a List of the Serials in the Bolus Herbarium of the South African College. [Pp. 54.] (Published by the Trustees of the South African Public Library, Capetown, 1912.)

THE printed work of South African geologists, and no doubt that of specialists in other branches of science, frequently contains the plaint that they are hampered by the lack of literature or by its inaccessibility. A partial remedy for this defect is attempted by the publication of this catalogue, which lists scientific and other periodicals in the possession of several South African public bodies. The list is in alphabetical order, place names are emphasised by italic type, and the volumes in possession of the six public bodies are noted in six parallel columns following the name of the periodical. The letters *inc.*, denoting "incomplete," occur with distressing frequency throughout the list. In geology, for example, even the Transactions of the Geological Society of South Africa are incomplete, not to speak of other well-known periodicals. It is to be hoped that all interested in South African scientific work will note the appeal made at the end of the catalogue, and assist in filling up the numerous gaps.

G. W. T.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

A Book of Homage to Shakespeare, 1916. Edited by Israel Gollancz, Litt.D., F.B.A., Honorary Secretary of the Shakespeare Tercentenary Committee. Humphrey Milford, Oxford University Press. (Pp. xxvi + 553.) Price 21*s*. net.

We regret that we have no space in this number to publish an Essay-Review upon this magnificent tribute to Shakespeare (due to Professor Gollancz), which, though it is rather a literary than a scientific work, will still appeal to all men of science. Another book which will make the same appeal is the one mentioned below; and we propose to review both in our next number.

Sonnets and Poems. By John Masefield. Published by John Masefield at Lollingdon, Cholsey, Berkshire. Printed at Letchworth by the Garden City Press Ltd., 1916. (Pp. 52.) Price 3*s*. 6*d*.

Fundamental Sources of Efficiency. By Fletcher Durell, Ph.D., Head of the Mathematical Department of the Lawrenceville School. London and Philadelphia: J. B. Lippincott Company, 1914. (Pp. 368.) Price 10*s*. 6*d*. net.

Guida allo Studio della Storia delle Matematiche di Gino Loria, Professore della Università di Genova. Ulrico Hoepli, Editore Libraio della Real Casa. Milano, 1916. (Pp. xvi + 227.) Price 3 lira.

Combinatory Analysis. By Major Percy A. MacMahon, F.R.S., D.Sc., LL.D. (late Royal Artillery), of St. John's College, Cambridge. Vol. II. Cambridge: at the University Press, 1916. (Pp. xix + 340.) Price 18*s*. net.

Fermat's Last Theorem. Rigid Proof of Elementary Algebra, also Dissertation on Test for Primes and Recurring Decimals. By M. Cashmore. London: G. Bell & Sons, Ltd., 1916. (Pp. 63.) Price 2*s*. net.

Exercices et Leçons de Mécanique Analytique. Par R. de Montessus, Professeur à la Faculté Libre des Sciences de Lille. Centres de Gravité, Attraction Potentiel, Moments d'Inertie, Dynamique des Corps Solides et des Systèmes. Les Fonctions Elliptiques dans le Domaine Réel. Paris: Gauthiers-Villars et Cie., Editeurs, Libraires du Bureau des Longitudes, de l'École Polytechnique, Quai des Grands-Augustins 55, 1915. (Pp. ii + 334.)

The Emission of Electricity from Hot Bodies. By O. W. Richardson, F.R.S., Wheatstone Professor of Physics, King's College, London. With Diagrams. Monographs on Physics, Edited by Sir J. J. Thomson, O.M., F.R.S., and Frank Horton, Sc.D. London: Longmans, Green & Co., 39 Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. vii + 304.) Price 9*s*. net.

Hyperacoustics. By John L. Dunk. Division I., Simultaneous Tonality. London: J. M. Dent & Sons, Ltd. New York: E. P. Dutton & Co., 1916. (Pp. vi + 311.) Price 7*s*. 6*d*. net.

A Treatise on the Theory of Alternating Currents. By Alexander Russell, M.A., D.Sc., M.I.E.E., Principal of Faraday House, London, Vice-President of the Institution of Electrical Engineers. Vol. II. Second Edition, with 239 figures in the Text. Cambridge Physical Series. Cambridge: at the University Press, 1916. (Pp. xiv + 566.) Price 15*s.* net.

Analytical Chemistry. Based on the German Text of F. P. Treadwell, Ph.D., Professor of Analytical Chemistry at the Polytechnic Institute of Zurich. Translated and Revised by William T. Hall, S.B., Assistant Professor of Analytical Chemistry, Massachusetts Institute of Technology. Vol. I. Qualitative Analysis. Fourth English after the eighth German Edition. New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1916. (Pp. xiii + 538.) Price 12*s.* 6*d.* net.

A Method for the Identification of Pure Organic Compounds, by a Systematic Analytical Procedure Based on Physical Properties and Chemical Reactions. Vol. II. Containing Classified Description of about 4,000 of the more important compounds of Carbon with the elements Nitrogen, Hydrogen, and Oxygen. By Samuel Parsons Mulliken, Ph.D., Associate Professor of Organic Chemical Research at the Massachusetts Institute of Technology, Boston, Mass. New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1916. (Pp. ix + 327.) Price 21*s.* net.

On the Control of River Channels. By T. S. Ellis. Proc. Cotteswold Nat. F. C., vol. xix. pt. 1, 1915. (Pp. 29 + 48.)

The Mechanistic Conception of Life. Biological Essays by Jacques Loeb, M.D., Ph.D., Sc.D., Member of the Rockefeller Institute for Medical Research. The University of Chicago Press, Chicago, Illinois. (Agents, Cambridge University Press, London.) (Pp. 232.) Price 6*s.* net.

The Story of a Hare. By J. C. Tregarthen, F.Z.S. With 8 Illustrations. London: John Murray, Albemarle Street, W., 1915. (Pp. 195.) Price 2*s.* 6*d.* net.

The Life Story of an Otter. By J. C. Tregarthen, F.Z.S. With 8 Illustrations. London: John Murray, Albemarle Street, W., 1915. (Pp. xiii + 188.) Price 2*s.* 6*d.* net.

The Contingency of the Laws of Nature. By Émile Boutroux, Member of the Académie Française. Authorised Translation by Fred Rothwell. Chicago and London: The Open Court Publishing Co., 1916. (Pp. vii + 196.) Price 5*s.* net.

The Problems of Physiological and Pathological Chemistry of Metabolism for Students, Physicians, Biologists, and Chemists. By Dr. Otto von Fürth, Professor Extraordinary of Applied Medical Chemistry in the University of Vienna. Authorised Translation by Allen J. Smith, Professor of Pathology and of Comparative Pathology in the University of Pennsylvania. Philadelphia and London: J. B. Lippincott Company. (Pp. xv + 667.) Price $6.

Natural History of Hawaii. Being an Account of the Hawaiian People, the Geology and Geography of the Islands, and the Native and Introduced Plants and Animals of the Group. By William Alanson Bryan, B.Sc., Professor of Zoology and Geology in the College of Hawaii, Fellow of the American Association for the Advancement of Science; Member, the American Ornithologists' Union; National Geographic Society; American Fisheries Society; Hawaiian Historical Society; Hawaiian Entomological Society;

American Museums Association; National Audubon Society; seven years Curator of Ornithology in the Bishop Museum, etc. Book I. The People, the Islands, and the Plant Life of the Group. Illustrated with 117 full-page plates from 441 photographs elucidating the ethnology of the native people, the geology and topography of the Islands, and figuring more than one thousand of the common or interesting species of plants and animals to be found in the native and introduced fauna and flora of Hawaii. Honolulu, Hawaii: the Hawaiian Gazette Co., Ltd., 1915. (Pp. 516.)

Reptilia and Batrachia. By George A. Boulenger, D.Sc., Ph.D., F.R.S. A Vertebrate Fauna of the Malay Peninsula from the Isthmus of Kra to Singapore including the Adjacent Islands. Published under the Authority of the Government of the Federated Malay States. Edited by H. C. Robinson, C.M.Z.S., Director of Museums, Federated Malay States. With 79 Figures in the Text. London: Taylor & Francis, Red Lion Court, Fleet Street. Kuala Lumpur: Federated Malay States Government Press. Singapore: Kelly & Walsh, Ltd., 1912. (Pp. xiii + 294.)

Growth in Length. Embryological Essays. By Richard Assheton, M.A., Sc.D., F.R.S., Trinity College, Cambridge, Lecturer in Biology at Guy's Hospital in the University of London, Lecturer in Embryology at the Imperial College, Lecturer in Animal Embryology in the University of Cambridge. With 42 illustrations. Cambridge: at the University Press. (Pp. xi + 104.) Price 2*s*. 6*d*. net.

Memoirs of the Indian Museum. Vol. V. Fauna of the Chilka Lake, No. 4. July, 1916. With 16 plates. Calcutta: Published by Order of the Trustees of the Indian Museum, Printed at the Baptist Mission Press, 1916. (Pp. 327–439.) Price 8 rupees, 8 annas.

Records of the Indian Museum. Vol. XII. Part III., May 1916. With 18 Plates, (Pp. 101–128.) Price 2 rupees.

Rambles of a Canadian Naturalist. By S. T. Wood. Colour Illustrations by Robert Holmes; Decorative Headings by Students of the Ontario College of Art. London and Toronto: J. M. Dent & Sons. (Pp. vi + 247.) Price 6*s*. net.

A Bibliography of British Ornithology. From the earliest times to the end of 1912. Including Biographical Accounts of the Principal Writers and Bibliographies of their Published Works. By W. H. Mullens, M.A., LL.M., F.L.S., M.B.O.U., and H. Kirke Swann. To be issued in six bi-monthly parts. London: Macmillan & Co., Ltd., St. Martin's Street, 1916. (Pp., Part I., 1–112; Part II., 113–24.) Price 6*s*. each part.

Mosquito Control in Panama. The Eradication of Malaria and Yellow Fever in Cuba and Panama. By Joseph A. Le Prince, C.E., A.M., Chief Sanitary Inspector, Isthmian Canal Commission, 1904–14, and A. J. Orenstein, M.D., Assistant Chief Sanitary Inspector, Isthmian Canal Commission. With an Introduction by L. O. Howard, LL.D., Entomologist and Chief, Bureau of Entomology, United States Department of Agriculture. With 100 illustrations. London and New York: G. P. Putnam's Sons, The Knickerbocker Press, 1916. (Pp. xvii + 335.) Price $2·50 net.

More Minor Horrors. By A. E. Shipley, Sc.D., Hon. D.Sc. Princeton, F.R.S., Master of Christ's College, Cambridge, and Reader in Zoology in the University. Illustrated. London: Smith, Elder & Co., 15, Waterloo Place, 1916. (Pp. xvi + 163.) Price 1*s*. 6*d*. net.

The Animal Parasites of Man. By H. B. Fantham, M.A. Cantab, D.Sc. Lond., Lecturer on Parasitology, Liverpool School of Tropical Medicine, Sectional Editor in Protozoology, *Tropical Diseases Bulletin*, London, etc.; J. W. W. Stephens, M.D. Cantab, D.P.H.; Sir Alfred Jones, Professor of Tropical Medicine, Liverpool University; and F. V. Theobald, M.A. Cantab, F.E.S., Hon. F.R.H.S., Professor of Agricultural Zoology London University, Vice-Principal and Zoologist of the South-Eastern Agricultural College, Mary Kingsley Medallist, Grand Médaille Geoffrey St. Hilaire, Soc. Nat. d'Acclim. de France, etc. Partly adapted from Dr. Max Braun's *Die Tierischen Parasiten des Menschen* (fourth edition, 1908), and an Appendix by Dr. Otto Seifert. With 423 figures in the text. London: John Bale, Sons & Danielsson, Oxford House, 83-91, Great Titchfield Street, Oxford Street, W., 1916. (Pp. xxxii + 900.) Price 45*s*. net.

Milk and Its Hygienic Relations. By Janet E. Lane-Claypon, M.D., D.Sc. Lond., Assistant Medical Inspector under the Local Government Board. Published under the direction of the Medical Research Committee (National Health Insurance). With 8 plates and diagrams in the text. London: Longmans, Green & Co., 39 Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. viii + 348.) Price 7*s*. 6*d*. net.

Laboratory Manual in General Microbiology. Prepared by the Laboratory of Bacteriology, Hygiene, and Pathology, Michigan Agricultural College. New York: John Wiley & Sons. London: Chapman & Hall, 1916. (Pp. xvi + 418.) Price 10*s*. 6*d*. net.

Medical and Veterinary Entomology. A Text-book for use in Schools and Colleges as well as a Handbook for the use of Physicians, Veterinarians, and Public Health Officials. By William B. Herms, Associate Professor of Parasitology in the University of California, Consulting Parasitologist for the California State Board of Health, and formerly Professor of Zoology and Parasitology in the San Francisco Veterinary College. New York: The Macmillan Company, 1915. (Pp. xii + 393.) Price 17*s*. net.

Discovery; or the Spirit and Service of Science. By R. A. Gregory. With 8 plates. London: Macmillan & Co., St. Martin's Street, 1916. (Pp. vii + 340.) Price 5*s*. net.

The Cruise of the *Tomas Barrera*. The Narrative of a Scientific Expedition to Western Cuba and the Colorados Reefs, with Observations on the Geology, Fauna, and Flora of the Region. By John B. Henderson. With 36 illustrations and maps. London and New York: G. P. Putnam's Sons, The Knickerbocker Press, 1916. (Pp. ix + 320.) Price 12*s*. 6*d*. net.

Biblioteca Scientifico-Politecnica Internazionale. Bibliografia delle più importanti opere italiane e straniere sulle Scienze Esatte, Dell' Ingegnere, le Arti Belle e le Arti Utili con speciale riguardo alla loro applicazione pratica industriale pubblicate dal 1905 al 1915. Disposte in ordine Alfabetico delle materie. Proemio del Senatore Giuseppe Colombo. Le opere elencate in questo catalogo si trovano o si procurano presso la Libreria Internazionale di Ulrico Hoepli, Editore Libraio della Real Casa. Milano, Galleria de-Cristoforis 59-65. Febbraio 1916. (Pp. xxiv + 468.) Price 3 lira.

Thomas Alva Edison. By Francis Rolt-Wheeler. With 5 illustrations and frontispiece. New York: The Macmillan Company, 1915. (Pp. viii + 201.) Price 50 cents.

The Declining Birth-rate: Its Causes and Effects. Being the Report of, and the Chief Evidence taken by, the National Birth-rate Commission, instituted with official recognition by the National Council of Public Morals, for the Promotion of Race Regeneration, Spiritual, Moral, and Physical. London: Chapman & Hall, Ltd., 1916. (Pp. xiv + 450.) Price 10*s*. 6*d*. net.

George Boole's Collected Logical Works. Vol. II. The Laws of Thought (1854). Chicago and London: The Open Court Publishing Company, 1916. (Pp. xvi + 448.) Price 15*s*. net.

The Influence of Greece on Science and Medicine. By Prof. D. Fraser Harris, M.D., D.Sc. Reprinted from the *Scientific Monthly*, July 1916. (Pp. 51–64.)

A Scientific German Reader. Edited, with Introduction, Notes and Vocabulary, by Herbert Z. Kip, Ph.D., Associate Professor of Germanic Languages, Vanderbilt University. With 31 figures in the text. Oxford German Series, by American Scholars. General Editor, Julius Goebel, Ph.D., Professor of Germanic Languages in the University of Illinois. London: Oxford University Press, and New York, Toronto, Melbourne, and Bombay, 1916. (Pp. xii + 445.) Price 5*s*. net.

An Emperor's Madness or National Aberration? By Ernesto Lugaro, Professor Extraordinary of Neuropathy and Psychiatry in the University of Modena. Translated by W. N. Robinson, M.D. London: George Routledge & Sons, Ltd. New York: E. P. Dutton & Co., 1916. (Pp. 135.) Price 2*s*. 6*d*. net.

Poland's Struggles for Independence. By Rajmund Kucharski. With a Foreword by Lord Weardale. Published for the Polish Information Committee by George Allen & Unwin, Ltd., Ruskin House, 40, Museum Street, W.C. (Pp. 48.) Price 6*d*. net.

A Modern Job: An Essay on the Problem of Evil. By Étienne Giran, with Introduction by Archdeacon Lilley. Authorised Translation by Fred Rothwell. Chicago and London: The Open Court Publishing Company, 1916. (Pp. 92.) Price 2*s*. 6*d*. net.

ANNOUNCEMENTS

ROYAL ASTRONOMICAL SOCIETY. Meetings, 5 p.m., November 10, December 8.

ROYAL METEOROLOGICAL SOCIETY. Meetings, 7.30 p.m., November 15, December 20.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

SCIENTIFIC PARLIAMENTARY REFORM

By W. H. COWAN, M.P.

It has been well said that it is the greatest glory of the British people to have evolved and perfected that system of Representative Government and those Parliamentary Institutions which, in one form or another, have been adopted, or imitated, by every progressive nation and constitute our greatest contribution to the happiness and prosperity of mankind. Every national representative assembly in the world is modelled, either at first or second hand, on the British House of Commons, which is, indeed, the mother of them all.

Theoretically, the House of Commons is more powerful to-day than ever before in the whole course of its long and eventful history. Having, through centuries of painful effort, wrested power from the hands of kings, it has now, at last, to all appearance, secured for itself the authority of a Single Chamber Legislature, while escaping the odium of having abolished that picturesque and historic Chamber which still provides an attractive place of refuge, or retirement, for many a weary Commoner whose Radical antecedents do not, in the opinion of his leaders, disqualify him for its dignified repose.

And yet, all is not well with the Mother of Parliaments, for this seemingly all-powerful House of Commons has, of late, become the object of almost universal distrust. The evidence of this changed attitude on the part of the Electorate meets one on every hand. The Press teems with hostile criticism and complaint which, whether well or ill founded, is indicative of a feeling too significant and too disquieting to be disregarded. Payment of members, without a mandate from the country, and the prolongation of the life of Parliament by its own action are no doubt partially responsible for this state of feeling, but we must look deeper for an adequate explanation of a phenomenon which cannot be wholly due to any temporary or superficial cause. Statesmen, desirous of restoring the moral authority of an Institution the ancient reputation of

which has been gravely compromised by the events and changes of recent years, are now strenuously advocating the extension of the Franchise and the reform of our Electoral System, but these tardy concessions to popular sentiment are powerless, to-day, to satisfy a public which is at last awakening to a perception of the real nature of the disease which afflicts the body politic.

Public attention has been focused upon the real cause of much of the trouble by an incident which occurred within the last few weeks. Speaking in the House of Commons on October 31, a member of Mr. Asquith's Cabinet had the audacity to address that assembly in the following terms:

> "We must carry on the government of the country, badly I agree, but as well as we can do it; and we cannot share that responsibility with the House of Commons or with anybody else—not during the war."

As the late Prime Minister, when subsequently invited to dissociate himself from this astonishing pronouncement, replied, "I do not repudiate it in the least," it must be assumed that this utterance represented the view of his Cabinet as a whole. It is the acquiescence of the House of Commons—dating from a period long prior to the outbreak of the war—in the usurpation of its powers by the Cabinet and in the gradual extinction, in favour of the Government, of the rights of private members that is chiefly responsible for the contempt into which the House has fallen in recent years. Just as the incredible folly of the House of Lords in rejecting a Budget afforded the long-awaited opportunity for destroying its powers, so the systematic obstruction practised by certain members of the House of Commons, in the early years of the Home Rule Controversy, justified, and indeed necessitated, the introduction of the Closure which, applied as time went on with ever-increasing stringency and ingenuity, now enables any Government with a sufficiently numerous and docile majority to silence inconvenient criticism and stifle debate. While such alterations as have been made in Parliamentary procedure have resulted in a progressive curtailment of the rights of private members, successive leaders of the House have affected a pedantic attachment to ancient tradition which has excused them from

making any comprehensive attempt to adapt mediæval practice to modern conditions, and has resulted in the perpetuation of anachronisms which even the most sentimental regard for a glorious past fails altogether to justify. A picturesque instance of this survival is to be found in the Parliamentary fiction of the Speaker's Eye. Any one who takes the trouble to scan the pages of Hansard will discover, if his scrutiny extends over any reasonable period, that some 90 per cent. of the speeches in the House of Commons are made by some 10 per cent. of its members. While no alteration of the Standing Orders of the House could endow Mr. Speaker's eye with that wider range of vision so much to be desired, it should not pass the wit of man to devise some means of calling upon members, at once less archaic and more logical than the present system. Pending such a reform, the Speaker's task would admittedly be rendered more easy by the imposition, under a new Standing Order, of some drastic limitation of the length of speeches. The time thus set free would, more or less automatically, fall to the share of those members who, under present conditions, are crowded out of almost every important debate by the handful of self-constituted experts who can always make good their claim to be heard, and who, once on their feet, rarely know when to sit down. But, often as this suggestion has been made, the existing method possesses so many conveniences from the point of view of those responsible for the dispatch of business, in an artificially congested Assembly, that no assistance can be obtained from these gentlemen in bringing about so obviously desirable a reform.

It is scarcely too much to say that, under the dexterous guidance of a succession of old Parliamentary hands, the whole machinery of the House of Commons has, by almost imperceptible stages, been deflected from its original purpose, and is now employed to impose upon the nation the autocratic will of Cabinets which, if little less tyrannical than the Stuart Kings, avoid the cruder methods which so often brought those interesting but misguided monarchs into conflict with an assembly which was, then, still jealous of its rights and privileges.

Space does not permit of our tracing the gradual process by which the virtual substitution of Cabinet for Parliamentary Government has been brought about, nor need we waste time

in bemoaning the apathy with which each successive House of Commons has yielded something to the pressure of the political machine. Our present purpose is to deal with facts, rather than to inquire exhaustively into the causes which have conspired, under a process of peaceful penetration, to convert a free democracy into an oligarchy the members of which enjoy practically absolute power, under cover of Parliamentary forms and institutions. Just as Samson of old was able to impose his will upon the Philistines until the secret of his strength was disclosed, so will British Cabinets continue to dictate to British Parliaments, until the conditions under which they exercise their usurped authority are more fully understood by the British people. It is as a contribution to that fuller understanding, which must precede Parliamentary reform, that the following suggestions are presented.

It must not, of course, be supposed that the Government—to whichever party it may belong—is necessarily a greater sinner than the Opposition. It has merely, for the time being, greater opportunities of domination. When an Opposition becomes a Government, it inevitably succumbs to the temptation to use the implements which it finds lying ready to its hand, to maintain the discipline of its Party and the authority of its leaders.

Under modern Parliamentary conditions, the Prime Minister of the United Kingdom is a more powerful autocrat than the head of many a sovereign State, for he not only makes and unmakes Ministers, but, in virtue of his constitutional right to advise the Crown to dissolve Parliament, is able to control a recalcitrant majority in the House of Commons, by threatening his followers with the instant loss of their seats in the event of their failing to obey his behests as interpreted to them by the Party Whips. These are tremendous powers to place in the hands of any man and were formidable indeed when exercised, as in recent years, by a personality so powerful, and an intellect so acute, as that of the late Prime Minister. They are powers which not even the most brilliant and sagacious of Party leaders should possess. Human nature being what it is, and the love of power inherent in the human breast, and most highly developed in the most capable members of our race, it would be futile, as well as unjust, to censure individuals for exercising an authority which they fondly imagine

that a trustful and confiding nation has deliberately placed in their hands. Nor, perhaps, would it be altogether fair to blame men who regard themselves as indispensable to the country, for taking such steps as seem to them necessary to entrench themselves in positions which they honestly believe that they alone can adequately fill.

Cabinet autocracy is not, however, solely dependent upon the power of the Prime Minister to appoint and dismiss his colleagues, nor upon the latent power of dissolution, though the latter is probably its main support. It is reinforced by all the influence of a vast and complex party organisation, in the House and in the country. The Party Whips administer an elaborate system of rewards and punishments with a single-minded devotion to the object to be achieved. It is always pleasant for a member to be on good terms with these gentlemen, who are generally courteous and considerate. There are circumstances in which it is not only disagreeable but dangerous to offend them, either by a pedantic observance of pre-election pledges, or by the display of a temperamental independence inconsistent with the proper working of a machine which, if it grinds slowly, can often grind exceedingly small. Little wonder that, after more or less resistance, the average member ends by placing his conscience in commission and renouncing an independence sometimes as little appreciated by his supporters as by his leaders. One by one, the troublesome members of a new Parliament succumb to the threatenings or to the blandishments of their Whips, and thus is preserved the solidarity of the Party, and thus, we are given to understand, is the Empire saved from disruption. The few whose individuality will not permit them to conform, in all respects, to the wishes of their chiefs, maintain a precarious Parliamentary existence whose rigours may, however, be tempered by the approval of constituents whose respect is often slowly won by qualities little appreciated at Westminster. The power of the Whips, both Government and Opposition, in the country, makes itself felt chiefly in the selection of candidates, and so perfect is their control over the local party organisations, that, in ninety-nine constituencies out of a hundred, no candidate not approved at headquarters has a chance of adoption. A " safe " man, who will vote the party ticket and obey orders, is sure of a warm welcome when he

reaches the House, which is at once the goal of his ambitions and the grave of his ideals. The system is as nearly perfect, for the purpose in view, as human ingenuity and vast experience can make it, for the men so selected and so shepherded can generally be relied upon to march through the division lobbies at the crack of the whip, and to emerge therefrom with the proud satisfaction of having done their duty to their Party even though, as is often the case, they are unable to name the subject upon which their vote was given.

The control of the House of Commons over finance is an empty boast. In the House which is the guardian of the people's purse, untold millions were voted each session (even in pre-war days) without discussion, often in the small hours of the morning, by members too tired and too indifferent to be conscious of any breach of trust.

Much has been heard about the congestion of business in the House of Commons, and much sympathy has been wasted upon Ministers on this account. To this cause are conveniently attributed the neglect of many important matters, the postponement of necessary legislation, and the inadequate discussion of vital questions. Yet, such is the perverse ingenuity of mankind, that it is doubtful whether this manifest evil is not regarded, by some of those responsible for the business of the House, as a veritable blessing in disguise; for, as the end of each session approaches, and jaded legislators emulate wearied Ministers in their anxiety to seek repose, hard-working Whips find in this very congestion of business their most powerful ally in rushing the final stages of belated Government Bills, and in inducing the authors of all sorts of inconvenient projects to consent to the massacre of their offspring. "Unless this is dropped, we shall have to sit until the end of August"; "Unless that is withdrawn, we must meet in Autumn Session," and so Agriculture languishes, Science is starved, and Education neglected, while half-baked Government Measures are rushed upon the Statute Book, full of glaring anomalies and imperfections, lest fuller discussion should lead to undesired results. Members are reminded, by their ubiquitous mentors, that under the Standing Orders of the House, all Bills which have not reached their final (Third Reading) Stage, automatically die with the expiring Session, and that, as each session has its own heavy burden of new legislation to face,

there is little chance of time being found to revive, and again pass through all their stages, measures sacrificed on this account. Yet one Government after another has refused to consent to the trifling alteration in procedure which would be required to enable uncompleted Bills to be "carried over," at the stage they had reached, from one session to another; the only reason vouchsafed for such an attitude being the irrelevant argument that if the House of Commons carried over its Bills the House of Lords could do the same! It is, of course, comprehensible that Ministers should hesitate to part with one of the arguments which enable them, on occasion, to overawe troublesome members by declaring in committee that discussion of their amendments will "kill the Bill." It is the Whips' part to privately, and in the most obliging manner, warn inexperienced legislators that their Constituencies may resent their "wrecking" Government measures. The warning is usually sufficient.

Again, all-night sittings are not considered an unmixed evil, for many a "deal" is done in the silent watches of the night between an astute Minister and a tired-out Opposition, which buys repose by the temporary sacrifice of its principles, or momentarily abandons obstruction in order to get to bed. It is scarcely surprising that an assembly whose members are thus manipulated, wrapped in the swaddling clothes of effete tradition and strangled by the Closure, has become the obedient instrument of powerful Ministers, who have arrogated to themselves its powers, annulled its rights, and contemptuously ignore its privileges.

Was it for this, one is tempted to ask, that the House of Commons defied kings and disarmed nobles? And will the British People, in this its time of testing, stand by and see its representatives abandon to any man, no matter how great or how distinguished, powers which belong to the People and to it alone? Will it not, rather, in the days of reconstruction which are soon to come, insist upon a new Magna Charta, under which all usurped authority shall be laid down and the supremacy of the Mother of Parliaments restored?

No revolution is required to recover for Parliament its rightful place in the Constitution nor to regain for the House of Commons the respect and confidence of the nation. Trifling alterations in procedure, permitting, for example, the carrying

over of Bills from one session to another, and limiting the duration of speeches, would go far to relieve that congestion of business which is the excuse for so many legalised irregularities; while two Parliamentary Reforms, so simple as to be readily understood and appreciated by the man in the street, would suffice to relieve the elected representatives of the people from Cabinet Autocracy, on the one hand, and from Party tyranny on the other. Let us, in the first place, following the example of other nations, and abandoning, to that small extent, our insular conservatism, elect our popular Chamber for a fixed term of four or five years. A Prime Minister deprived, by this change, of the power of dissolution, dare not attempt to force unpopular measures upon supporters who, without endangering their seats, could at any time replace him by a statesman enjoying the confidence of the majority of the House. By this easy and well-tried method, Ministers would become in fact, what they are now in theory only, the leaders and not the masters of the House.

The influence of Party cannot be eradicated in a Representative Assembly, but experience has shown that it must be limited and controlled. In order, not to destroy, for that is impossible, but to curtail the power of the Whips let us, in the second place, extend to members, who now vote openly in the Division Lobbies, the protection which the Ballot Act has for many years conferred upon electors voting at the polling booths. The object of the Ballot Act is to ensure that every elector shall be able to vote according to his convictions, and without fear of consequences; but the object of the Party Whips in the House of Commons is to ensure that every member shall vote with his Party, irrespective of his convictions, and with a clear understanding of the consequences of any failure to obey their injunctions. Every argument which can be advanced against granting this protection to the representatives of the people can, with equal force, be employed against giving it to the people themselves, while the contention that members of the House of Commons must be denied the protection of the ballot in order that their constituents may know how they vote in each division, is disingenuous and designed to conceal the real object of open voting. The Closure, now so often oppressive, and even brutal, in its application would, under the ballot, lose much of its harshness while pre-

serving all of its utility, for members, thus protected against unfair pressure, would not hesitate to vote against the arbitrary suppression of reasonable criticism, and could be relied upon to co-operate with the Chair in checking merely vexatious opposition and in putting down actual obstruction.

Though the Whips are apt to regard private members as mere voting machines, and party organisations too often look upon them as delegates, it cannot be in the best interests of the State that members should accept either position, nor do the electors, apart from the Machine, desire or expect them to do so. The obligation of an honourable man who accepts election as the candidate of a Political Party, is to support, in all its essential principles, the policy of that Party; using his intelligence to determine, in each individual case, whether or not a particular measure is in harmony with those principles. Such a reform as is here suggested would ensure greater care, as well as greater freedom, in the selection of Parliamentary candidates, would exclude from the House of Commons persons who could not be trusted to fulfil implicitly the honourable obligations which they undertook, and would open up the possibility of a Parliamentary career to many distinguished men who, under present conditions, are unwilling or unable to enter the House.

It may, of course, be argued that neither the election of Parliament for a fixed term nor the introduction of voting by ballot into the House of Commons would, in itself, provide a complete remedy for all the ills that Parliament is heir to; but while this may be admitted, it is contended that as these reforms are at once simple in themselves, easily understood, and strike at the roots of a tyranny which has been shown to be the chief cause of Parliamentary decline, their adoption is a matter of urgent necessity and ought not to be postponed. It may be well, however, to emphasise the fact that while the election of Parliament for a fixed term and voting by ballot in the House of Commons would, operating together, afford valuable protection against the undue pressure and improper influence now brought to bear upon members, neither of these reforms would, by itself, be adequate for that purpose; for, as the secrecy of the ballot would not rob the threat of dissolution of its terrors for members nervous as to the retention of their seats, neither would the removal of the

threat of dissolution prevent the Party Whips influencing members by calling attention to their votes as given in the Division Lobby.

There are, of course, Constitutional questions of the first importance long overdue for settlement. Of these the most pressing are probably devolution and the reconstitution of the Upper House, and, although it is not here suggested that the adoption of the reforms recommended above should depend upon the prior solution of these highly contentious matters, it is submitted that the difficulties in the way of their settlement have been quite unduly magnified, and that the present is an opportune time for disposing of them finally and by agreement.

Such congestion of business at Westminster as is really inevitable is due entirely to the attempt of one Parliament to legislate for three Kingdoms and for a whole Empire; a task as unnecessary as it is impossible of adequate performance. Four-fifths of Ireland clamours for Home Rule. Ulster could be satisfied by a similar grant in her own area; eventual reunion being inevitable provided that the parties were left free to negotiate it at their own time. Scotland is, admittedly, capable of managing her local affairs, and is more than willing to do so. Wales is anxious to shoulder her own burden, while England, dominated by the "Celtic fringe," longs to be mistress in her own house. "Home Rule all round" would satisfy all these aspirations, set free the Imperial Parliament to deal with Imperial matters, and facilitate that Federation upon which the future strength and happiness of the peoples of our vast Empire depend.

No responsible statesman has ever pretended that the House of Lords, as at present constituted, and limited, can be a permanent feature in our Constitution. The Government which under the Parliament Act deprived that Assembly of all but the shadow of its former powers declared, in the preamble to the Parliament Bill, that it was its intention to set up, in its place, a Second Chamber constituted upon a popular instead of a hereditary basis. That Government never found a convenient season for fulfilling this important pledge, which is none the less morally binding upon those who gave it and, though perhaps to a less degree, upon their successors in office. The House of Lords, by the patriotic self-restraint

which it has shown during the war, and by the moderate use which it has made of such powers as it is still able to exercise, has, to some extent, rehabilitated itself, and this at the expense of a Cabinet-ridden House of Commons. The country is not likely to forget that it was the rejection by the House of Lords of the Declaration of London (in virtue of its right of temporary veto) which made it possible for a British Government to escape, without discredit, from fetters which would have hampered the Navy in the exercise of its functions and rendered an effective blockade of the German coasts impossible. No serious politician can, at this time of day, defend the hereditary constitution of the Upper House, but the time has, surely, come for setting up such an assembly as Mr. Asquith pledged himself to create, and there should be no serious opposition to entrusting such a body, including, as it no doubt would, the best elements in the present House of Lords, with all the proper functions of a Revising Chamber, reserving, of course, to the House of Commons exclusive control over finance.

All these matters are of greater present importance than the Franchise and Electoral Reforms of which we hear so much, and of which, at this particular time, we should probably have heard nothing at all had it not been for the late Coalition Government's mismanagement of the simple issues involved in the enfranchisement, on a War Service qualification, of soldiers, sailors, and munition workers, and its strange, and to some minds suspicious, reluctance to bring the ordinary Parliamentary Register up to date.

The suggestion that the enfranchisement of women munition workers involved the whole question of Adult Suffrage need not have been listened to, for the advocates of that particular extension of the Franchise, who are by no means lacking in intelligence, would undoubtedly have accepted such an instalment, on account of their larger claim; realising as they do that with the Statutory Enfranchisement of even one woman the case against Female Suffrage is given away.

But a Government which, from first to last consistently avoided the line of least resistance could not be satisfied with giving the country just what it demanded. It must prove its quixotic generosity by forcing upon it, in the very crisis of the greatest war in history, a huge scheme of Franchise and Electoral Reform and therefore, after exhausting every possibility of

procrastination and delay, and autocratically overruling a recent decision of the House, appointed a Committee "to examine and, if possible, to submit agreed resolutions on the following matters":

(*a*) Reform of the Franchise.
(*b*) Basis for redistribution of seats.
(*c*) Reform of the system of the registration of electors.
(*d*) Method of elections and the manner in which the costs of elections should be borne.

The Cabinet, to quote the words of Sir Edward Carson, thus "referred the whole constitution of this country to a Committee not appointed by the House at all," for Mr. Speaker, who has been "kind enough to consent" to preside over this Committee (officially described as a Conference), accepts responsibility for the selection of its members. The implied claim of this curiously constituted body to instruct the House how to vote upon such questions as Adult Suffrage, Women's Suffrage, and Suffrage for Soldiers and Sailors was, fortunately, repudiated in advance, as "one of the most absurd things ever attempted," by the then leader of the largest and most influential section of independent opinion in the House, who, moreover, did not hesitate to predict that "very little attention will be paid to its report when we get it."

Realising, no doubt, the inadequacy of this device to stem the rising tide of indignation in the country, at the dilatory tactics of a Government which at one time declared it to be impossible for soldiers and sailors, on active service, to vote at all, at another time expressed anxiety to enable them to do so and, all the time, refrained from taking any steps to make their participation in a war-time election possible, Mr. Asquith and his colleagues decided to introduce a Special Register Bill which, when presented, contained no provision for the registration, as Parliamentary electors, of soldiers, sailors, and munition workers, as such. It is unnecessary to follow further the fortunes of this unhappy measure, the fate of which is well known.

The conviction that until Parliament has set its own house in order, no extension of the Franchise, however wide, and no Electoral Reform, however drastic, can restore its former authority to the House of Commons, or regain for it its place in the regard of the nation, need not preclude a

very brief examination of the matters referred to the Speaker's Conference, though these are, for the most part, under present circumstances, of quite secondary importance.

(*a*) The reform of the Franchise and (*c*) the reform of the system of registration of electors are closely associated subjects. The former—if it is serious at all—implies the widest possible extension of the right to vote, while the latter resolves itself into the simplification of our present absurd system of registration. These are highly technical subjects, owing to the present multiplication of franchises and the consequent complexity of the Register. "Adult Suffrage," once the war cry of the most dangerous type of Radical, no longer alarms the most Tory of Conservatives. The question is admittedly one of expediency, and, subject to reasonable safeguards, there is every reason to believe that all parties will ultimately agree to the enfranchisement of all reputable persons, male and female, of a specified age. But whether this future measure of enfranchisement is accorded soon or late, it is certain that the nation will insist upon the immediate grant of the Franchise, the symbol of citizenship, to every soldier and sailor who has served his king and country, on land or sea, as well as to the men and women whose tireless and equally patriotic labour in the munition factories will have no less contributed to the certain victory of our arms. It is unnecessary here to enumerate the multitudinous qualifications, and disqualifications, for the Franchise which are recognised by our existing electoral laws or to refer to the vagaries of the Annual Registration Courts. While it is obvious that machinery must exist to provide, among other things, for the identification of every individual voter, and that registers must therefore be kept, it is equally obvious that no reform can be satisfactory which does not reduce this machinery to a minimum, in the interests alike of efficiency and economy.

(*b*) A system under which one-half of the members of the House of Commons represent practically twice as many electors as are represented by the other half of the House, is too absurd to be capable of defence. That, in a nutshell, is the case for redistribution. In the presence of this glaring anomaly, it is futile to discuss individual cases. Apart from the not unreasonable objection of the Irish members to any reduction in the representation of Ireland, which (though on the basis

of population admittedly excessive) is justified as being an essential condition of the Treaty of Union, there is no serious difficulty in dealing with this matter on a scientific, and therefore permanent, basis. To effect a satisfactory solution, the boundaries of many existing constituencies must be rectified; a few constituencies must be merged in others; a certain number of new Parliamentary divisions must be created; and all this must be done in such a way as to secure numerically equal voting areas. As the population of these areas will vary in the course of time, it will be necessary to provide machinery for an automatic readjustment of boundaries, say once in ten years, in accordance with the results of each successive census. This system is already in successful operation in some of our self-governing Dominions, and its adoption here would not, therefore, be in the nature of an experiment. The actual work of redistribution should, of course, be entrusted to a judicial commission, free from any suspicion of political or party influence.

(*d*) "Methods of election" is a somewhat vague expression which it is not necessary, at the moment, to follow in all its ramifications. Under this heading arise, of course, the proposals of the Proportional Representation Society. Irrespective of the attitude of the Speaker's Conference on this debatable matter, it may be remarked that this particular method of election has much to recommend it, securing, as it does, that "minority representation" which is essential in any Assembly which claims to reflect the balance of parties in the constituencies. The backbone of the resistance to this reform is probably to be found in the conviction of party managers that its adoption would tend to a splitting up of existing parties, and to the formation of groups on the Continental model in the House of Commons. There is, of course, room for difference of opinion as to whether this modification, or subversion, of our two-party system is desirable. It may, however, be considered doubtful whether the adoption of Proportional Representation, as a method of election, would do more, in this direction, than accelerate a process which is already going on. Proportional Representation would, moreover, in certain circumstances, weaken the authority of Governments by making impossible such inflated majorities (out of all proportion to the balance of parties in the country) as are, from time to

time, returned under our present methods of election. It may well be that this would not be an altogether undesirable result. There will be much difference of opinion as to the "manner in which the costs of elections should be borne," but it is probable that all parties may agree that Returning Officers' fees, at any rate, should be borne by the State. The question as to whether it may not be desirable to discontinue payment of members, on the understanding that the State shall bear the whole burden of all legitimate election expenses, is worthy of careful consideration. No reform of our methods of election can be complete or satisfactory which does not provide for the recording and counting of the votes of absent electors such as fishermen and other persons who, when following their callings, are under our present system frequently disfranchised.

Though the value attaching to the Report of the Speaker's Conference may be small, its chief interest must be the indication it gives as to how far any general agreement, among members of different parties, is possible on matters which have in the past been the subject of acute and sometimes acrimonious controversy.

While it may be difficult to take this Committee quite seriously, there is a danger that it may be used to divert public attention from the real causes of Parliamentary inefficiency and decline. Such a result would, perhaps, not be altogether unwelcome to rulers, who, wiser in their generation than the autocrats of old, seek, not to destroy, nor even to suppress, Reformers, but are content to divert their energies into innocent channels, arguing that Reforms which do not touch their own privileges, or threaten the loss of the authority they have usurped, may be tolerated and even encouraged. They know that it is a mockery to extend the Franchise to new classes of electors who are only to share the privilege of voting for members powerless to give effect to the wishes of their constituents, yet they do not hesitate to insult the intelligence of the Democracy by offering it a larger influence in determining the composition of a House with which they refuse to "share the responsibility" of governing the country.

When will statesmen, who seemingly regard the House of Commons as existing merely for their personal convenience,

realise that a nation which has outgrown its childhood will no longer accept the shadow for the substance or allow itself to be duped by fine phrases? Under the influence of an experience unparalleled in human history, Britain has awakened from her long slumber. Let those who would keep her still in leading strings beware. Institutions which are the slow growth of centuries of conflict and experience are of priceless value to a nation, and those who endanger their stability, by refusing to allow them to adapt themselves to changed and changing circumstances, incur a grave responsibility. They are the real revolutionaries of to-day.

In this connection, it is certainly significant that although the National Government which has recently come into being owes its existence to extra-Parliamentary pressure; Mr. Asquith's resignation not having been brought about by any hostile vote of the House of Commons; the new Prime Minister —a man of boundless energy and democratic temperament— has taken the earliest opportunity of dissociating himself from the dictum that the Ministry "cannot share its responsibility" for the government of the country with the House of Commons; declaring that "the control of Parliament as a whole must, and always must, be supreme." As Mr. Lloyd George, not content with the expression of a pious opinion, added, "I do not think that the present methods of Parliamentary control are efficient," it is perhaps not too much to hope that, recognising the futility of any mere tinkering at the Franchise, he will, at the first favourable opportunity, boldly attack the larger problem of restoring to the House of Commons its rightful place in the Constitution.

While the time has gone by for merely empirical alterations in our Parliamentary system, a *bona-fide* attempt at reorganisation on scientific principles would be universally welcomed. Such a reform, avoiding extremes and conserving all that is best in what has come down to us from past ages, would recover for Parliament the moral authority upon which its influence so largely depends, and would enable the nation to undertake the tremendous task of general reconstruction, which must follow the re-establishment of peace, under conditions ensuring a glorious future for the Empire.

A THEORY OF COLOUR VISION[1]

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1. The eye is sensitive only to a limited range of wave-lengths in the spectrum, from 4000 A.U. at the beginning of the violet to 7600 A.U. at the end of the red. It is not equally sensitive to all the wave-lengths lying within this range, but there is a maximum on both sides of which the sensitiveness falls away rapidly. If the spectrum is a very bright one, this maximum lies in the yellow, but if the brightness is decreased it moves towards the green.

What is the cause of this selective action of the eye? It may be ascribed to the ultra-violet and infra-red radiations being absorbed by the substance of the eye, *i.e.* the aqueous and vitreous humours and crystalline lens, before they reach the retina. But this explanation is at best only a partial one. It has been shown by experiment that the near ultra-violet and near infra-red are not absorbed by the substance of the eye, and thus, as Helmholtz points out, the selective action must be ascribed to something in the retina itself.

The selective action of the eye is analogous to phenomena occurring in many other regions of physics. For example, if we have two tuning-forks of the same pitch, and the first of these is sounded and held near the second, the sound waves strike the second and set it into vibration. There is then said to be resonance between the two forks. For resonance to take place in this case it is necessary that the pitches of the two forks should be very accurately adjusted to equality. The second fork thus exercises a selective action on the sound waves; if their pitch coincides with its own, it absorbs energy from them and is set into vibration, but, if their pitch is different from its own, it remains at rest.

[1] A non-mathematical account of a theory which has already been described in the *Proc. Roy. Soc.* A, 92, p. 424, 1916.

In the same way, if a ship at sea is lying broadside on to the waves, and the natural period of rolling of the ship coincides with the period of the waves, the ship may be made to roll violently, even though the waves are comparatively small. For the effect of each successive wave is added to that of its predecessor, and the cumulative effect is great. But if the period of the waves does not coincide with the natural period of the ship, the effects of successive waves neutralise one another and the ship remains at rest. For example, the first wave may give the ship a motion to the right, and the second may strike it when it is coming back again through the equilibrium position, thus destroying the motion which the first wave has created. The ship thus exercises a selective action on the waves, taking energy only from those the period of which is approximately the same as its own.

The resistance of a selenium cell changes when the cell is exposed to the action of light. The change is found to be a selective one, dependent on the wave-length of the light. If the cell is stimulated in succession with monochromatic light of different wave-lengths, and the change in resistance noted and plotted as a function of the wave-lengths, the curve is found to have a maximum for one particular wave-length and to fall away on both sides of this maximum. The change can be explained by supposing that in the selenium cell there are electrons capable of executing vibrations about positions of equilibrium. These electrons are set into vibration by the incident light wave, and the amplitude of the vibrations is greatest when the period of the incident light coincides with the natural period of the electrons. When the amplitude passes a certain critical value, the electron breaks free from the point to which it is attached, and the increase in the number of free electrons causes the increase in the conductivity.

Now these three cases—tuning-fork, rolling ship, vibrating electron—are all represented mathematically by the same equation, namely the equation of the forced vibrations of a pendulum, though in the different cases different physical meanings are attached to the symbols involved. It seems reasonable, therefore, to attempt to explain the selective action of the eye in the same way. We shall do so here, and shall begin by giving an account of a piece of apparatus that gives forced vibrations and describing some experiments made by

it. Thus by means of a model we shall obtain the results of the mathematical theory without having any recourse to mathematics at all.

2. AB is a compound pendulum consisting of a long thin brass rod with a knife-edge and a cylindrical weight C. The length of the rod is 148 cm. and its diameter is 6 mm. The weight was made by filling a brass cylinder with lead. It can be moved up and down, and clamped in any position on the lower half of the rod, and thus the period of the pendulum can be altered from 1·60 to 2·40 sec. EF is a simple pendulum consisting of a ping-pong ball hung by a thread. This thread is attached to a wire DE, which is fastened to the rod by a

FIG. 1.

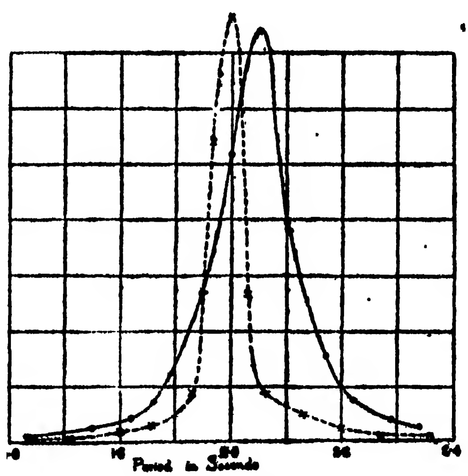

FIG. 2.

spring clip at D. The wire DE is parallel to the edge of the knife-edge.

The length of the simple pendulum was kept constant and its period was 2·03 sec. It is so light that its vibrations do not affect the motion of the compound pendulum appreciably.

The following experiment was made with this apparatus: the cylindrical weight was clamped high up so that the period of the compound pendulum was 1·63 sec. The compound pendulum was then drawn aside, so that it came against a wooden peg in a position making an angle of about 3° with the vertical, and held until the simple pendulum was perfectly at rest in a vertical position. It was then let go and com-

menced to execute vibrations. The amplitude of these vibrations remained practically constant throughout the course of the experiment. The point of support E of the simple pendulum participated in the motion of the compound pendulum, and thus the simple pendulum was set into vibration. A horizontal scale not shown in the diagram was fixed up close behind the thread parallel to the direction of motion, and the maximum value of the amplitude read. The cylindrical weight was then clamped in another position and the measurement repeated. The initial angle of the compound pendulum was the same in each case.

The results are represented by the smooth curve in fig. 2. The abscissæ give the period of the compound pendulum, and the ordinates the square of the maximum amplitude of the simple pendulum. The square of the maximum amplitude is plotted instead of the maximum amplitude itself, because the former, not the latter, is proportional to the energy acquired by the simple pendulum. The curve has a maximum at 2·05 sec. falling away rapidly on both sides of this. In the experiment recorded in the diagram, the spring clip was attached a distance of 6 cm. below the knife-edge. The relative value of the different ordinates does not depend on the position of D, but their absolute value does, *i.e.* if the distance of D below the knife-edge is increased, the ordinates of the curve would increase everywhere in the same ratio.

After the experiment with the ping-pong ball was completed, a similar one was carried out with a simple pendulum consisting of a small "marble" at the end of a thread. This time the clip was attached at a point 4 cms. below the knife-edge, and the period of the simple pendulum was 1·99 sec. The results are represented by the dotted curve in fig. 2, but on only half the vertical scale that the other curve is plotted on. If they were plotted on the same scale, the maximum ordinate of the dotted curve would be more than twice the maximum ordinate of the smooth curve.

Both the curves are of the same type, the difference being that the dotted one is narrower. This is because the marble offers less resistance to the air than the ping-pong ball. The smaller the frictional resistance, the narrower the curve, and consequently the more perfect the resonance.

3. Now let us apply the curves shown in fig. 2 to the

explanation of the selective action of the eye. Let us suppose that the energy of the compound pendulum represents the energy of the light wave, that in the retina there are a great number of oscillators or vibrators to perform the function of the simple pendulum, and that the energy of the simple pendulum represents the part of the energy of the light wave taken up by the eye.

Fig. 3 is taken from a paper by H. E. Ives,[1] and represents the sensitiveness of the eye to light of different wave-lengths. We learn from it, for example, that if the same quantities of energy are in succession changed into light of the wave-lengths 6400 A.U. and 5600 A.U., the luminosities of the sensation produced will be in the ratio of the numbers 1·7 and 9·8. The curve gives the mean of the results for eighteen observers, and the field of observation had the brightness of a magnesium

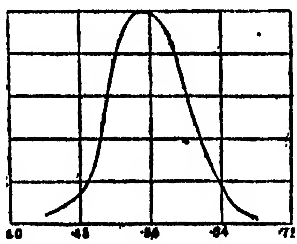

FIG. 3.

oxide surface undergoing an illumination of 25 metre-candles and viewed by an eye in which the pupil is at its normal aperture. If we compare Ives' curve with the two curves shown in fig. 2, we find it is broader than they are. They can be made broad enough by increasing the frictional resistance to which the simple pendulum is subject, but the shape does not then agree very well; the maximum of Ives' curve is smoother and not so pointed as they are. If, however, we assume that the vibrators in the eye have not all the same period, but that their periods vary about a mean value, which is itself in the yellow, we can represent Ives' curve perfectly.

The question arises as to the nature of the vibrators. It is possible, though not essential to the explanation, that they are electrons, and that, when they vibrate too far and break

[1] *Phil. Mag.* (6), 24, p. 860, 1912. The curve has recently been verified and extended by P. G. Nutting, *Phil. Mag.* (6), 29, p. 301, 1915.

free from the centres to which they are attached, the change occurring is to be identified with the bleaching of the visual purple.

Sir William Abney[1] likens the colour-perceiving apparatus in the retina to three pendulums, one for each primary colour. Thus he would regard Ives' curve as due to the sum of three curves, one representing the amount of energy taken up by the mechanism that produces the red sensation, one representing the amount of energy taken up by the mechanism that produces the green sensation, and one representing the energy taken up by the mechanism that produces the blue sensation. We shall leave the question as to whether there is only one or whether there are three classes of vibrators in the retina open at present, and shall proceed to a further study of experimental results obtained with the apparatus shown in fig. 1.

FIG. 4.

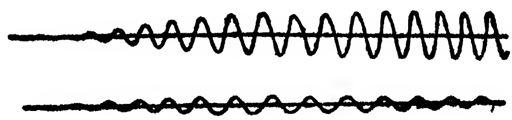

FIG. 5.

4. A clockwork kymograph—an apparatus consisting of two drums over which a band of smoked paper passed with a uniform speed—was lent me by the Physiology Department, and I endeavoured to obtain records of the motion of the simple pendulum. They did not prove very successful as long as a single thread was employed, but when the simple pendulum was replaced by a short brass cylinder suspended by two threads GH and EF, one at each end, and a short thread HP was allowed to trail a metal point P on the smoked paper, which was passing in the direction shown by the arrow, no difficulty was experienced. Fig. 5 gives two records, both, of course, reduced in size. The original was in each case about 24 in. long. For the upper curve the period of the compound pendulum was 1·93 sec., and for the lower curve it was 2·105 sec. In each case the period of the simple bifilar pendulum

[1] *Tyndall Lectures on Colour Vision*, pp. 36–9.

was 2·00 sec. Both records show the simple pendulum starting from an approximate state of rest.

The vibrators in the retina are not allowed to pursue their motion undisturbed. They may start from rest, but before many periods have elapsed they may break free from their points of attachment, or they may be brought to rest by an impact. Thus as typical of their motion we may take a piece of one of the curves given in Fig. 5. I have studied two portions of the lower curve very carefully, namely from the end of the first to the end of the ninth inch, and from the end of the first to the end of the twenty-first inch. The first portion gives the rise of the amplitude from zero to its maximum value. The second portion gives practically the whole curve.

According to Fourier's integral theorem, any curve may be represented as the sum of an infinite number of sine curves. In the special case here considered when the original curve is approximately periodic, although sine curves of all possible wave-length take part in its composition, those with approximately the same wave-length as the original curve have much the greatest amplitude. For example, the wave-length of the original curve, as obtained with the kymograph, seemed about 1·7 in. It can consequently be regarded as due to the superposition of sine curves of wave-length 1·0, 1·1, 1·2, 1·3 up to 2·4 in., the sine curves in the middle of the range having the greatest amplitudes. If we take the constituent sine curves twice as close, *i.e.* every twentieth inch apart, when added together they will reproduce the original curve with greater accuracy.

Just as the original motion may be regarded as built up of an infinite number of sine motions, the energy of the original motion may be regarded as distributed over these different sine motions. Lord Rayleigh has shown how to calculate the proportion that falls to each.[1] I made the calculation for both the 8-in. length and the 20-in. length of the lower curve in fig. 5. It was rather a tedious calculation, but as the details of the procedure may be interesting, I describe them in the following paragraph with special reference to the 20-in. length.

The curve was reproduced on squared paper, divided in tenths of an inch, and the value of the ordinate measured

[1] *Collected Works*, vol. iii. p. 268.

every tenth of an inch. This gave 200 figures. These were then arranged in 15 sets, in rows of tens, elevens, twelves, etc., up to twenty-fours, the rows of each set being arranged above one another; for example, the elevens gave 18 rows of eleven and one row of two, and the set was arranged so that they formed two columns of 19 and nine columns of 18. Each column was then added. In the case of the elevens we had eleven results; these eleven results were multiplied in succession by $\cos 0$, $\cos \frac{2\pi}{11}$, $\cos \frac{4\pi}{11}$, etc., the results added and the sum squared; they were then multiplied in succession by $\sin 0$, $\sin \frac{2\pi}{11}$, $\sin \frac{4\pi}{11}$, etc., the results added, and the sum squared. The two squares were then added. The result

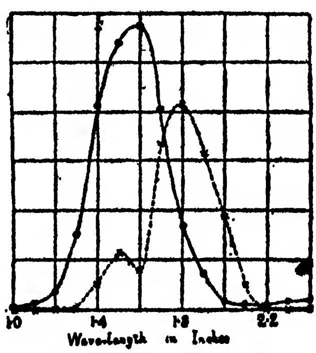

FIG. 6.

is proportional to the energy belonging to the wave-length 1·1 in. The other sets were treated similarly. In the case of the 8-in. length the procedure was the same, but only the first 80 ordinates were required.

The results are shown in fig. 6. The dotted curve gives the energy distribution for the 20 in., and the smooth curve the energy distribution for the 8 in. The vertical scale is ten times as great for the smooth curve as for the dotted curve. As mentioned above, the wave-length of the curve seemed from inspection to be about 1·7 in. If the period during which the amplitude is attaining its full value is taken by itself, however, most energy is associated with the wave-length 1·5 in. The dotted curve has two maxima, one at 1·5 in. and the other at 1·78 in.

5. Let us now apply the results of the preceding section to the eye. We have postulated vibrators in the eye to take up the energy of the light wave; we must also postulate a nerve to carry the energy from the eye to the brain. The preceding section shows that no matter how monochromatic the incident light is, the motion of the vibrator will not be monochromatic, but its energy will be distributed over a range of wave-lengths; for example, if the incident light is yellow, the motion of the vibrator when analysed will contain also some red and green. Consequently the energy passing along the optic nerve to the brain will be distributed over the red, yellow, and green with a maximum in the yellow. No matter how monochromatic the incident light is, the impression on the brain is not monochromatic.

The arrangement may be compared with a telephone. The vibrators in the retina play the rôle of the diaphragm in the transmitter which takes up the sound waves, and the optic nerve plays the rôle of the telephone wire. There is, however, one important difference. When a musical note is sung into the telephone, it is transmitted correctly along the wire, because the diaphragm reproduces the sound wave accurately. In the eye the vibrators do not reproduce the wave accurately. It is as if the diaphragm in the transmitter were subjected to disturbing influences of such a nature that the person at the other end hears a medley of musical notes over a range of half an octave on each side of the note originally sung into the transmitter.

Any arrangement of molecular dimensions for taking up the light wave must have some such modifying effect upon its character. This fact has hitherto escaped notice, but must receive attention as the modern views as to the nature of white light become more prevalent. The sine curve of uniform wave-length extending from negative to positive infinity was an abstraction, and has given rise to erroneous ideas.

Suppose, now, that the eye is simultaneously excited with monochromatic red and monochromatic green light, say lithium red and thallium green. Then two disturbances travel along the optic nerve, one which has an energy distribution given by the full curve on the right in fig. 7, and one which has an energy distribution given by the full curve on the left. If we add the ordinates of these two curves, we obtain the

brain we may ascribe it to excessive disturbance of the vibrators in the retina. This would make the energy curves widen out and lose their distinctive form.

The colour of the sensation produced by monochromatic stimulation varies with the intensity of the latter, becoming whiter for high intensities. This follows naturally from the theory developed here; when the amplitude of the vibrators is increased, they will be subject to more disturbance, and their motion will consequently be more irregular. Hence its energy will be more widely distributed over the spectrum.

THE JAW OF THE PILTDOWN MAN

A REPLY TO MR. GERRIT S. MILLER

By W. P. PYCRAFT, F.Z.S., A.L.S., Memb. Roy. Anthrop. Instit.
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SOME time ago Mr. Gerrit S. Miller of the Smithsonian Institute, Washington, was requested by Dr. Aleš Hrdlička, of the United States National Museum, to compare a set of casts of the skull of the Piltdown man, supplied by the British Museum, with the skulls of anthropoid apes. This comparison was undertaken with the perfectly legitimate purpose of arriving at an independent opinion as to the conclusions arrived at by Dr. Smith Woodward, and other British anthropologists, as to the nature of the Piltdown remains. The result of that inquiry has been to convince Mr. Miller that the cranial portions of this now famous skull are unequivocally human, while the lower jaw is as certainly that of a chimpanzee.[1]

In his summary[2] he tells us that "The Piltdown remains include parts of a brain-case showing fundamental characters not hitherto known except in members of the genus *Homo*, and a mandible, two molars, and an upper canine showing equally diagnostic features hitherto unknown, except in members of the genus *Pan*.[3] On the evidence furnished by these characters the fossils must be supposed to represent either a single individual belonging to an otherwise unknown extinct genus (*Eoanthropus*) or to two individuals belonging to two now-

[1] Since this was written Dr. W. K. Gregory, of the American Museum of Natural History, has published a memoir—*Studies on the Evolution of the Primates*—in which he throws the weight of his authority on the side of Mr. Miller; but since he accepts Mr. Miller's arguments without investigating the matter for himself, the objections herein set forth to Mr. Miller's contentions apply equally to Dr. Gregory.

[2] "The Jaw of the Piltdown Man," *Smithsonian Miscellaneous Collections*, vol. 65, No. 12.

[3] When Mr. Miller speaks of the genus *Pan* he means the genus *Simia*.

existing families (Hominidæ and Pongidæ). . . . In order to believe that all the fragments came from a single individual it is necessary to assume the existence of a primate differing from all other known members of the order by combining a brain-case and nasal bones possessing the exact characters of a genus belonging to one family, with a mandible, two lower molars, and an upper canine possessing the exact characters of a genus belonging to another. Thus must be associated in a single skull: (*a*) one type of jaw with another type of glenoid region, (*b*) one type of temporal muscle origin with another type of temporal muscle insertion, (*c*) a high degree of basicranial adjustment to the upright position with absence of that corresponding modification in the lower jaw called for by all that is now actually known of the structure of the brain-case and mandible in primates, and (*d*) a protruding lower jaw with a form of nasal bone not elsewhere known except in connection with a contracted upper dental arch. In each instance the opposed characters are sharply defined and easily recognisable in the fossils. . . ."

It is difficult to reconcile these very dogmatic statements, which have a semblance of statements of fact, with Mr. Miller's remarks in the opening passages of this summary: "The fossils are so fragmentary that their zoological meaning will probably remain a subject of controversy."

There does not seem to be much room for doubt about the "zoological meaning" of these remains; indeed Mr. Miller, in his main thesis, is very emphatic in every statement he makes thereon. He creates an impression indeed of having gone very thoroughly into the matter, with a wealth of material at his disposal enabling him to demolish completely the now generally accepted views as to the character of the skull of the Piltdown man. A very brief study of his arguments will show, however, that they are based on assumptions such as would never have been made had he not committed the initial mistake of overlooking the fact that these remains—which, by the way, he has never seen—are of extreme antiquity, and hence are to be measured by the standards of the palæontologist rather than of the anthropologist. This unfortunate lack of the right perspective has caused him to overlook some of the most significant features of these remains, and has absolutely warped his judgment in regard to the relative values

of the likenesses between these fragments and the skulls of the chimpanzee which he has so woefully misread.

When Mr. Miller commits himself to the statement, " Thus must be associated in a single skull : (*a*) one type of jaw with another type of glenoid region," he implies that there exists a relationship between the mandible and the glenoid cavity which has no existence in fact. With the whole mammalian phylum to choose from he will seek in vain for data which will enable him to foretell, by an inspection of the glenoid cavity alone, what was the form of the jaw articulating therewith. What Mr. Miller appears to mean is, that because the glenoid cavity of the Piltdown man is of the type characteristic of modern man, *therefore* the mandible must be in keeping therewith, that is to say it must possess a "chin," and must be horse-shoe shaped. This does not in the least follow, and there is less reason to expect it in the present case because the cranium has *not* lost all its primitive characters, though he apparently assumes the contrary. What he really means becomes apparent when his remarks on p. 17 are compared with his indictment under heading " (*c*)," where he protests that it is impossible to associate in this skull " a high degree of basicranial adjustment to the upright position " with the " absence of that corresponding modification in the lower jaw called for by all that is now actually known of the structure of the brain-case and mandible in primates." In the first place there is good reason for believing that the Piltdown man had not yet fully attained to the upright position, agreeing in this respect with the Heidelberg man. In the second the mandible does *not* present " the *exact* characters of a genus belonging to another family." The " absence of mechanical unity " between the mandible and the postulated face of this skull referred to on p. 17 is a purely imaginary absence. The jaw of the Piltdown skull, applied to a recent human skull of the same length as the Piltdown skull—of a Torres Straits Islander in the British Museum, to be precise—projects, at the incisors, no more than 3 mm.! Mr. Miller proposes to hang a lot on 3 mm. if he insists on his interpretation. As a matter of fact the Piltdown skull, even with this jaw, was less prognathous than in many modern men. This much is demonstrated by the restoration of the skull made in the American Museum of Natural History,

New York. For this has a facial angle of no more than 85°, which harmonises completely with the alveolar index of 105. In the British Museum are several skulls with facial angles ranging from 88° to 90°, and an alveolar index of as much as 110. Nor does it follow that because the toothrow in the Piltdown jaw was straight, as in chimpanzees, *therefore* the rami must have run parallel with the sagittal plane. The relation to this plane is determined, not by the toothrow, but by the intercondylar width. In megadont examples of modern man a straight toothrow frequently occurs, as I shall show later. His contention that the nasals, which he agrees to accord to the Piltdown skull, cannot possibly be associated with this jaw, is, like the rest of his argument, but a crude deduction founded on false premises. So much for his summary. Now for his details.

I will begin with the cranium. As to the human character of this there can be no two opinions. Mr. Miller himself is very emphatic on this point, building thereon much of his case against the human character of the jaw. He has missed, however, some of its most interesting features owing to his obsession in regard to the mandible. In the squamosal or "temporal," for example, the bone which he has designated as the type of *Eoanthropus*, he has failed to appreciate the fact that it is remarkable as much for its negative, as for its positive, characters. Having regard to its undoubted age one would have expected to find the simian spur on its anterior border which articulates, in all the apes, and in monkeys, and in many of the lower human races, with the frontal. We should also have expected to find a shallow glenoid cavity, as in the anthropoid apes, and many of the lower human types. Instead, this fossa is of most unusual depth, so much so that I have found but few modern skulls displaying a like depth. The *eminentia articularis*, on the other hand, is not only very extensive, but unusually oblique, which again is a peculiar character. In the form of the petrous portion it is most emphatically human, but here again it is peculiar, since it exceeds in length that of modern skulls by no less than 10 mm., giving a total breadth to the skull of no less than 20 mm. in excess of modern skulls. That Mr. Miller has entirely missed this feature is shown by a footnote (p. 17 of his memoir) in which he remarks that ". . . it is perhaps not safe to assume that the distance

from one glenoid cavity to the other was as great as in recent *Homo*. . . ." As a matter of fact, as we have just pointed out, the skull of *Eoanthropus* must have been no less than 20 mm. wider than in modern *Homo*!

He is no less unfortunate in his interpretation of the temporal fossa, and in the inferences he draws from that interpretation. "The anterior border of the (temporal) muscle," he remarks, "appears to have extended upwards on to the frontal with somewhat unusual abruptness, an impression that may be heightened by the way in which the bone is broken." There is no room for doubt in this matter. The anterior border of the muscle *did* extend upwards with "unusual abruptness." The way in which the bone is broken has nothing whatever to do with the matter. A line drawn from the bregma to the external orbital process and compared with a similar line drawn across any modern human skull will at once demonstrate this point. But the abrupt trend of this portion of the temporalis fossa is very closely approached in many modern skulls of low races. What is unusual about this arc is due in part to the lowness of the cranial roof, and in part to the shortness of the frontal; which brings the coronal suture nearer to the upward limb of the arc in question. The backward extension of this muscle Mr. Miller has, I venture to think, underestimated, and he has failed also to notice the peculiar sinuous curve of its superior border. Furthermore, it is surely unfortunate to contrast the area of this muscle with that of the great apes in relation to the size of the *animal*; such a comparison should surely be between the size of the *head* in the two groups. But in any case he is inaccurate as well as inopportune in his statements in regard to this muscle in the great apes. According to him, in *Simia*, or *Pan* as he prefers to call this genus, the temporal muscle meets its fellow in the middle line in adult individuals. This is true only of some males, belonging to some species, or subspecies. But even here the comparison lacks point. The muscle succeeds in reaching the middle line only because of the small size of the brain-case, but that factor he leaves entirely out of consideration.

When he comes to treat of the area of insertion of this muscle his distortions of fact could not well go further. The form of the coronoid process and the ascending ramus of the

mandible, he insists, are so like what obtains in the chimpanzee that that likeness can be due only to one factor—the pull of a temporalis muscle such as obtains to-day in living chimpanzees, a muscle, he is at great pains to show, which differs profoundly from that of the human temporalis which covered the parieties of the skull of the Piltdown cranium. In his anxiety to prove this point, he proves too much, for if his main argument is sound, then the ascending ramus and coronoid process must present uniform characters in all chimpanzees. A very cursory survey of a number of jaws of these animals will suffice to demonstrate the baselessness of such an assumption. The form of the coronoid process, and of the ascending ramus, in regard to its width, is apparently governed by the size of the fossa bounded externally by the jugal bar and internally by the cranial wall. Where this fossa is long and wide, relatively to the length of the skull, the ascending ramus is wide, and the coronoid process is low. But the form of the sigmoid notch varies also, as a glance at the accompanying illustration (fig. 1) will show. It is clear, therefore, that Mr. Miller has placed a quite exaggerated importance on the likeness which seems to obtain between the form of the coronoid process and the sigmoid notch in the Piltdown skull and that of the two chimpanzees which he figures in his memoir. That likeness, by the way, is so close that I venture to suspect that it is largely due to that process of "mutilation" to which he tells us he has submitted these jaws in order that they may be made comparable to the Piltdown jaw. I have had the good fortune to examine far more jaws of chimpanzees than has fallen to the lot of Mr. Miller, and only in one instance have I come across even an approximate likeness. But the case is otherwise when a comparison is made with the jaws of prognathous human races, where a short and wide ascending ramus is common. The jaw of the Kaffir (fig. 1) well illustrates this point. But even if the likeness between these jaws is occasionally as close as Mr. Miller maintains, it adds but little to the strength of his arguments. In an investigation of this kind we have to take the sum of a large number of differences and resemblances; we cannot base far-reaching conclusions on trivial characters, such as he so often selects. Mr. Miller indeed has staked well-nigh everything on his interpretation of the coronoid process. This is unfortunate for his case, for

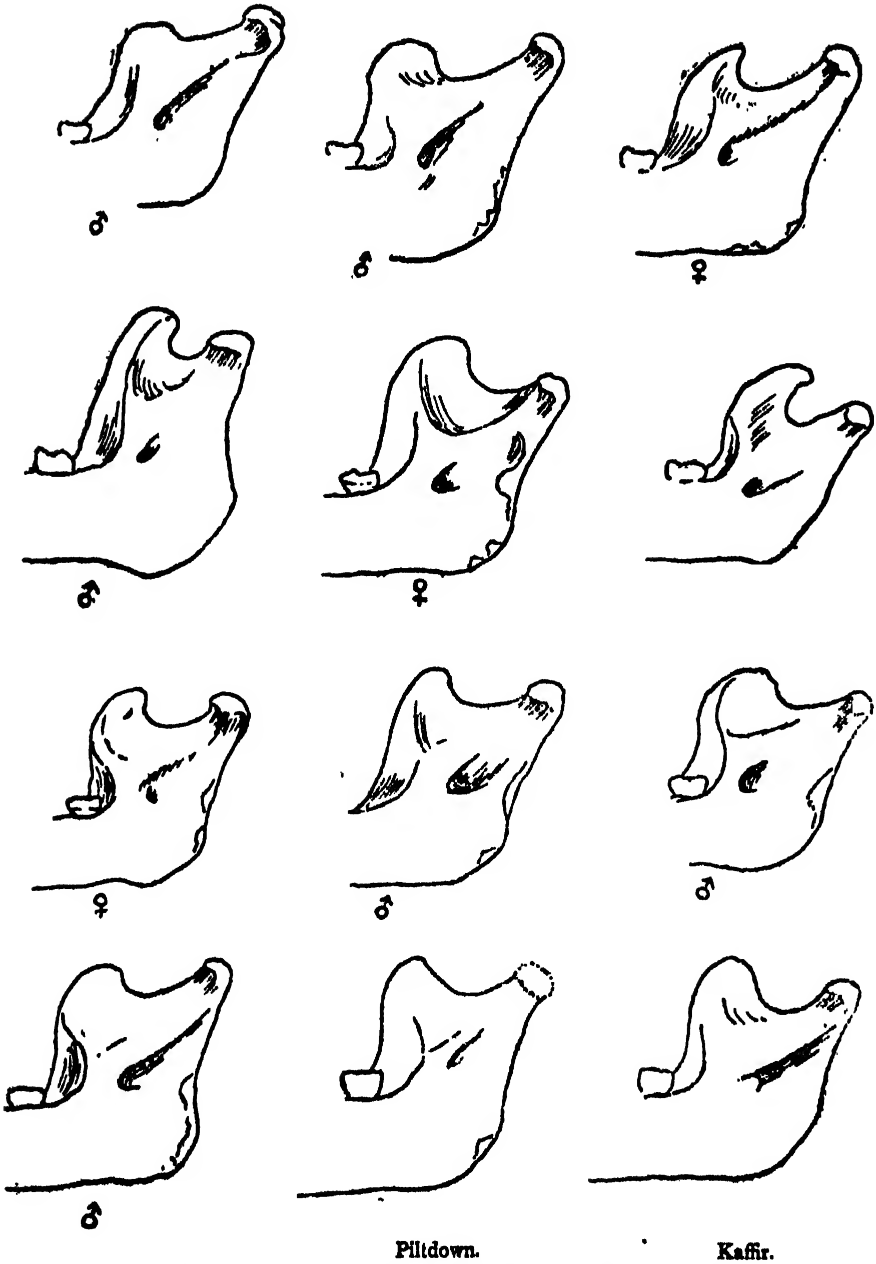


FIG. 1.—The ascending ramus of ten chimpanzee jaws, seen from the inner side, and compared with the same region in the Piltdown jaw, and that of a Kaffir. The extreme variability of the sigmoid notch, and of the size and shape of the coronoid process, show that no value whatever can be placed on inferences as to the size and shape of the temporalis muscles inserted here. The Piltdown jaw more nearly resembles that of the Kaffir than that of the chimpanzee.

if the form of this process reflects so intimately the form, and mass, of the temporalis muscle that he can tell from its inspection in the Piltdown jaw what was the precise character of the temporalis inserted thereon, then the coronoid process of male chimpanzees' jaws in which the temporalis extended to the middle line of the skull should be readily distinguished from jaws in which this muscle failed to reach the median line, as in some males, and most female chimpanzee jaws. Yet such discrimination is impossible. On the contrary, the precise factors which mould the form of the coronoid process and of the sigmoid notch so far defy discovery, if only because of the extreme variety which they present. Typically, in the chimpanzee, the sigmoid notch, on which the form of the coronoid process largely depends, runs directly downwards and forwards, in a straight line from the condyle to the very base of the process in question, which commonly ends in a hook-shaped and backwardly directed summit. In some individuals, however, this notch is excessively deep and narrow, when the coronoid process closely approaches the condyle, as a glance at the accompanying figures will show. A notch of this latter type seems always to be associated with a very small jugal fossa. But save for this apparent correlation between the form of the jugal fossa and the ascending ramus of the mandible, no other governing factors in these extremely variable features seem discoverable. The dissimilarities of the ascending ramus of the mandible of the chimpanzee is remarkable, having regard to the restricted area of distribution of this animal. To find its parallel among human jaws all the races of the world must be sampled.

Great stress is laid on the significance and importance of the "secondary ridgelets" associated with the "ridge which extends upwards from the base of the coronoid process which gives extreme strength of attachment to the muscle fibre. This stage of roughening on the mandible is associated in the chimpanzee with the closest approach of the upper end of the muscle to the median line of the brain-case and especially with the formation of a sagittal crest. It is well marked in the Piltdown jaw." These very circumstantial statements have no support in fact, at any rate in any of the specimens I have examined. The "ridgelets" are nowhere very obvious, and are no better developed in male than in female jaws, and

they are *not* present in the Piltdown jaw. The "highly developed strengthening ridge characteristic of the genus *Pan*" which runs downwards and forwards from the mandibular condyle to terminate above the dental foracen, though a common, is by no means a constant, feature of the chimpanzee jaw, and in some modern human jaws is almost as well marked as in the Piltdown jaw. Nor is the presence of the mylohyoid ridge a constant feature of the human jaw; in some, especially in low races, it may be barely perceptible. Below the missing condyle Mr. Miller insists the hinder border of the jaw "shows more lateral compression" than he has been able to find in any specimen of *Homo*. This failure can only be attributed to the limited number of human skulls to which he has access, an inference which also accounts for his apparent impression that the pterygoid fossa "immediately below the articular surface of the condyle is an invariable feature of the human jaw." It is not, and furthermore it is present in some chimpanzee jaws. Mr. Miller finds no indication of this fossa in the Piltdown jaw. Surely this is not a matter for surprise, having regard to the fact that the whole of the area in which this fossa is lodged is missing! Finally the form of the glenoid cavity of the squamosal or "temporal" bone affords but an indifferent guide to the form of the mandibular condyle, as a very cursory examination of a number of human crania will suffice to demonstrate. In some modern skulls this cavity is as deep as in the Piltdown skull, in others well nigh as shallow as in the chimpanzee, but there is no corresponding modification of the condyle.

Mr. Miller is always most emphatic where contradiction is most difficult, as for example when he elects to enlarge upon the character of missing elements in this jaw. Nowhere is this more apparent than in his comments on the missing condyle, but even here he is inconsistent. For on page 14 he remarks that "The articular process is worn off to the level where it begins to widen to form the base of the condyle." Yet, on the very next page (p. 15), he would have it believed that at least a portion of the condyle remains. This is the only interpretation possible on his statement, "Hence in order to fit its articulating surface to that of the skull it would be necessary to imagine an abrupt change of plan in the *few millimetres of condyle* that have been lost" (italics mine). What

does Mr. Miller mean by a " few millimetres " ? Certainly not less than 12, and probably 15 millimetres are missing. And this amount amply suffices to show that the support for the glenoid surface of the condyle, when restored by producing the parieties of the mandible upwards, takes the contour characteristic of the average human jaw much more readily than of the average chimpanzee jaw, which, by the way, commonly shows extremely prominent ridges and tubercles, for the

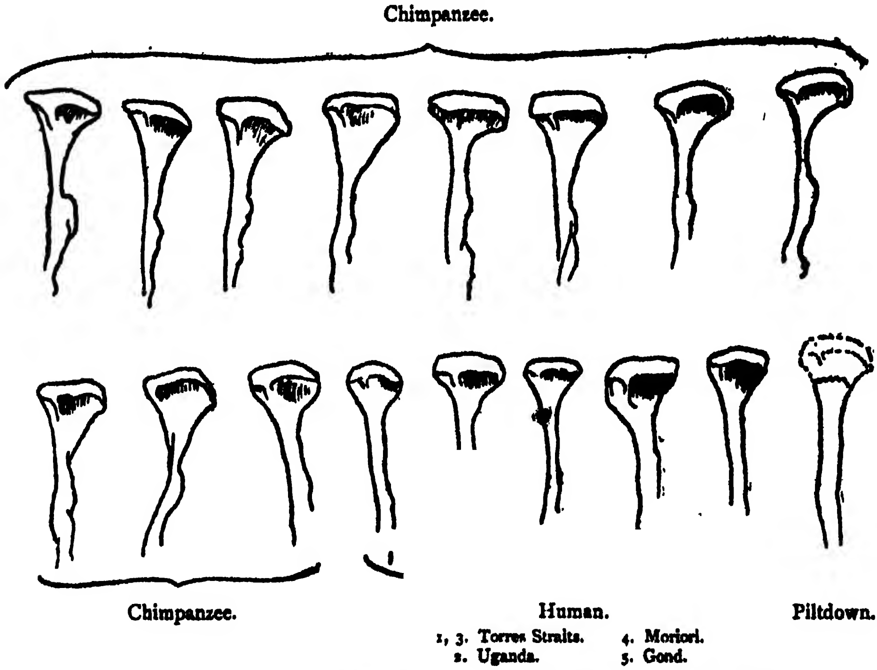


FIG. 2.—Outlines showing the contours of the posterior border of the ascending ramus, and of the condyle, in the jaws of chimpanzees and in human jaws. The condylar end of the sigmoid notch is just indicated in each case. It is clear that the Piltdown jaw more nearly conforms to the human than to the chimpanzee type.

insertion of the internal pterygoid muscle (fig. 2), though jaws in which these ridges are but slightly developed are also found. They are never so prominent in the human jaw, and they are barely perceptible in the Piltdown jaw.

In regard to the comparisons drawn between the jaws of chimpanzees and recent human jaws, and set forth in a Table of Measurements on p. 20 of Mr. Miller's memoir, there are several points which call for comment. Thus, in com-

paring the length of the mandible at the alveolar level he gives 101·8 mm. as the maximum length in recent *Homo*. In the collections under my charge at the British Museum are jaws which, in this particular, range from 101 mm. to 115 mm., the latter the jaw of a Torres Straits Islander, that is to say only 5 mm. less than the Piltdown jaw. The distance from the posterior border of the mandible to the front of M1 in recent *Homo*, according to Mr. Miller, attains its maximum at 75·6. In the British Museum collections we have recent human jaws ranging from this up to 88 mm., the highest being that of a Moriori. The maximum diameter of recent *Homo* of the ascending ramus at the alveolar level in this table is given as 46·8 mm. In the British Museum are jaws ranging up to 50 mm. In the chimpanzee, according to Mr. Miller, the maximum is 52 mm. Jaws which I have measured in the collection which Lord Rothschild has generously placed at my disposal range from this up to 55 mm. In Mr. Miller's table it will be noticed that his figures are all calculated to accentuate the differences between the Piltdown man and modern man, and the agreement with the chimpanzee. These revised figures lend no support to Mr. Miller; on the contrary, they place the Piltdown jaw well within the limits of human variation.

And now as to the teeth. "The two molars," we are told, "show no indication of the beginning of a curve in the tooth-row. The main axis of the first tooth is continued by that of the second in a line passing as far to (the) inner side of (the) condyle as in the Pongidæ. In front of the first molar the entire hinder border of the alveolus of pm2 is plainly visible. It shows that the missing tooth was fully as large as in the great apes, and that the toothrow did not become sharply weakened at the point where this conspicuous change takes place in all known Hominidæ." These statements are to be read with his earlier arguments on p. 5. Here he tells us that "The toothrow in the Hominidæ is narrowed and weakened in front of the molars, the change taking place abruptly with (the) posterior premolar. Each premolar is single rooted, and the crown area is less than half that of the first molar. . . . Among the great apes the robust character of the toothrow is carried forward through the large, double-rooted premolars to the strongly functional canine. . . . A line joining the middle

of (the) posterior of m2, with the middle of (the) interior border of m1, will if continued forward in front of (the) incisors, converge rapidly with the sagittal line similarly extended. In the great apes . . . a line passing through the middle of (the) posterior border of M2 and the middle of (the) anterior of m1 is essentially parallel to the sagittal line. In the Hominidæ the inward curve of the toothrow normally begins with the first lower molar. The axis of this tooth prolonged backwards diverges rapidly from a line parallel to the sagittal plane and crosses the posterior of m2 on (the) outer side of (the) middle: continued still further it passes through the condyle. That of the second tooth similarly prolonged, while diverging slightly from a line parallel to the sagittal plane, passes considerably to (the) inner side of (the) condyle. In all living genera of great apes, and in the fossil *Propliothecus*, *Dryopithecus*, and *Sivapithecus*, the axes of the two teeth lie in one line essentially parallel to the sagittal line, and passing further to (the) inner side of (the) condyle than is the case with the axis of m2 in the Hominidæ." The reference to the sagittal plane must be ignored since, according to Mr. Miller, the Piltdown jaw is that of an ape; regarded as a human jaw the greater intercondylar width of necessity causes a wide divergence from the median plane of the condyles.

The foundations of this argument are very insecurely laid. To begin with, there is no justification for the assumption which Mr, Miller makes, that the toothrow in chimpanzees is invariably straight. It is indeed commonly so, but an examination of a sufficiently large series of jaws will show that in some the molars are ranged along a backward and inward curve, and such instances I have reason to suspect are indicative of racial differences. But accepting Mr. Miller's interpretation, the application thereof fails, if only because the toothrow in human megadont jaws is also, commonly, as straight as in chimpanzees. Nor is there any justification for the statement that in the chimpanzee jaw the "robust character of the toothrow is carried forward through the large double-rooted premolar to the strongly functional canine," while in "all known Hominidæ" the toothrow becomes "abruptly weakened" by the conspicuously inferior size of the premolars; for in some chimpanzee jaws the premolars are emphatically small, while in megadont human jaws they may be of large size.

As to the statements in regard to the axes of the molars, in relation to the condyle, they are not only inexact in regard to the chimpanzee, but they are equally inapplicable to the human jaw; since in megadont jaws the axis of m1 passes directly backwards, and is coincident with the axes of m2 and m3, as in the cases where this applies to the chimpanzee. Further, the common axis of the molars of the chimpanzee does not invariably, if produced backwards, pass further to the inner side of the condyle than in the Hominidæ. On the contrary, in some chimpanzee jaws this axis takes the same course as in the human jaw, that is to say, it touches the condyle. Mr. Miller's statements in regard to these relationships thus show either a very superficial acquaintance with the morphology of the human jaw, or a habit of basing far-reaching conclusions on ill-digested data.

Not the least characteristic feature of the teeth of the Piltdown jaw is their state of wear, the cusps having been worn down so as to reduce the crown to a perfectly level surface. Mr. Miller tells us that he has found teeth in the jaw of a chimpanzee worn in a precisely similar manner, and he gives a photograph purporting to bear out this statement. Yet no impartial critic will agree that this photograph in the least supports his statement. Of all the teeth which I have examined, and these represent at least twice as many as Mr. Miller has examined, I have failed to find one which can in any way be compared to these teeth of the Piltdown jaw in this matter of wear. But be this as it may, the determination of the generic identity of this jaw does not turn upon the state of wear of the crowns of the teeth, but upon a number of characters of far greater importance. Nor does their size affect the question, though according to Mr. Miller this is the only character by which they can be distinguished from the teeth of the chimpanzee, which, he asserts, are never so large as in the Piltdown jaw. This is not so. I have recently found teeth in the jaw of a chimpanzee from the Belgian Congo which are quite as large as those of the Piltdown jaw.

The molars of the Piltdown man, materially reduced as they are by wear, are still much more hypsodont than any chimpanzee teeth which I have yet seen, and this is true even if these worn teeth are compared with unworn chimpanzee teeth, which, by the way, are not, as Mr. Miller seems to suppose, of

so uniform a character that observations made on one jaw will apply to all chimpanzee jaws. As a matter of fact, variety in the details of the contour of the crowns, and in the size of the cusps, as well as in the actual size of the teeth, show as great a range as in human jaws. Commonly the long axis of the lower molars of the chimpanzee exceeds that of the transverse axis, as is also the case in some modern human jaws, but it is by no means unusual to find teeth wherein these axes are equal, as is the rule in modern human teeth.

Even where the teeth of the chimpanzee are as large as those of the Piltdown jaw, and such cases are rare, there is no difficulty in distinguishing between the molars of the ape and those of the man. In the Piltdown jaw the protoconid, metaconid, and hypoconulid are conspicuously larger than in those of the largest chimpanzee tooth, and these differences are still more marked in the case of the normally smaller teeth characteristic of the chimpanzee. Furthermore, the sulci dividing the cusps one from another are longer, and far more conspicuously marked, than in any human teeth, including the teeth of the Piltdown jaw (fig. 3, B). The widest part of the crown, in the chimpanzee tooth, is immediately above the roots, the grinding surface being conspicuously less in diameter. In human teeth, including the teeth of the Piltdown jaw, the crown passes almost insensibly into the root, and is not perceptibly wider or longer at its base than at its grinding surface; the reverse is the case with the molars of the chimpanzee. It is idle indeed to pretend that the molars of the chimpanzee are indistinguishable from those of the Piltdown jaw. As Prof. Keith has already remarked, radiographs of the Piltdown jaw show that they are of the typical "taurodont" type, therein differing conspicuously from the molars not only of the chimpanzee but of all the great apes.

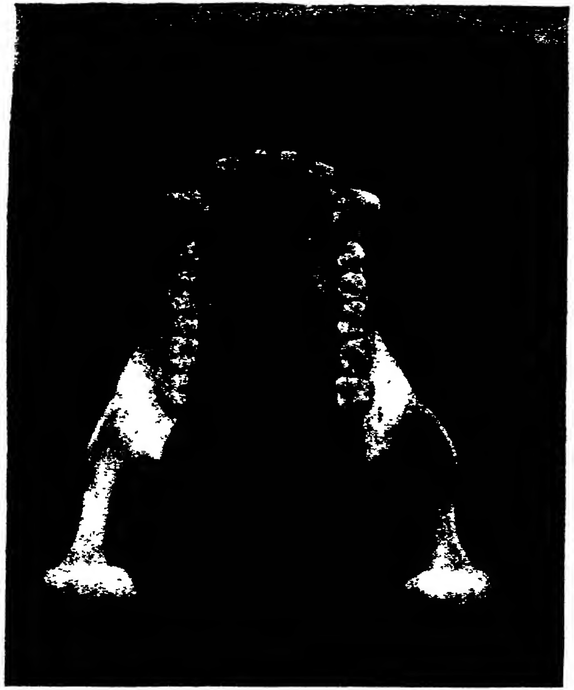
A careful study of the unworn chimpanzee molars carried on side by side with human molars, also unworn, will speedily show yet other important differences between them. Those of the chimpanzee differ, for example, emphatically from all human teeth in having the external cusps—protoconid, hypoconid, and hypoconulid—sharply defined by deep sulci (fig. 3, B), and this is true even where the hypoconulid is degenerate. The contours of the grinding surface at their circumference vary so widely that no importance can be placed thereon in this

connection. But all chimpanzee teeth agree in that the protoconid and metaconid are united by a broad ridge showing a steep face posteriorly, and a deeply grooved upper surface (fig. 3, B). This is a feature which never occurs in human teeth where the ridge slopes gradually to the bottom of a cruciform valley, while in chimpanzee teeth the centre of the crown is occupied by a cup-shaped depression. Traces of this depression are to be found in some megadont human jaws, apparently also in the Piltdown jaw. Traces of the groove at the top of the ridge connecting the protocone with the metacone occur commonly in human molars. The cruciform valley, characteristic of the human molar, cuts the crown into four subequal moicties: this is never the case with the chimpanzee molar; since when lines answering to these valleys are drawn across the tooth, the transverse crosses the longitudinal axis obliquely.

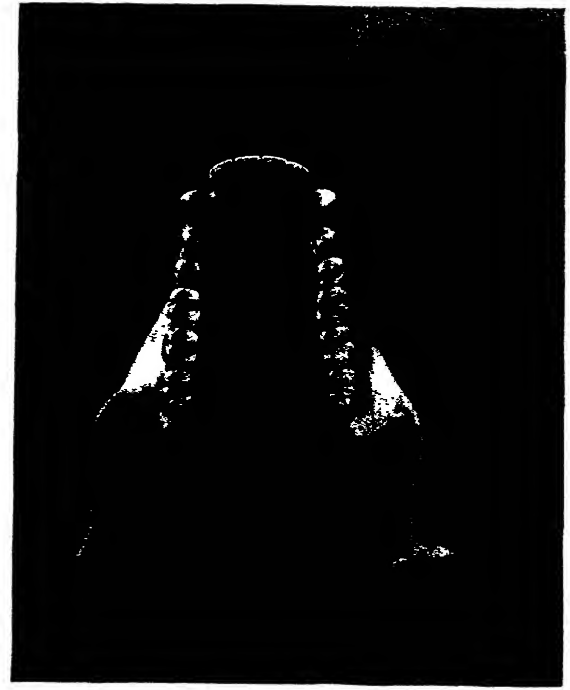
Not the least characteristic feature of the worn surface of the teeth is the extreme thinness of the enamel, wherein they differ most emphatically from the teeth of the Piltdown jaw and from modern human teeth. Yet, curiously enough, Mr. Miller brushes this point aside as of no importance. And no less characteristic is the condition of the grinding surface of the crowns of worn molars of the chimpanzee. Contrary to Mr. Miller, I have in no single instance yet found these teeth with the surface worn to a perfectly level table at right angles to the vertical axis of the tooth. This is a common feature of human teeth, and is most conspicuous in those of the molars of the Piltdown man. In those of the chimpanzee, on the contrary, the outer sides may be worn down to the very base of the enamel while the cusps of the inner side are reduced to no more than half their original height. That the molars of the chimpanzee should occasionally present a perfectly flat surface need occasion no surprise, since, on the other hand, human teeth occasionally present the obliquely worn surface characteristic of the Chimpanzee, as for example in the case of the Moriori and some ancient Egyptians. This being so, the extremely flat surfaces of the crowns of the Piltdown molars should have given Mr. Miller occasion for reflection, having regard to the rule which undoubtedly obtains among the chimpanzees in this particular. At least it should have occurred to him that the wear of the teeth

was no safe guide to the form of the glenoid cavity, since it is clear that a precisely similar result of wear may accrue whether the glenoid cavity is deep, as in the human skull, or shallow, as in the chimpanzee.

And now as to the canine. Mr. Miller's arguments to prove that Dr. Smith Woodward is entirely wrong in his view that this is the right lower canine are founded upon assumptions for which no warrant can be found on an appeal to fact. "In all living great apes," he remarks, "the postero-internal surface of the lower canine is convex." This is true only of the tooth in transverse section: in a longitudinal section it is concave. Nor is he more happy in his statement that "No matter how long a lower canine may have been in use it never assumes the form seen in *Eoanthropus*, nor does it lose all trace of the original convexity of its inner portion." Mr. Miller's experience of worn canines in the chimpanzee is evidently limited, or he would not have failed to find examples which flatly contradict him when he says that all traces of the original convexity of the inner surface of the tooth are never lost. How largely he has substituted "intuition" for investigation is shown by his assurance that "a mechanical inter-relation of the teeth, such as would produce a worn surface" presenting a wide shallow concavity directly backwards and inwards, "is not only unknown among primates, but (in a lower canine) I have never been able to find any mammal with the upper and lower teeth so arranged that it could exist." This may very well be true, so far as Mr. Miller is concerned, but in the British Museum are two chimpanzee jaws (1.8.9.4. and 1.8.9.10.), in the collection of Lord Rothschild is a third, and in the Royal College of Surgeons' Museum is a fourth, achieving Mr. Miller's standard of the impossible. In the British Museum specimens the worn surfaces, however, present a slight twist, recalling that of a propeller, but in that in Lord Rothschild's museum the resemblance to the Piltdown tooth is extraordinarily close. In studying the worn surfaces of these teeth in the great apes the aspect which most impresses one is not the similarity, but the remarkable dissimilarity, they present, a phenomenon due to variations in their size, and the angle at which they stand in the socket, for these vary the incidence of wear by the opposing teeth. In some cases attrition is caused, not by the



A



B

FIG. 3.—Jaws of Chimpanzees, showing the lateral torsion of the alveolar border, which carries the teeth obliquely across the body of the jaw.

In *A* [No 61. 7. 29. 10. Brit. Mus. Coll.] the mesial aspect of the ramus, from the level of M2, is much inflated, forming a conspicuous lateral cushion meeting behind the incisors and overhanging a spacious "genial pit. In *B* the "lateral cushion" is indicated merely by a slight bulging of the inner wall of the mandible. The symphysis presents a steep face, thereby considerably reducing the size of the genial pit. The molars in this jaw are slightly larger than in the Piltdown jaw. This photograph is from a specimen belonging to the Belgian Government for the Congo.

upper canine, but by the lateral incisor. Hence it is that in no two canines, even in the same jaw, are the results of wear the same. This being so, even if the Piltdown jaw were indubitably that of a chimpanzee, it would be unreasonable to expect to find its exact counterpart in modern chimpanzees. There is nothing, in short, in the character of the worn surface of this tooth which is inconsistent with the position in the lower jaw which has been assigned to it.

That this canine is *not* that of a chimpanzee is surely abundantly proved by the fact that it lacks all traces of the "heel" at the postero-external angle of the base, as was pointed out by Dr. Smith Woodward, as well as of the "cingulum" which is present in the canine of all chimpanzees, in some examples of which it is extremely well developed.

While I am convinced of the correctness of Dr. Smith Woodward's interpretation as to the cause of the wear in the Piltdown tooth, the worn surface of the teeth in the jaw in Lord Rothschild's collection, already referred to, lends colour to the contention of Prof. Keith that this wear was caused, not by the upper canine, but by the upper lateral incisor. That, however, is a matter of detail of no great importance.

Mr. Miller may urge that my arguments do not affect his case in the least, since he associates this tooth with the upper, and not with the lower jaw. But there is, if possible, even less to support him in this view. Nevertheless Mr. Miller is not alone in this association, but it is almost incredible that it should have received serious consideration. Those who hold this opinion, indeed, do so either because they have not had an opportunity of studying the actual tooth, or from a wholly insufficient examination of the evidence afforded by the canine of the chimpanzee.

In the first place the root of the Piltdown tooth, both in its curvature and in its section, is quite unlike what is demanded of an upper canine, since it has a strong outward and only a very slight backward curvature, and presents a very conspicuous flattening throughout its entire internal aspect. In the second place the condition of its worn surface is quite inconsistent with a position in the upper jaw. In all the jaws of chimpanzees which I have examined the wear of the upper canine, where it is at all comparable to that of the Piltdown

tooth, ends abruptly, forming an overhanging ledge at the base of the tooth, whereas in the Piltdown tooth it presents a continuous curve. But, further, the worn surface of the Piltdown tooth looks backwards and outwards, a form of wear impossible in an upper canine, while it is demonstrably possible in a lower canine. The jaw in the College of Surgeons already referred to (No. 1, Du Chaillu Coll. Gaboon, Male) shows a lower canine which in this matter of wear is almost identical with that of the Piltdown tooth; it differs, indeed, only in that its posterior external edge is slightly worn apparently by the upper canine, the rest of the surface seeming to have been worn by the upper lateral incisor. The upper canines of this skull have been broken and worn to mere stumps, yet showing a form closely resembling the upper canine postulated by Dr. Smith Woodward in his restoration of the Piltdown skull.

Two points of paramount importance in the conformation of the Piltdown jaw have been entirely overlooked by Mr. Miller. The first of these concerns the conformation of the inner surface of the body of the jaw. In chimpanzees this shows very striking differences, forming, when extremes are compared, two well-marked types (fig. 3). In the one (fig. 3, A) the inter-ramal area, from the symphysis backwards as far as the level of M2, has a curiously inflated appearance, so that the teeth seem to arise from a cushion-like bed, which, anteriorly, overhangs the genial pit. In the other this inner wall may dip downwards from the teeth almost as abruptly as in human jaws (fig. 3, B). But even here the differences between the human and the chimpanzee jaw are readily apparent, since in the chimpanzee this inner wall shelves downwards, and inwards, towards the symphysis, which is always much deeper, and never so steep, as the human jaw. As to the actual symphysis in the Piltdown jaw no positive statements can be made, but there can be no possibility of doubt about the human character of the whole region in question which has been preserved. Mr. Miller's photographs of the inner aspects of the jaws, which he uses to demonstrate the community of descent between the owner of the Piltdown jaw and modern chimpanzees, succeed only in demolishing the theory which he has been at such pains to elaborate. For one can see at a glance, by the high lights,

which are the jaws of chimpanzees and which is the human —Piltdown—jaw. Between the two extremes seen in the jaws of chimpanzees every gradation will be found, but in no case would there be any possibility of confusing the Piltdown fragment, or any similar fragment of a modern human jaw, with similar fragments of chimpanzee jaws. This character alone suffices to demolish the whole of Mr. Miller's arguments.

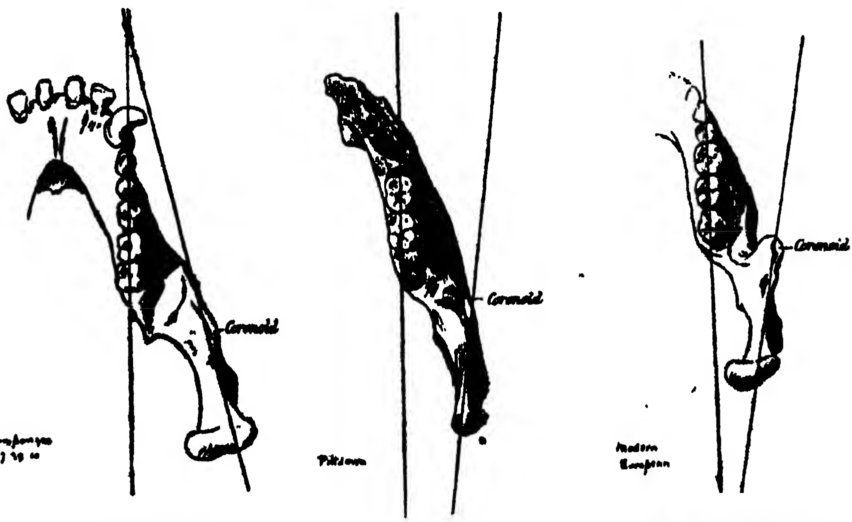
But there is yet another test as to the human character of the Piltdown jaw which I venture to think is the most convincing of all. Mr. Miller makes much apparent capital out of the fact that when the jaw of a chimpanzee is compared with that of a modern man it will be found that the toothrow from the canines backwards, in the chimpanzee, are parallel, while in man the molars along the two sides of the jaw diverge; and he supports his statements by means of diagrams. No one doubts the correctness of these; the facts which he points out have long been familiar to anthropologists. But this comparison is absolutely valueless in the present connection, since we have but one ramus of the Piltdown jaw. This alone, however, furnishes two extremely important characters. One of these has already been discussed. The other may be briefly stated as follows.

If, in the jaw of a chimpanzee, a line be drawn down the middle of the toothrow from the canine backwards, and another be drawn through the ascending ramus entering by the posterior border and passing out through the anterior border, it will be found that the two lines converge *in front* of the canine (fig. 4). If these lines be drawn along the toothrow, and through the ascending ramus, of the jaw of a modern man they will be found to converge at a variable distance behind the articular condyle of the jaw; they may in rare cases run parallel, but in no case have I yet found them converging in front of the canine as in the chimpanzee.[1] The Piltdown jaw agrees with that of modern man (fig. 4).

The interpretation of these differences is not difficult. In the chimpanzee the toothrows of the right and left rami en-

[1] The line through the ascending ramus can best be taken by means of a "straight edge" held immediately above the sigmoid notch, but not necessarily over the tip of the coronoid process, which, both in man and apes, may be deflected outwards. At the same time another "straight edge" should be held over the toothrow.

close a long, narrow, lingual space, so narrow that the ascending rami have to undergo a wide lateral extension in order to provide the intercondylar width necessary for the articulation of the jaw with the cranium. In the human jaw the teeth are arranged horse-shoe fashion, diverging rapidly posteriorly, hence the ascending rami have a relatively smaller lateral extension in regard to the toothrow. The fact, then, that the Piltdown jaw in this regard agrees with the modern human jaw suffices to show that in the arrangement of its



Chimpanzee. 61. 7. 29. 10. Piltdown. Modern European.

FIG. 4.—**Lines drawn along the molars, and through the ascending ramus, to show that in the chimpanzee they converge in front of the canine, while in the human jaw they converge at a variable distance behind the condyle. The human character of the Piltdown jaw, judged by this test, is unmistakable.**

teeth, and in the form of the enclosed lingual space, it differed as widely from that of the chimpanzee as does the man of to-day.

That the Piltdown jaw does present many points of striking resemblance to that of a chimpanzee is beyond dispute. Dr. Smith Woodward pointed out these resemblances long ago, in his original description of the jaw. But Mr. Miller contends that because of these resemblances therefore it *is* the jaw of a chimpanzee. It will be obvious, to those who will take the trouble to analyse the evidence wherewith he supports his arguments, that he has endeavoured, throughout, to confirm

a preconceived theory; a course of action which has unfortunately warped his judgment and sense of proportion. He has laboured under the further disadvantage of never having seen the original remains. This implies no reflection upon the accuracy of the casts supplied him from the British Museum, which were indeed most faithful reproductions, but it is humanly impossible to impart to plaster of Paris the subtle qualities of mineralised bone and enamel which count for so much in a task such as Mr. Miller set himself.

In order that I might do Mr. Miller full justice I have made a very careful study of the skulls of chimpanzees in the British Museum and the Royal College of Surgeons. These were supplemented by the fine collection in the museum of Lord Rothschild, which he generously placed at my disposal, sending them up to me at the British Museum to facilitate comparison. I therefore desire here to express my grateful thanks both to Lord Rothschild and to Prof. Keith for the very material help they afforded me.

Finally, I have submitted my objections to Mr. Miller's contentions to Dr. Smith Woodward, Prof. Elliot Smith, Prof. Arthur Keith, Prof. A. S. Underwood, and Dr. Broom, and with one accord they permit me to say that they are in entire agreement with me.

WHAT IS A DISEASE?[1]

By CHARLES MERCIER, M.D., F.R.C.P., F.R.C.S.

THE next question to determine is whether structural damage is necessary to the concept of disease. Do we, when we think of a disease, necessarily mean structural damage with its consequences, or may there be diseases in which no structural damage is known? Unquestionably there may. In many of the diseases, nay, in most of the diseases, considered as such by our forefathers, no structural damage formed part of the concept, or was known to exist. When cough, syncope, hæmoptysis, hæmatemesis, hæmaturia, albuminuria, were reckoned as diseases, they had no structural basis. In our own time, paralysis agitans and diabetes were known as diseases, though they had no known structural basis. It is true that in these cases, though no structural basis was known, yet it was assumed that there was a structural basis, and it was confidently anticipated that this structural basis would eventually be found; so that it may be contended that some structural basis, even if only conjectural or postulated, entered into our concept of these diseases; but this is not so, for there are some diseases, to which every one would allow the title, in which no basis of structural damage is suspected, and in which we feel confident that no such basis exists. In hysteria, for instance, in blindness and deafness from shock, in some cases of epilepsy, in some cases of insanity, we can affirm with confidence that no structural basis underlies the disease; and yet these are undoubtedly and indisputably diseases.

Although, therefore, in many cases the structural basis of the symptoms does enter into our concept of the disease, yet in others a structural basis forms no part of the concept; and in these cases nothing is left but symptoms. The symptom or symptoms then constitute the disease, and we are once more confronted with the problem of what it is in our way of contemplating a symptom or a group of symptoms that con-

[1] Continued from SCIENCE PROGRESS, October 1916.

verts it or them into a disease. Is it the combination of two or more symptoms? No, for in the whilom diseases cough, hæmaturia, palsy, dropsy, and in the present-day diseases writer's cramp, wryneck, neuralgia, Dupuytren's contraction, and others, there is but a single symptom; and moreover there are combinations of symptoms that do not constitute a disease. A patient may suffer simultaneously from the symptoms of heart-disease, ringworm, and hammer-toe, but we do not regard this combination of symptoms as a disease: we regard them as three different diseases. This instance throws some light upon our problem. It is clear from it that whatever symptoms a patient may present, they do not constitute a disease unless they are correlated together; that is to say, unless they are combined by a causal nexus into a single group. It may be that we can identify the tie that binds them together, as when we find symptoms so diverse as disorder of the circulation, asthenia, vomiting, and pigmentation of certain areas of the skin, all associated with and dependent on destructive disease of the supra-renals; or it may be that no such tie can be discovered, but yet the occurrence in different persons of the same distinctive group of symptoms, or their repetition from time to time in the same person, as in epilepsy, satisfies us that there must be a causal tie; but in either case, we take the whole of the correlated symptoms, and mentally combine them with the causal tie, and call the whole correlate THE DISEASE. The correlating tie which binds the group together in our minds may be structural damage of this organ or that, or it may be a microbic invasion, or it may be a poison, or it may be unknown, and purely conjectural; but some tie, some common intra-corporeal cause, must exist or be supposed to exist before we can group the several instances of disease together into an integrated whole or individual thing, and call that object of contemplation a disease. By a disease we mean, therefore, the whole group of correlated disorders, both functional and structural, from which the patient suffers; and by correlated disorders are meant all the disorders that are attributed to a single intra-corporeal cause, together with this cause.

It is necessary to add the qualification 'intra-corporeal,' for many diseases have a plurality of causes if we take into our consideration extra-corporeal causes. Many diseases need

for their causation some special action from without, such as the bite of an infected insect, faulty diet, cold, damp, or what not; but these are not parts of the disease. They do not enter into our concept of the disease. They are causes of the disease, but, being extra-corporeal, they are not parts of the disease.

1. This concept of a disease needs some further qualification before it can be accepted as complete and final. To recur to the combination of rheumatism, heart disease, embolism: these, as long as they co-exist, constitute, with their symptoms and other consequences, a single disease. They constitute a correlated group of disorders, structural and functional, all assignable to a single intra-corporeal cause, the rheumatic infection which correlates them and combines them into a single group, contemplatible as an individual whole. Nevertheless, each of them has a certain separateness and individuality of its own. The rheumatic infection may die out and disappear, and then the disease is no longer rheumatism: it is now heart disease complicated with embolism. Let us suppose that the heart disease also recovers and its structural integrity is restored: the disease is now embolism only. Or there may have been no embolism, and on the disappearance of the rheumatism the disease is heart disease only. Heart disease and embolism may, therefore, in some cases be sole diseases, complete diseases, separate diseases; but when they co-exist with each other, or with acute rheumatism, what is their position? Separate diseases they certainly are not, but may not each be regarded as a disease, complete in itself? In a sense, yes: in a sense, no. Each is a complete disease in the sense that it is a group of disorders correlated by a causal structural change sufficient to account for them all; but it is clear that when associated together, or with rheumatism, there is a further correlation, and thus our concept of the disease from which the patient suffers is not complete until they are all correlated into a single more comprehensive concept. The whole disease from which the patient suffers is rheumatism, complicated with heart disease and embolism; but these constitute subordinate correlations, which we may appropriately term *sub-diseases*. Such sub-diseases are not infrequent, and may be of such importance and danger to life as to compel us to concentrate our attention upon them, and to treat them, to the subordination, for the time being,

of the principal disease. Optic neuritis is the basis of a sub-disease of tumour of the brain; cirrhosis of the liver is the basis of a sub-disease of alcoholism; nephritis is the basis of a sub-disease of scarlet fever, and so on. The principal disease may subside, or its basis may be removed, and then it disappears, leaving outstanding the sub-disease, which then becomes the principal disease.

2. A disease, then, is the whole group of correlated disorders from which the patient suffers. If there is more than one such correlated group, the patient suffers from more than one disease; but how if there is but one symptom, and this not correlated with any assignable intra-corporeal cause? What is the nosological position of neuralgia, of asthma, of writer's cramp, of diabetes insipidus, of dry mouth, wryneck, and many other such disorders? We call these diseases, but are they rightly so called? Each of them is but a single symptom, uncorrelated with any other disorder, and due to no assignable cause. Are we justified in calling them diseases? We usually do call them diseases, but we do so with a certain reservation and a shade of doubt. Each of them satisfies the definition in as far as it is in the whole disorder from which the patient suffers; but is it a correlated group of disorders? for that is the definition of a disease. I think it is. I think that when we regard any of these disorders as a symptom, we limit our contemplation of it to what we observe; and we observe but one thing—one uncorrelated disorder; but when we regard the disorder as a disease we do not so limit our contemplation.

3. In the first place, we should not regard as a disease a single twinge of neuralgia, a single attack of asthma, a single rigor, a single manifestation of writer's cramp, or epilepsy. Each single attack or manifestation is a symptom only, or, if more than a symptom, is certainly less than a disease. The disease does not exist until the manifestation is repeated, for not until there are two things to bring into relation with each other can there be a correlation; and it is the correlation, or the group of correlatives, that constitutes the disease. In the second place, when the manifestation is repeated, we do not regard the repetition as fortuitous and accidental. We cannot help supposing that the two or more manifestations are connected together by some underlying cause or condition which

persisted in the interval between them. We may not know, and may not be able to conjecture, the nature of this underlying cause or condition, but it is impossible to help postulating its existence; and with this causal basis the manifestations are correlated: by it they are unified and constituted into a single thing—a disease. In the third place, if this basis is postulated to account for repetitions of a symptom, separated from one another by intervals, equally is it postulated when there are no intervals, and the manifestation or symptom is continuously present; or when, as in the case of a fatal first attack of angina pectoris, it cannot be repeated. Whenever we speak or think of a disease, we correlate what we observe with an intra-corporeal cause, known, conjectured, or postulated; and it is this correlated group that constitutes the disease.

For consider. Some disorders of function, both extrinsic and intrinsic, are their own symptoms. We witness the disorder, or the immediate result of the disorder. Vomiting, coughing, excessive or defective sweating are extrinsic disorders of this kind: rashes on the skin and caries of the teeth are results of disorder of intrinsic function. Are these disorders diseases, or are they symptoms merely? That depends entirely on whether they do or do not enter as correlatives into a combination of disorders all owning but one intra-corporeal cause. Hæmatemesis and coughing are now known to be correlated with other disorders—with disorder of the intrinsic function of the stomach and structural lesion of the stomach in the one case, and with some disorder of the air-passages or lungs in the other. Hæmatemesis and coughing are therefore now reduced in rank, from diseases to symptoms. When they were diseases, they were correlated with a postulated but unassignable cause, and were not contemplated apart from that cause. Now that the cause is assignable, they can be disentangled from it and contemplated apart from it and isolated from it; and so contemplated in isolation they are no longer diseases: they are symptoms. If a rash on the skin cannot be correlated with any other disorder, but only with a cause, known or unknown, it is a disease, and is called the disease of psoriasis, or acne, or icthyosis; but if it can be correlated with other disorders, such as fever, then the rash is a symptom only, and the disease is the group, consisting of the rash and the fever, correlated by some under-

lying cause. Even the same rash, *e.g.* psoriasis, which, when uncorrelated with other disorders is called a disease, becomes, when correlated with other disorders, such as those of gout, a symptom. Caries of the teeth, seeing that it cannot be correlated with any other disorder, is clearly a disease. It is the whole of the disorder that can be attributed to a single intra-corporeal cause.

Another difficulty in applying the definition may be felt when the correlating cause of the disorder is widespread, but concentrates its attack mainly upon a single organ. Few diseases are more distinctively diseases, or are better entitled to be called a disease, than Addison's disease ; yet in Addison's disease the correlating cause is usually tuberculous disease of the supra-renals, a local manifestation of a tuberculosis which may exist in other parts of the body also. In this case, the sum of the correlated disorders includes more disorders than are included in the concept of Addison's disease ; and Addison's disease stands in the same relation to tuberculosis as endocarditis to acute rheumatism. It is a sub-disease of tuberculosis. It is a local focus, carrying its own symptoms and consequences, of a general infection. Gumma of the brain or of the liver stands in the same relation to syphilis. All these are sub-diseases ; but there are practical reasons which render it expedient to put them in a different position. Addison's disease is still Addison's disease, whether the destruction of the supra-renals is due to tuberculosis or to cancer. Tumour of the brain or of the liver has its own characteristic symptoms, which with the tumour make up the disease, whether the tumour is syphilitic, or tubercular, or gliomatous, or cancerous. It is therefore practically convenient to place the chronic infections, such as tuberculosis, syphilis, cancer, hydatids, and so forth, in a separate class, and to regard each of them as capable of producing various diseases according as their attack is focussed in this organ or in that, reserving the name of 'a disease' for those clinical aggregates that result from the localisation of an infection in a particular organ.

The presence of certain other correlatives besides disorders of function is sometimes allowed to enter into the concept of a disease. Instances of such correlatives are the time of life at which the disorder occurs, and the extra-corporeal cause to which it is due. By taking into the correlate the time of life

at which the disorders occur we construct such diseases as ophthalmia neonatorum, infantile diarrhœa, scurvy of childhood, adolescent insanity, climacteric insanity, senile gangrene, and so forth; and by including in the correlate the extra-corporeal cause we construct such diseases as caisson-disease, wool-sorter's disease, chimney sweeper's cancer, mountain-sickness, and many others. Are these true diseases, or are they not?

As long as we adhere to the meaning of 'a disease'—that it is the whole group of correlated disorders from which the patient suffers, together with their intra-corporeal cause—it clearly does not matter in the least what name we give to this group so long as the name is not misleading; but the titles of diseases, when they are meant to be significant of the cause, are so apt to mislead, or to become obsolete and inappropriate in the light of new knowledge, that it is inadvisable to use such titles. The time of life at which a disease occurs is clearly not one of the disorders that enters into the composition of the disease; but there is no objection to calling attention by the name of a disease to the time of life at which it occurs provided that it does occur solely at that time of life, and provided that we do not confuse it with similar diseases that may occur at other times of life. In fact, the names of most of the diseases that are denominated by the time of life at which they occur are open to one or other of these objections. Infantile diarrhœa does not materially differ from diarrhœa occurring at other times of life, nor is there a special 'infantile' diarrhœa different from other diarrhœas that may occur in infants or adults. Gonorrhœal ophthalmia is more frequent in the newly-born, but no age is immune from it. Adolescent insanity and climacteric insanity have, it is true, their proper features, and the former title is unobjectionable, because that peculiar form of insanity does not occur at any other time of life, and no other form of insanity occurs in adolescents; but the term climacteric insanity is apt to mislead, and frequently does mislead, alienists into calling every form of insanity that occurs about the climacteric in women climacteric insanity, though women at that time of life are no more immune from other forms of insanity than men are.

A disease is always, as the article asserts, a single thing, a unified whole, an individual or unit. Yet, as has been said,

it is always a group of things. Even in such a case as neuralgia, the pain alone is not the disease. Neuralgia, considered as pain *et preterea nihil,* is a symptom. Neuralgia the disease is more than this : it is the symptom plus a postulated intra-corporeal cause, and it is the correlation of the symptom with an underlying cause that converts the symptom into a disease. A disease is an individual thing, but it is always a compound thing, and it is compounded by the mental operation of the observer. The constituents of the disease exist separately in the body and mind of the patient. Their combination into a whole is effected in and by the mind of the contemplator, and consists in the way in which he contemplates them. The constituents of the disease are either parted in space, like the clot in the brain and the paralysis of the limbs; or they are parted in time, like the hot fit and the cold fit of ague; or they are parted in nature, like the pain and the swelling of inflammation. The several factors that go to make up a disease are separate things, which in the patient exist apart, and have no unity until they are combined in the mind of the observer by his mental operation. The disorders are disorders of the same person, but in him they are not one, but many. What unity they have, other than what is imposed by the causal nexus, is a conceptual unity, a mental grouping, not a material propinquity. A disease is a group of things, but the things are collected together, not in space, nor in time, but in the mind of the observer. A disease is a mental construct, and exists, not in the patient, but in the mind of the observer only. What exists in the patient is not a disease, but one or many disorders of function.

When we speak of a disease attacking a person, we are using a convenient figure of speech, but a figure of speech only. The person is attacked by bacteria, or protozoa, or what not; but he cannot be attacked by that which has no existence except in the mind of the observer. We may speak of a disease as arising within or affecting a person, but what arises in him and affects him is not a disease, but a disorder of some function, which is not a disease until the idea of it is combined in the mind of the observer with other ideas. The patient suffers also from the consequences and symptoms of this disorder of function; but these consequences and symptoms are not the disease. It is the combination of them all that

constitutes the disease, and this combination never exists in the body of the patient; for all the disorders are never present in him at the same time. The combination is in the mind of the observer: it is he who constructs the disease, which has no existence outside of his mind. When we speak of treating a disease, or of a disease getting better or worse, we are using convenient but inaccurate figures of speech. We do not administer drugs to the disease: we administer them to the patient. It is the patient, not the disease, that is put to bed, poulticed, fomented, and bathed. It is the patient, not the disease, that gets better or worse, that recovers or dies. A disease in the human body no more has any existence *in rebus naturæ* or *in rerum naturâ* than a riot in the body of the community. When three or more persons behave violently in the street, we call the combination a riot; but there is no such single thing in the street as a riot. The riot is a mental construct. What exists in the street is a number of persons behaving riotously, and we mentally combine these several instances of riotous conduct into a single concept, and call the concept a riot. If a person is injured or killed in a riot, we do not say, nor can we think, that he was killed by the riot. He was killed by one or more of the rioters. The police do not charge or disperse the riot: they charge and disperse the rioters. It is easy in this case to see that the malady of the community is a mental construct and not a perceptible thing; and it is easy to appreciate because lawyers have strictly defined what they mean by a riot, and when they speak of a riot they know what they are talking about; but doctors do not recognise that a disease is a mental construct, because they have never defined a disease, and when they speak of a disease they do not, with all deference to them, know what they are talking about.

There are two other terms in medicine that clamour for definition. These are 'functional disease' and 'organic disease,' which are commonly used as complementary and antithetic.

Every disease is of necessity functional, in the sense that it includes defect or disorder of function as an integral and necessary part of its composition. In fact, if we include in function the intrinsic as well as the extrinsic function, every disease consists entirely of disorders of function and of nothing else; and in this sense, every disease is of necessity purely

functional. But in the common acceptation of the terms, do we mean, when we speak of organic disease, a disease in which there is structural damage that could be recognised on the post-mortem table or under the microscope, and by functional disease a disease in which no such damage could be found? This I think is the distinction that most people would formulate off-hand, but it is unquestionably wrong. Hysteria is the very type and example of what is called a functional disease, and in hysteria there are often structural changes correlated with the other disorders and therefore forming part of the disease. There may be wasting of muscles, shortening of muscles, deformities, changes even in ligaments and bones. Obviously, what is meant by a functional disease is not a disease which is unaccompanied by structural changes, but one in which the structural change, if present, is not correlating but correlated. It is a result, not a cause, of the disease. May we then describe a functional disease as a disease in which there is no structural basis, in which the cause of the whole combination of symptoms cannot be identified with any structural damage that can be found and recognised? Many would, I think, accept this as a good definition, but it is unquestionably wrong, for it would include among functional diseases epilepsy, chorea, neuralgia, infantile convulsions, and many cases of insanity, and none of these is ever considered a functional disease. Moreover, many 'organic' diseases whose correlating structural basis is now known were considered to be 'organic' diseases long before their structural basis was known.

I believe that what is meant by a functional disease is a disease in which not only can no correlating structural basis be found, but also no correlating structural basis is believed to exist. This would exclude from the class of functional diseases all the diseases just mentioned, for we undoubtedly believe that in them such a basis exists, although we are unable to discover it. If this is so, and if there is no structural change as a correlating basis of the functional disease, what is the correlating basis? It is, or it is attributed to, the patient's imagination. I do not say that 'functional' disease is the same as imaginary disease. It is not. It is clear that in hysteria the contractions and other disorders of nutrition are not imaginary. But what distinguishes the diseases that are

called functional is that their correlating basis, their cause, is in the patient's imagination. A disease is a mental construct. It exists nowhere but in the mind of the physician; and consequently we find that many diseases, such as plethora, marasmus, biliousness, sluggish action of the liver, chill on the liver, and so forth, are wholly imaginary. But the physician holds no monopoly of imagination. The patient has his share; and if the physician can imagine a complete disease, the patient can imagine the basis of a disease, and not infrequently he does so. That the basis of certain 'functional' diseases is in fact imaginary is shown by the fact that their symptoms may be anatomically and physiologically impossible. A paralysis or an anæsthesia, for instance, may be incompatible with the distribution of the nerves; and apart from supposition, no experienced physician can possibly doubt the existence of diseases whose basis is in the patient's imagination, and nowhere else. Taking hysteria as the type of 'functional' disease, it is found that, whatever the subsequent symptoms may be, it always begins as an inability or incapacity; and there is no difficulty in conceiving that an inability or incapacity may be imaginary. The patient imagines that she cannot stand, walk, move her arm, apply her mind, endure noise or light, or do something else that she has always been able to do; and as long as she is convinced of her disability, it is a real disability. Undeceive her, and it disappears. That the causal nexus is in the imagination, and in that alone, is proved by the erratic distribution of the disorders. They do not correspond or fit in with any natural grouping of organs or functions. Anæsthesia of one arm may be correlated with paralysis of the opposite leg, and loss of half the field of vision in only one eye. Numbness of the little finger may accompany numbness of the thumb of the same hand. Such a distribution cannot be accounted for by any material cause. No structural change, whatever its nature, could correlate them all. The correlating cause must be the imagination.

By an 'organic' disease, we understand a disease that is not of this nature. We may not know what the correlating cause of the disorders is, but we believe that it is not imaginary. We know that the correlating cause of some diseases is gross anatomical damage. Such diseases are certainly 'organic.' We know that the correlating cause of others is microscopical

damage, and such diseases also are certainly 'organic.' In a third class of diseases, such as epilepsy and neuralgia, we do not know the correlating cause. It may be some chemical aberration in the constitution of the tissue, or it may be something else; but we are quite confident that it is not anything imagined by the patient. Moreover, it is not and does not begin as a mere inability or incapacity. Such a disease is therefore excluded from the class of 'functional' diseases, and included among the 'organic.'

I think that this description of a functional disease as one whose correlating basis is in the imagination of the patient helps us to understand why it is often so very difficult to distinguish 'functional diseases' from pretended diseases; for they have in common that their origin is mental. In the one case the origin is in the imagination, but the disease is a true disease, and is curable by the ministrations of the physician. In the other case the origin is in the will, the disease is an imposture, and is curable by treatment at the hands of the magistrate.

These, then, are the fundamentals of medicine that are here examined and defined:

1. *Function.*—The duty, office, work, or part that is performed by an organ or tissue.
2. *Extrinsic Function.*—The work done by an organ or tissue for the other parts of the body, or for the body as a whole: the part the organ or tissue plays in the bodily economy.
3. *Intrinsic Function.*—The maintenance and repair by an organ or tissue of its own structural integrity.
4. *Disease.*—Disorder of function. The wrong or defective execution of function, either extrinsic or intrinsic. Also the signs and results of such defect or disorder.
5. *Symptom.*—A perceptible sign or manifestation of disorder or defect of function.
6. *A Disease.*—A mental construct or concept, consisting of a symptom or group of symptoms, correlated with or by a single intra-corporeal cause, known or postulated.
7. *A Sub-Disease* or *Symptomatic Disease.*—A symptom or group of symptoms correlated with or by a structural change which is part of an existing disease.

8. *An Infection.*—The invasion of the body by a living foreign agent which has the power of multiplication and distribution within the body, and may concentrate its attack on this or that part so as to give rise to a sub-disease of which the damage to that part is the correlating agent. (For the purpose in hand malignant disease is a foreign agent.)
9. *Organic Disease.*—A disease whose correlating basis is believed to be material, that is to say a disorder of the structure or function of some part of the material body.
10. *Functional Disease.*—A disease which is believed to have no correlating basis except in the imagination of the patient.

I do not examine the terms syndrome, symptom-complex, and other monstrosities made in Germany, and having the true German character of being made for show and not for use. They are mere jargon, signifying on the part of the user a pose of superiority and a striving to be up to date; and are much on a par with the exploit of the servant girl who calls a brilliant flash of lightning 'chronic,' or the alienist who calls primary dementia 'dementia præcox.'

LA DOCTRINE SOCIOLOGIQUE D'EMILE WAXWEILER

PAR N. IVANITSKY ET F. VAN LANGENHOVE

" LA sociologie n'avance pas. Alors que les techniques modernes de l'observation et de l'expérimentation ont renouvelé toutes les sciences, elle seule reste enlisée, retenue dans son essor par cent attaches diverses qu'en se débattant, elle ne parvient pas à briser. . . ." Ainsi s'exprimait Emile Waxweiler en 1906, dans la préface de sa première œuvre sociologique.

Il est mort d'un fatal accident le 26 juin 1916, à l'âge de 49 ans. Dans ce court intervalle de dix années, dont deux furent des années de guerre qu'il consacra entièrement à la défense des intérêts de la Belgique, son pays, on est en droit de dire qu'il fit notablement progresser la sociologie et que, peut-être, il la dégagea de l'ornière où elle était enlisée.

Jusqu'au milieu de sa carrière, cependant, il s'était exclusivement consacré aux questions économiques ; il laisse une œuvre écrite, d'ordre proprement sociologique, relativement peu considérable. L'*Esquisse d'une Sociologie*, où, en 1906, il fixait le schéma de sa doctrine, est le seul volume de quelque étendue. La guerre a mis obstacle à la publication d'un second ouvrage, fruit d'un long travail, sur l'élaboration sociale de l'écriture. Puis il reste d'assez nombreux articles.

Mais si cette œuvre, matériellement représentée par des livres, paraît assez réduite, c'est que son importance réside moins dans le nombre de pages où elle s'exprime, que dans la solidité du fondement sur lequel elle a édifié sa doctrine et dans la fécondité de la méthode qu'elle a fixée. Pendant dix années d'une extraordinaire intensité de travail, l'une et l'autre ont été constamment mises à l'épreuve, améliorées et accrues. Malgré la disparition prématurée de celui qui les créa, on peut les tenir dès maintenant pour définitivement acquises. Elles porteront leur résultat.

. . . . . .

Appelé en 1902 à prendre la direction de l'Institut de

Sociologie dont le grand inventeur Ernest Solvay venait de décider la fondation à Bruxelles, Waxweiler s'efforça d'en faire un véritable laboratoire de recherches sociologiques. Il porta ses soins à le doter, comme il eût fait pour un laboratoire de sciences exactes, d'un matériel rigoureusement approprié à son objet. La bibliothèque devait, naturellement, en être le principal outil. Mais il ne suffisait point, pour la constituer, de réunir au hasard une collection de volumes traitant de questions sociales. Il importait, au contraire, pour que cet outil offrît des facilités réelles aux recherches et rendît celles-ci productives, que ces volumes enssent été l'objet d'une sélection attentive.

Cette nécessité amena Waxweiler à passer en revue la littérature des sciences sociales et des sciences connues, d'en faire le triage, de la classifier.

Ce fut sa première tâche dans le nouvel institut. Elle lui prit plusieurs années d'un labeur écrasant. Cet effort ne fut pas vain. C'est en parcourant cette masse énorme de documents, en la maniant, en l'annotant, en dominant ce vaste horizon, qu'il entrevit la place de la sociologie dans le plan général des connaissances. Peu à peu les contours de cette science se dessinèrent dans son esprit ; ses traits essentiels se précisèrent ; ses rapports avec les autres disciplines se dégagèrent naturellement. "Dans le même temps," écrivait-il, "que cette science s'obstine à ne pas mûrir, la matière qu'elle doit discipliner apparaît avec évidence ; bien plus, la place qu'elle est appelée à occuper se crée toute seule par un arrangement nouveau de la grande mosaïque des connaissances" (*Esquisse d'une Sociologie*, p. 10).

Cette place qui revenait de façon si évidente à la sociologie, il la voyait dans le cadre de la biologie générale.

Or, depuis plusieurs années, un courant nouveau soulignait, en biologie, l'importance des facteurs tenant au milieu, et mettait en relief la façon dont ces facteurs influent sur la forme, les organes, les fonctions, les manifestations psychiques de l'être vivant. De telle sorte que l'étude complète de celui-ci exige, outre l'étude de sa forme extérieure, de sa structure interne, des fonctions internes de son organisme, celle de la façon dont il se comporte à l'égard du milieu auquel il appartient. L'éthologie est venue s'ajouter à la morphologie, l'anatomie et la physiologie.

Waxweiler conçut toute l'importance de cette notion du milieu pour l'intelligence des phénomènes sociaux. Pour lui, comme pour Robin, " toute idée d'être organisé vivant est impossible si l'on ne prend en considération l'idée du milieu " (*loc. cit.*, p. 23).

Mais le milieu est une notion complexe qui comporte une multiplicité d'aspects. La commodité des recherches conduit à distinguer du milieu physique, celui qui est constitué par les êtres vivants. Le milieu physique groupe les conditions géographiques, climatériques, d'alimentation inorganique. Le milieu vivant diffère suivant qu'il comprend des êtres d'espèces différentes ou de même espèce. Les phénomènes qui relèvent du premier répondent au besoin de se nourrir et affectent la forme soit du prédatisme, du parasitisme, de la symbiose ou de l'élevage; ou bien, ce sont des rapprochements entre des êtres d'espèces différentes d'où résultent un appui, une protection, une assistance : ainsi, des insectes divers se mêlent aux expéditions prédatrices des fourmis, des agglomérations de plantes partagent des conditions communes d'existence, ou encore, des animaux sont domestiqués par l'homme.

Les rapports entre les êtres de même espèce sont d'un ordre différent; ils naissent de l'aptitude de reconnaître ses semblables, ou de l'affinité spécifique. Et ici ce peuvent être des rapprochements sexuels, des colonies d'organismes unicellulaires, des organismes pluricellulaires, enfin, des rapports entre pluricellulaires de même espèce.

Ces derniers rapports, qui se présentent chez divers animaux, sont surtout propres à l'espèce humaine. Ils se diffèrent d'une façon suffisamment nette dans l'ensemble de l'éthologie organique, en général, et parmi les phénomènes d'affinité spécifique, en particulier, pour qu'on soit justifié à en faire l'objet d'une discipline séparée. " La partie du milieu ainsi détachée par la pensée, " dit Waxweiler," s'appelera le milieu social ; celle des sciences biologiques qui en fera son domaine sera l'éthologie sociale ou, puisque le mot existe déjà, la sociologie " (*loc. cit.*, p. 62).

Ainsi s'établit la filiation de la sociologie dans le cadre de la biologie générale. En délimitant dans celle-ci des réalités de plus en plus étroitement circonscrites, elle apparaît en fin de compte comme la science des phénomènes résultant des actions réciproques des êtres de même espèce. Posée sur ces assises

concrètes et solides, elle devient une discipline essentiellement positive et précise.

L'homme réagissant aux sollicitations du milieu social, tel est le terme irréductible de la sociologie humaine. Ces réactions, elle les considère sous deux aspects caractéristiques.

Tout d'abord, elle s'attache à l'acte par lequel l'individu s'adapte à son milieu. Elle recherche quelles conditions particulières provoquent cet acte, lesquelles le font varier. Elle suit ce que deviennent ces adaptations spontanées dans les circonstances répétées de la vie collective. Elle s'attache à découvrir, celles qui, devenant permanentes, prennent le caractère de véritables fonctions qui assurent l'équilibre de la vie sociale. Dégager ces fonctions devient sa principale préoccupation.

Mais un second aspect des réactions interindividuelles fixe son attention. Il tient à certaines propriétés spécifiques de l'être humain. Si les rapports de celui-ci avec l'ambiance s'opèrent à l'aide d'organes de relation, dont le principe est commun à beaucoup d'autres êtres vivants, ces organes présentent chez lui un développement et un degré de complication que l'on n'observe nulle part ailleurs. Le fonctionnement du cerveau humain est tel que les adaptations spontanées y font naître des élaborations mentales. Sans doute, ces adaptations peuvent, chez l'animal comme chez l'homme, se transformer en habitudes. Mais là ne s'arrête point nécessairement leur évolution, chez celui-ci. Saisies par la conscience, il arrive qu'il les exprime en y ajoutant un caractère obligatoire auquel d'autres hommes consentent ; l'habitude individuelle devient usage. Ce n'est point là le seul travail auquel l'esprit se livre sur l'acte initial d'adaptation. Il peut encore arrêter sa réflexion sur l'usage, y accrocher des élaborations nouvelles, l'incorporer à un système logique. Dès lors, l'acte individuel, devenu habitude, puis usage, se transforme en règle qui, à son tour, peut évoluer en institution. Soumises à ce processus acte-habitude-usage-règle-institution, les adaptations de la vie collective et les fonctions sociales qui sont issues de celles-ci s'organisent en systèmes d'impératifs, régulateurs des actions individuelles.

Ainsi, l'analyse sociologique dégage, parmi les réalités

humaines qu'elle embrasse, deux pôles solidaires, dont elle étudie les actions réciproques : d'une part, les adaptations spontanées de l'individu aux conditions du milieu social, adaptations qui, devenant constantes, peuvent prendre le caractère de véritables fonctions ; d'autre part, les élaborations logiques qui résultent du travail de l'esprit s'appliquant à ces adaptations et à ces fonctions, et les organisant en systèmes d'impératifs.

. . . . . .

L'objet assigné à la recherche sociologique lui impose une méthode propre. Ayant à surprendre des adaptations, à suivre leurs variations en accord avec les conditions changeantes du milieu, à démêler les élaborations mentales qu'elles suscitent, la sociologie ne saurait se borner, désormais, à l'étude des aspects extérieurs de la vie sociale. Il faut, au contraire, qu'elle s'attache à ses aspects internes et génétiques ; qu'elle néglige la forme des phénomènes pour considérer leur processus de formation. Et elle dégagera aussi les phénomènes des arrangements arbitraires conçus sur des apparences, pour mettre en relief le mécanisme de leurs enchaînements successifs.

Par-dessus tout, elle restera constamment en contact direct avec l'individu. " Que la règle soit, en Sociologie," dit Waxweiler, "de se cramponner à l'individu agissant dans son milieu social ; observer des activités plutôt que des résultats d'activités ; surprendre la vie dans ses manifestations agissantes ; expliquer le passé par le présent " (*Loc. cit.* p. 64).

Son but essentiel étant de découvrir des fonctions, cette méthode est appelée " fonctionnelle."

. . . . . .

La place assignée dans la science à la sociologie, de même que l'application qu'elle fait de la méthode fonctionnelle, mettent en relief sa portée la plus remarquable.

Ethologie sociale, elle est désormais solidaire de la biologie générale et des différentes disciplines qui en ressortissent. Elle n'est plus ni en dehors ni au-dessus d'elles, mais sur un plan identique au leur. Apparentée de la sorte, il est impossible qu'elle vive de son propre fonds ; leur collaboration lui est indispensable ; elle a besoin de leurs apports. L'interprétation des diverses manifestations de la vie lui est un guide nécessaire.

Branche de l'éthologie organique, il lui importe de connaître les modes propres, suivant lesquels s'établissent les rapports interindividuels dans chaque espèce, comment ces rapports se modifient en étroite correspondance avec les conditions du milieu, comment chaque adaptation, chaque habitude nouvellement acquise retentit sur le fonctionnement de l'organisme, en particulier du cerveau, comment la personnalité psychique des individus varie, au cours de leur développement, d'après leur âge, leur sexe, leur race, leur espèce.

Ainsi, de par son fondement même, une intime solidarité unit la sociologie à la biologie générale, à l'éthologie, à la physiologie, à la psychologie. Elle trouve dans ces sciences outre les acquisitions précieuses qu'elle en tire, une confirmation de son orientation réaliste et positive.

Mais une solidarité non moins étroite rattache la sociologie à d'autres disciplines ; à celles qu'on est convenu d'appeler sciences sociales. Ces dernières constituent le champ proprement dit de ses investigations ; elle leur applique la méthode fonctionnelle : au droit, à la morale, à la religion, à l'art, à l'économie, à l'ethnologie, à l'histoire. . . . Elle extrait des données que chacune lui fournit ce qu'elles peuvent renfermer d'explicatif ; elle en fait le triage ; elle en dégage les grandes lignes de l'interprétation sociologique. En même temps qu'elle s'en alimente, rompant avec l'isolement où ces disciplines se confinaient, elle leur fournit un point de vue commun qui les éclaire toutes d'une interprétation centrale féconde.

Ainsi conçue, la sociologie démontre à la fois la nécessité et la possibilité d'une vaste collaboration des sciences biologiques et sociales. Réaliste et concrète dans son fondement, elle devient, dans sa portée, essentiellement explicative et synthétique.

Sa portée synthétique impose à la sociologie des procédés de travail particuliers. La collaboration des sciences biologiques et sociales exige la collaboration de spécialistes appartenant à ces disciplines.

Seules les recherches coordonnées d'une véritable équipe de spécialistes peuvent promouvoir la sociologie ; à leur tour ces recherches appellent un cadre approprié au travail en commun. Elles ne pourront se poursuivre que dans un

institut, où seront rassemblés les instruments nécessaires aux investigations, et où chacun trouvera, comme dans un cloître, le recueillement propice à l'étude, cependant qu'une même grande voûte, recouvrant les cellules individuelles, assurera les liaisons et l'unité indispensables.

Cet institut a été fondé en 1902 par Ernest Solvay, dans un parc de Bruxelles. Waxweiler l'équipa. Il y réunit un personnel scientifique.

C'est dans cette époque d'organisation, qu'il publia l'*Esquisse d'une Sociologie*, afin de " donner," ainsi qu'il le dit lui-même dans l'avant-propos, " une base à des recherches de sociologie positive pouvant être entreprises dans l'Institut." Il précisa le point de vue, il fixa la méthode, et, laissant chacun dans sa spécialité, il orienta l'attention de tous vers le point de vue commun ; il réduisit les acquisitions des disciplines particulières à leur commun dénominateur sociologique ; il les incorpora dans les cadres d'une grande œuvre constructive. Il fut, dans cet institut, le foyer central ; à la fois celui qui inspire et celui qui coordonne.

Après plusieurs années, en 1910, les efforts des collaborateurs avaient acquis une cohésion suffisante. Waxweiler entreprit une application systématique de sa doctrine. Il commença la publication régulière des *Archives sociologiques*. Les études récentes, dans les diverses spécialités, y servent de prétexte à vérifier le point de vue sociologique ; les données utiles qu'elles renferment en sont extraites, ramenées à ce point de vue, mises à pied d'œuvre pour les synthèses futures.

Une mort prématurée a surpris Waxweiler au moment où il commençait à recueillir le fruit de ces travaux préparatoires. Elle a interrompu la tâche qu'il avait entreprise ; elle n'y a point mis fin. D'autres, qu'il a entraînés à sa discipline, la poursuivront, et apporteront en son nom les résultats que ses efforts ont préparés.

Bien que son œuvre soit jusqu'ici privée de ce couronnement, l'étude attentive de sa doctrine suffit cependant à en faire mesurer l'importance. La sociologie était partagée entre des théories, parfois brillantes, souvent incertaines et flottantes. Il a précisé sa place dans le plan général des connaissances, fixant, conformément à une heureuse harmonie, ses rapports avec les autres sciences, et il lui a donné en même temps des assises certaines ; il a défini son objet de façon précise et

concrète, tout en lui ouvrant un champ étendu ; il lui a fourni une méthode rigoureuse et productive ; il lui a assuré une large partie explicative et synthéthique ; il lui a procuré, enfin, un cadre, un outil, une discipline de recherche appropriés.

Cette œuvre est essentiellement constructive. Elle a donné à la sociologie un point de vue fécond.

POPULAR SCIENCE

THE OLDEST FLINT IMPLEMENTS. By J. Reid Moir, F.R.A.I.

In a former article in Science Progress ("Flint Fracture and Flint Implements," July 1916, pp. 37–50) the present author was privileged to give some account of the past and present position of prehistorical research, and to describe a number of experiments in the fracture of flint, which were carried out with the object of providing prehistorians with some satisfactory data upon which to base their acceptance or rejection of any series of flaked flints as being humanly fashioned. It is proposed in the present article to apply the criteria furnished by the above-mentioned experiments to a series of very primitive and ancient flaked flints first discovered by Mr. Benjamin Harrison in the high plateau gravel of Kent, and which have been the cause of much disagreement and disputation amongst archæologists. Without troubling the reader with the somewhat complex geological facts which demonstrate the great antiquity of these primitive flaked flints, it may be stated that the deposits in which they mostly occur pre-date, by a considerable period, those in which the earliest implements of river-drift, or "palæolithic," man are found, and in consequence of which the chipped stones recovered from these very ancient deposits have been rendered suspect by those who hold conservative views upon the question of man's antiquity. But it must be pointed out that those who believe that these very ancient flaked flints are humanly fashioned may with equal justice be said to hold extreme views in favour of the high antiquity of man, and unless either school can bring forward definite scientific facts in support of their respective opinions, those opinions must be regarded as being of very little real value. The author does not propose therefore to enumerate the various "reasons" which have been advanced in the past for the rejection or acceptance of these particular specimens. He proposes to confine his remarks solely to the characteristics of the flaking exhibited by them, and by com-

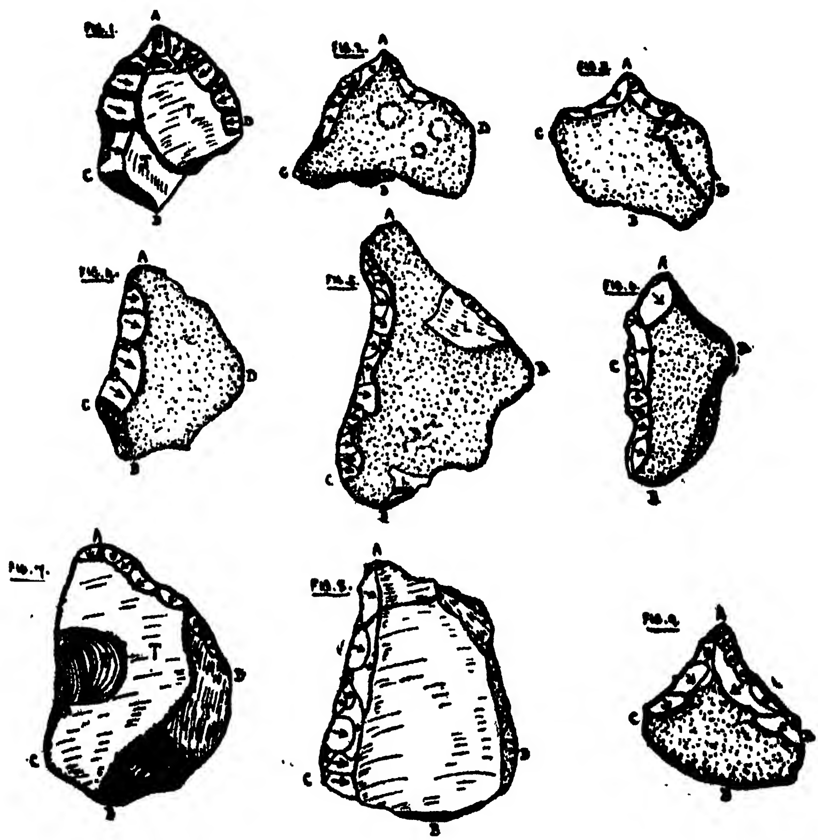
paring it with that produced on other stones—on the one hand by means of fortuitous percussion and pressure, and on the other by blows delivered with a hammer-stone used in the ordinary "human" manner—to decide whether these very primitive flints have been flaked by man, or by the unguided forces of nature. But it may be as well, before proceeding to describe the specimens selected for illustration, to give the reader some idea of the general appearance of these primitive edge-flaked flints.[1] They are usually of a more or less tabular form, the whole of one side of which very often exhibits the original crust or cortex of the flint, while the whole of the other is represented by the hard interior owing to the specimen having been split by some disruptive force, which very frequently, but not always, can with some confidence be described as being of thermal origin. The edges of these tabular flints have then been trimmed by somewhat steep flaking so that the specimens assume a pointed form (figs. 1, 2, and 3), or are provided with a more or less straight cutting edge (figs. 4, 5, and 6), and it has been suggested by those who regard these specimens as being humanly fashioned, that such forms might have been used by a primitive race of people for boring and scraping purposes.

The colour of the flaked and broken surfaces of the specimens is generally reddish-brown, some being of a darker shade than others. They very often show signs of having been subjected to rolling by water, which has had the effect of rounding the sharp edges and angles of the flints, and of imprinting upon their surfaces incipient cones of percussion due to the impact of other stones. Very frequently too, the specimens exhibit striæ on one or other of their flat surfaces, showing that at some period of their history they have been subjected to a slight amount of pressure. There can be no doubt that it is of importance to make a really dispassionate study of these primitive edge-flaked stones, as their antiquity is undoubted, and if of human origin, they represent man's first efforts to shape flints to his needs.

The author has had various opportunities during the past few years of critically examining a number of these ancient

[1] These specimens have been known to archæologists as "Eoliths" (*eos*, dawn, and *lithos*, stone), but as some observers have used this term to describe any piece of roughly broken flint, it is necessary to adopt a more precise nomenclature.

flints, and has selected six (together with three others) from his own collection for illustration in this paper. In the delineation of these specimens a severely diagrammatic style has been adopted, but each flake area is faithfully outlined, and the arrows which indicate the direction in which the force acted



FIGS. 1 TO 9.

which removed the flakes have been put in with very great care.

DESCRIPTION OF THE SPECIMENS

Fig. 1.—Found in a gravel-pit in the occupation of Messrs. A. Bolton & Co., Ltd., Henley Road, Ipswich. The gravel occurs at a height of about 120 O.D., and forms one of the plateau series of deposits. It is generally supposed to be

intermediate in age between the early glacial Contorted Drift of Cromer and the glacially deposited Chalky Boulder Clay. The specimen, and others of the same order from the gravel, have evidently, by their condition, been derived from some still more ancient deposit which at some remote period was broken up and redeposited.

The flaked areas of the flint are light chestnut-brown in colour, and the side opposite to that figured is formed of unflaked cortex which is extensively coated with a deposit of what appears to be manganese. The major fractures (marked T in the illustration) are almost certainly of thermal origin, while the edge flaking which has given to the stone the well-marked pointed form is considered, for reasons which will be given later, to be the result of blows. The specimen, which measures in greatest length (A—B) $2\frac{1}{4}$ in., greatest width (C—D) $2\frac{1}{8}$ in., greatest thickness $\frac{5}{8}$ in., exhibits neither scratches nor incipient cones of percussion upon its flaked surfaces. The edges and angles of the flint are somewhat smoothed, possibly by the action of running water.

Fig. 2.—Found in the high-level plateau gravel of Kent. The exact geological age of this deposit, though undoubtedly extremely ancient, is still in debate, and as it does not contain any fossiliferous remains, it is possible that it may never be satisfactorily dated. The flaked areas of the specimen range in colour from yellowish-white to yellowish-brown. The upper surface is formed almost entirely of unflaked cortex which is of a dark brown colour. The under-surface, which appears to be attributable to thermal breakage, exhibits the hard interior of the flint, and is extensively scored with small striæ which follow very erratic and non-parallel courses. The edge-flaking, which has given to the stone the well-marked pointed form, is considered to be the result of blows. The specimen, which measures in greatest length (A—B) $1\frac{3}{4}$ in., greatest width (C—D) $2\frac{7}{16}$ in., greatest thickness $\frac{3}{4}$ in., exhibits no incipient cones of percussion upon its flaked surfaces, nor does it show any signs of having been subjected to the action of running water.

Fig. 3.—Provenance the same as specimen represented in fig. 1. This specimen (fig. 3) has been made from a piece of tabular flint, and its upper and lower surfaces are represented by unflaked cortex. The flaked areas of the specimen are of

a deep chocolate-brown colour, and exhibit a well-marked glaze. The edge-flaking, which has given to the stone the well-marked pointed form, is considered to be the result of blows. The specimen, which measures in greatest length (A—B) $1\frac{11}{16}$ in., greatest width (C—D) $2\frac{1}{8}$ in., greatest thickness $\frac{11}{16}$ in., exhibits neither incipient cones of percussion nor striæ upon its flaked surfaces, nor does it appear to have been subjected to the action of running water. The original surfaces of the flint, which are more ancient than the flaking which has produced the present pointed form, show, however, some little amount of abrasion.

Fig. 4.—Provenance the same as specimen represented in fig. 2. This specimen (fig. 4) has been made from a piece of tabular flint, and its upper and lower surfaces are represented by unflaked cortex. The flaked areas of the specimen are of a chocolate-brown colour. The edge-flaking, which has provided the flint with a more or less straight cutting edge, is considered to be the result of blows. The specimen, which measures in greatest length (A—B) $2\frac{3}{8}$ in., greatest width (C—D) $\frac{15}{16}$ in., greatest thickness 1 in., exhibits a few incipient cones of percussion upon its flaked surfaces, and some small striæ are to be seen upon some of these surfaces. It would appear that the specimen has been subjected to some amount of rolling by water action, as the edges and sharp angles of the stone are somewhat abraded.

It is, of course, possible that some of this abrasion may have been caused by the slight amount of pressure to which the stone has been subjected, and to which the small striæ mentioned bear witness.

Fig. 5.—Provenance the same as specimens represented in figs. 2 and 4. This specimen (fig. 5) has been made from a piece of tabular flint which exhibits unflaked cortex over nearly the whole of its upper surface. The lower surface exhibits the hard unflaked interior of the flint, and is made up of several fracture surfaces which have in all probability been produced by thermal action. The under-surface of the stone is of a chestnut-brown colour, while the upper unflaked surface is of a *café-au-lait* shade. The edge-flaking, which has provided the flint with a more or less straight cutting edge, is considered to be the result of blows. This edge flaking exhibits a well-marked glaze, it is not stained, and

shows the almost unchanged black interior of the flint. The specimen, which measures in greatest length (A—B) $3\frac{7}{16}$ in., greatest width (C—D) $2\frac{15}{16}$ in., greatest thickness 1 in., exhibits no incipient cones of percussion upon its surfaces, but the lower thermally broken surfaces exhibit a few small striæ. The stone appears to have suffered some amount of abrasion, but at a time prior to that to which the edge-flaking is referable. The flaked areas marked L in the illustration are recent fractures.

Fig. 6.—Provenance the same as specimens represented in figs. 1 and 3. This specimen (fig. 6) has been made from a tabular-shaped nodule of flint, and exhibits unflaked cortex over the whole of its upper and lower surfaces. The upper surface of the specimen is lightish-brown in colour, while the lower shows little or no staining. The edge-flaking, which has provided the flint with a more or less straight cutting edge, shows a well-marked glaze, and is considered to be the result of blows. The flaked areas of the stone are toffee-coloured. The specimen, which measures in greatest length (A—B) $2\frac{11}{16}$ in., greatest width (C—D) $1\frac{7}{16}$ in., greatest thickness $\frac{5}{8}$ in., exhibits neither incipient cones of percussion nor striæ upon its surfaces. The flint shows little or no signs of abrasion.

Fig. 7.—Found upon the sea-beach at Lowestoft, Suffolk. This specimen, which is of a dark chocolate-brown colour, with a slight bluish tinge upon its upper surface,[1] has been formed, so far as its larger fracture-surfaces are concerned (marked T in drawing), by thermal action. The edge-flaking, which is of a different order from that exhibited by the specimens hitherto described, is considered to be the result of blows. The specimen, which measured in greatest length (A—B) $3\frac{5}{16}$ in., greatest width (C—D) $2\frac{11}{16}$ in., greatest thickness $1\frac{1}{2}$ in., is heavily abraded and exhibits numerous criss-cross striæ and incipient cones of percussion upon its surfaces.

Figs. 8 *and* 9.—Represent flints flaked by the author with another stone used as a hammer.

The above description, together with the illustrations, will enable the reader to form a very good mental picture of the specimens under examination, and it is now proposed to apply the criteria obtained in the experiments in flint fracture men-

[1] The upper surface of the specimens is the surface which is figured.

tioned above (SCIENCE PROGRESS, July 1916, pp. 37–50), and to decide whether the edge-flaking of the specimens illustrated in figs. 1–7 is of human or natural origin.

The first question for decision is the nature of the force which has produced this edge-flaking, whether percussion or pressure. The author does not consider there can be much doubt that percussion has been the agent of fracture. An examination of the edge-flaking reveals no evidence of the effects of pressure, while all the characteristics of flaking by percussion are, on the other hand, abundantly observable.

There are no "opposing cones of pressure" such as are produced when a stone is subjected to pressure between two resistant surfaces, while the fissures and ripple-marks developed upon the flaked areas are not such as are produced by the effects of pressure. Moreover, the ridges between the flakes are well marked, giving to the flaked edge a somewhat uneven appearance, and it has been ascertained that this prominence of the ridges between the flakes is very seldom, if ever, produced by pressure.[1] It seems, then, that the effects of pressure may be eliminated in our inquiry as to the origin of the edge-flaking of the specimens illustrated in figs. 1–6, and it is also obvious that fig. 7 represents a stone edge-flaked by percussion. The scope of the inquiry being thus narrowed down, we may proceed to try to ascertain whether the blows, which were responsible for the edge-flaking upon the specimens mentioned, were delivered by human or natural agency.

The next point to which attention is drawn is the direction of the arrows which appear on each flake area of the flints illustrated. It will be remembered that in the experiment in which flints were subjected to the effects of fortuitous percussion, it was found that the flakes removed from the edges of the stones had been detached at divergent angles, and it was pointed out that while it seemed reasonable to suppose that haphazard blows would strike the edge of a flint at different angles, the flakes removed by a human being, using a hammer stone, would be taken off at a constant angle to the edge.[2]

[1] See former article, SCIENCE PROGRESS, July 1916, pp. 48–9.

[2] The methods by which the direction of each blow removing a flake was ascertained were described fully in the article mentioned, and need not be recapitulated here.

If the reader will now examine the illustrations accompanying this paper, it will be noticed that while figs. 1–6 show that the force which acted on the edges of the flints removed the flakes at a constant angle to those edges, fig. 7, which was picked up on the sea-beach at Lowestoft, has had the flakes removed from its edge at divergent angles. It will be noticed also that in figs. 8 and 9, which represent flints flaked by the author by means of another stone used as a hammer, the specimens have had the flakes removed from their edges at a constant angle. It appears, then, that these flints (figs. 1–6) have been flaked along their edges by blows, and that these blows have been delivered at a constant angle to those edges. It is also noticeable that figs. 8 and 9, which represent specimens flaked by the author with a hammer-stone, have had their flakes removed at a constant angle to the edge. The specimen represented by fig. 7, on the other hand, which was found upon a shingle beach, where natural percussion has opportunities for operating, has had its flakes removed at divergent angles to its edge. This evidence points undoubtedly to the conclusion that the edge-flaking of the specimens illustrated in figs. 1–6 is of human origin, but we will proceed to a further examination of the flaking to ascertain if this conclusion is supported by other evidence. In the experiment in which flints were subjected to the effects of fortuitous percussion by shaking them in a sack,[1] it was noticed that the flakes removed from the edges of the stones differed in appearance from others removed by human blows. These differences were:

(*a*) The squatness of the fortuitous flakes as compared with those removed by human agency.

(*b*) The fact that the former had cut deeper into the flint, causing a step or ledge to appear at the point of their final separation from the parent block.

(*c*) The occurrence of numerous prominent ripple-marks upon nearly all the flakes produced by fortuitous blows, as compared with their comparative scarcity upon the "human" flakes.

When the specimens illustrated in figs. 1–6 are examined,

[1] See Science Progress, July 1916, pp. 43–4.

it is seen that the flakes removed from their edges are not squat, nor have they cut deeply into the flint, giving rise to a step or ledge at the point of their final separation from the parent block. And in these particulars they agree with the specimens illustrated in figs. 8 and 9, which were flaked by the author. The edge-flaking of the Lowestoft beach specimen, however, exhibits both the characteristics mentioned, and approximates very closely to the edge-flaking produced upon the flints in the sack experiment. The fractures forming the flaked edges of the specimens illustrated in figs. 1–6 also do not exhibit numerous prominent ripple-marks upon their surfaces, such as are so frequently produced by fortuitous blows, while the Lowestoft beach specimen (fig. 7) shows well-developed ripple-marks upon five out of its seven flake-areas. In the case of the two specimens flaked by the author, prominent ripple-marks upon the flake-areas are very infrequent. It will probably be remembered that in the sack experiment to which reference has been made, it was noticed that the edges of the flaked stones tended to assume a sinuous outline due to blows having fallen upon either side of these edges.

In the specimens illustrated in figs. 1–6, the blows which have produced the edge-flaking have all been delivered from one side of the flint only, and in consequence no sinuosity of the edge is observable. This is also the case with the specimens flaked by the author (figs. 8 and 9).

The Lowestoft specimen (fig. 7) shows a distinct tendency to assume a sinuous outline in its flaked edge, due to blows having removed flakes from both sides of that edge.

The specimens illustrated in figs. 1–6 do not exhibit an undue number of truncated flakes removed from their flaked edges,[1] and in the case of the Lowestoft beach specimen (fig. 7) there do not appear to be many developed, though the edge is so battered as to make a certain diagnosis impossible. The specimens flaked by the author (figs. 8 and 9) do not show an undue number of truncated flakes removed from their flaked edges.

The foregoing examination has shown that the primitive edge-flaked flints illustrated in figs. 1–6 exhibit the following

[1] The number of truncated flakes removed from the edge of a flint was shown to indicate whether the specimen had been flaked by man or by nature. See SCIENCE PROGRESS, July 1916, pp. 46–7.

characteristics which are believed to be associated only with human intention :

(*a*) The flakes removed from their edges are the result of blows which have been delivered at a constant angle to these edges.

(*b*) The flakes are not squat, nor do they cut deeply into the flint, causing a step or ledge to appear at the point of their final separation from the parent block.

(*c*) The flakes do not exhibit numerous and prominent ripple-marks upon their surfaces.

(*d*) The flaked edges do not exhibit a sinuous outline, nor do they show an undue number of truncated flakes removed in the formation of these flaked edges.

The specimens flaked by the author (figs. 8 and 9) agree in every particular with the above specimens. The Lowestoft specimen (fig. 7), found upon a shingle-beach, does not, however, exhibit edge-flaking of the same order as the specimens illustrated in figs. 1–6, but the flaking approximates very closely to that produced by fortuitous percussion in the sack experiment described. The author is of opinion that the foregoing examination has indicated with some amount of certainty that the primitive edge-flaked flints selected for illustration (figs. 1–6) are of human origin, and that such specimens deserve the closest attention by all serious prehistorians. But it is to be hoped that, in future, the old unscientific attitude towards these primitive and very ancient flaked flints will be abandoned, and that their acceptance or rejection as the work of man may be based upon some tangible evidence such as has been set forth in this paper. All the specimens illustrated are in the author's possession and can be examined by any one desirous of doing so.

ESSAY-REVIEW

THE SISTER OF SCIENCE, by SIR RONALD ROSS: on

Sonnets and Poems, by JOHN MASEFIELD. [Pp. 52.] (The Garden City Press, Ltd. Price 3*s*. 6*d*.) And

A Book of Homage to Shakespeare, 1916, edited by ISRAEL GOLLANCZ, Litt.D. [Pp. xxvi + 553.] (Oxford University Press. Price 21*s*. net.)

IN spite of our Gradgrinds and Grammarians, Science and Poetry are twin sisters, whose office it is to seek and to sum. Twice blessed he who is inspired by both; for the man of science should be a poet and the poet a man of science—not prepensely perhaps, but in caste. The one sister gives the flame without which seeking is seldom successful; and the other, such reality as will keep the mind from losing itself in the clouds. Thus the goddesses walk ever hand in hand—pure spirits lifting the mind of man, or, indeed, making it.

On Easter morning 1913 (Greek style) I was in the Valley of the Muses on Mount Helikon, among the tumbled columns and ruined pavements which are all that remain of their famous temple. The birthplace of Hesiod was on one side; and from the summit of the hill on the other side ran Hippocrene, past the deserted ruins and through the uninhabited valley—the stream which flowed from the hoof-stroke of Pegasus; and the twin peaks of Parnassus appeared between certain clefts of the mountain. There, in the old days, I thought, men were wise enough to worship, not this Muse or another, but all the Muses; for their temple was one, and, really, the worship of them is one. . . . After all, polytheism is the true faith. Let us therefore not sink to the condition of the present monotheistic occupants of that divine valley: huge fat black people, grunting after the roots of the earth; or lean, long-eared eloquent people, braying their wisdom at the eternal hills; or great tortoises sunning themselves into life upon the broken marbles of the past. Let the lovers of art spare roses for the altar of science, and the lovers of science lilies for the shrines of the arts; and

we shall all find sufficient asphodel, at least, everywhere about us, for both. Then we may see again in that deserted vale :

> 'Tis Apollo comes leading
> His choir, the Nine.
> —The leader is fairest,
> But all are divine.

. . . We may perhaps distinguish two kinds of poetry, the silvern, which is perfect, and the golden, which is perfect and wise. The latter is the rarer ; for it must contain, generally in allegory, the richest colour of human thought. The war is creating much good work of this type, but by far the finest which I have seen—which will especially interest scientific minds—is Mr. Masefield's *Sonnets and Poems*. At first sight the poems would appear to have no lien with the war, but the key is given by the author's stanzas in this number of SCIENCE PROGRESS. On opening the little book, we think we have only a casket of loose pearls ; but on lifting them one by one we find them all to be strung on that most delicate and rare of threads, the theme of Beauty—as revealed in the last of the stanzas referred to. The stringing has been done so exquisitely that we cannot always see the filament for the beads—as in a garden web in the morning sunlight. Really however the thridding is the thing itself—we are recalled from the horrors of the present gloomy day of night to that ideal which perhaps comprises all that is good in us and for us.

But there is another aspect of the poems which is naturally of especial interest to myself. Many of them are based upon a cosmogony of mind which was suggested by me to the poet years ago, and which will be found briefly described in SCIENCE PROGRESS for July last—now set by him in beautiful lines :

> What am I, Life? A thing of watery salt
> Held in cohesion by unresting cells
> Which work they know not why, which never halt,
> Myself unwitting where their master dwells.
>
> . . . . .
>
> If, pressing past the guards in those grey gates,
> The brain's most folded intertwisted shell,
> I might attain to that which alters fates,
> The King, the supreme self, the Master Cell ;
> Then, on Man's earthly peak, I might behold
> The unearthly self beyond, unguessed, untold.

Sonnets 14 and 19 continue the theme.

It is startling to reflect that if this hypothesis be sound, the present devastating war was precipitated by the single Master Cell—say one-five-hundredth of an inch in diameter—in the brainpan of a single German paranoic ! But this is only a detail ; and many lines, both here and in previous works, show how much the author's art is strengthened by various science—the grain of fact and the fibre of observation ; for at every turn, in the midst of an ideal theme, he touches the concrete. High poetry like a mountain must be based upon wide tracts of solidity. "Humanities" which are not built upon science are nothing but clouds.

Though it be the sweetest string in life's harp, Beauty yet remains a mystery to us. All that science can tell us is that it seems to belong to the class of instincts which apprise us of the beneficial—given to us by evolution to help us in rapid choice. Thus personal and rural beauty might signal fertility, or at least some kind of merit—*formosa facies muta commendatio* ; pleasant tastes and odours, wholesomeness ; musical harmonies, family and progenitiveness. But we do not yet always see quite clearly with this light. There is the beauty of barren landscapes, even of the desert ; the sunset on the mountain or the cloud is not particularly good for us ; some forms of beauty, as of the sea, are indeed terrible ; while the sublimest, as of the dawn and the stars, seem divine because they are absolutely above us.

> Roses are beauty, but I never see
> Those blood drops from the burning heart of June. . . .

And why are they beautiful—what are they to us ? Then, shall we say that the red kine deep in the meadows do not know Beauty ; or the swallows ; aye, or even the tiger, or the worm in the mould ? These cells in my blood are not only beautiful in themselves, but, I expect, feel beauty of some kind ; and this foul parasite when dissected becomes a cosmos of beauty :

> Here in the flesh, within the flesh, behind,
> Swift in the blood and throbbing on the bone,
> Beauty herself, the universal mind,
> Eternal April wandering alone.

There is also the opposite of beauty, the Shadow—set forth as in Sonnets 10 or 38.

Regarding the artistry—most of the forty-eight pieces are sonnets consisting of three separate alternately-rhymed quat-

rains and a terminal couplet, as in Shakespeare's Sonnets, Spenser's *Ruines of Rome*, Thomas Watson's *Teares of Fansie*, and Michael Drayton's *Idea*. Regarding the art—the loose rhyming suits the theme. The thoughts are seldom caught all within the stanza like cupids in a cage; but are spirits, for ever appearing and dissolving in a moonlight mist where Beauty herself is the heavenly enchantress. They are lovely or terrible or both, but can never be foretold, either in form or place. Many bear each a gem—more often emerald or topaz, but sometimes ruby or even fire; and beneath it all there is the undertone of the divine melancholy—which is never melancholy—of music, like that of oaks in a midsummer-night's-dream of some supreme philosophy. We have here then an order of art other than the marble one or the flaming one—let us say of *In Memoriam* or *Empedocles*; a more easy art perhaps, but not less perfect in its designed imperfection. An art of the night-mist, which however melts into ineffable revelations, while the goings of the stars are brightened by it. . . . Interspersed among the sonnets are three beautiful lyrics, numbers 20, 36 and 41, in which the music resolves itself for a moment into pure harmonies. In it we hear the voices of both the divine sisters singing together.

A great theme and a fine achievement—fundamentally, absolutely germain to poetry, and yet much better than Plotinus set in verse. It is a quest of the Holy Grail of Beauty by the Sir Percival of poets.

To the Shakespeare Tercentenary Committee and especially to its Honorary Secretary, Prof. Israel Gollancz, Britain owes a debt of gratitude for having saved her the shame of almost ignoring Shakespeare's Tercentenary; and this sumptuous volume, containing the contributions of one hundred and sixty-six "homagers" of Shakespeare, is at least and at last something. The contributions, coming not only from littérateurs but from men of all types, give a survey of the world's opinion upon one of its greatest poets and men of science. The contributions are in verse and in prose, and nearly all excellent and interesting.

Many of them dwell upon what Mr. Austin Dobson expresses by calling Shakespeare "The Riddle of our race," and what Mr.

Hardy treats of in his opening verses, namely the fact that a mind of such immense merit was scarcely recognised by contemporaries. But there is really no riddle in this. War, the revealer, has held up the mirror to us as a nation and shown us what an extremely dull people we are. In fact, we have obtained the hegemony of the world partly by our exceptionally lucky insular position, partly by our iron and coal, but chiefly by the fact that we have produced an extraordinary amount of the commodity called genius. Yet our men of genius are always foreigners among the people, who generally treat them exactly as cruelly as Swift figured that the Laputans treated their immortal Struldbruggs ; and at the same time we attribute to our own merits the prosperity which has been really given to us by our great victims. This attitude was even apparent in the England of Elizabeth—though, owing to the more recent infusion of French blood at that time, our intellects were then distinctly more acute than they are now. Indeed the Elizabethan dramatists can scarcely be called English at all—they were more French, Italian, and Spanish. For example Prof. W. P. Ker indicates the close similarity between English and Spanish literature of that day, and we know how much Shakespeare and Spenser were indebted to Italian literature. The magnificence of the Elizabethan dramatists depended chiefly upon their logical and superb constructions, demanding a southern mental clarity which is scarcely known in the Britain of to-day, in which the power of construction has almost vanished since the time of *Clarissa Harlowe*. Shortly after the Elizabethan period, the mind of the country was darkened by the puritanism of the Rebellion and the snobbery of the Restoration, which became the Whiggism and Toryism of a later age and the Radicalism and Conservatism of the present—all based upon false theories of life. But even in the days of Elizabeth the people were still too dull to grip the reality of genius—the most wonderful phenomenon in nature ; so that they left us almost no record of their principal men. There is really no riddle about Shakespeare, because no notice would have been taken of him as a man whoever he might have been short of a peer or a politician. We know almost as little about Marlowe and Spenser and many others of that period—they were all Struldbruggs. It is impossible to understand why some people seem to think that such men of genius should ever have attracted

more attention among their contemporaries. If another Shakespeare were to exist to-day, he would be treated in exactly the same fashion, or even worse. We may be quite sure that our very minor reviewers would still call him a minor poet.

Viscount Bryce complains that we cannot even tell whether Shakespeare had any, and what, political opinions—as if such a mind could ever possess anything so contemptible, which, as I have said, did not begin to appear until an age of later degeneration. Viscount Bryce also remarks on the poet's want of interest in the fate of his own work, and fancies that he did not know how good that work was. But this homager has evidently failed to understand the interpretation of Shakespeare's final work, the *Tempest*—in which the poet clearly figures himself as Prospero and the English public of that time as Caliban. Shakespeare knew quite well that it was no good trying to make Caliban understand his master's virtues. He worked his magic and remained contented at that. In fact it was the very obscurity of his life which gave opportunity for the brilliance of his intellect. We can never at the same time both sit in high places and work in them.

Some of the verses in this fine collection are excellent, especially, I think, the two opening stanzas of Mr. Hardy's. And the prose contributions are full of points of interest and information. There are many contributions in foreign languages (even in Chinese), always well and sometimes beautifully translated. But it is impossible in the space at my disposal to comment on particulars; so that I had better conclude simply by recommending the work for the humble libraries of men of science. Prof. Gollancz is especially to be commended for his share in this work—and I may add that his Epilogue is, in my opinion, the best poem in the book. The portraits of Shakespeare and the other plates are worthy of the volume.

RECENT ADVANCES IN SCIENCE

PHILOSOPHY. By Hugh Elliot.

The study of philosophy has for a long time past fallen into disrepute among men of science. The long succession of philosophers and the almost unbroken uniformity of their failures have given rise to the belief that the questions with which philosophy undertakes to deal are insoluble, and that the search for ultimate solutions is a vain and unprofitable undertaking. There is indeed no question of the general validity of this conclusion. And yet it seems probable that the reaction against every form of overt philosophy has been too extreme. For in effect everyone has *some* philosophy of life, including even those who most strenuously advocate the agnostic position. It is impossible to attack metaphysical systems without using metaphysical weapons, a procedure which not even Huxley or Spencer could avoid. In Spencer the effort to do so is almost ludicrous. So intent was he upon slaying the dragon of metaphysics, that he remained unconscious of the fact that he himself was giving birth on the field of battle to a new metaphysical system, fully as untenable as those he intended to destroy. There is something inevitable and omnipresent about metaphysics, which, when combined with the total failure of all attempts to deal with it, leads to an attitude of impotent despair. Take for instance such a problem as the relation of mind to matter. There are some philosophers who say that all matter is mind, there are some who say that all mind is matter, and there are others who affirm that the problem is insoluble. Yet none of these three views can be justified even in appearance, without entry into the dangerous morasses of metaphysics. The very question itself presupposes a metaphysical theory. It sets out with the assumption that there are two fundamental and distinct things, which may or may not be brought into some relation; and that assumption constitutes already a metaphysical belief. Too often the attitude of so-called common-sense is merely a crude metaphysical system,

set up in opposition to more refined systems. The defenders of "common-sense" have been numerous in the history of philosophy; but their theories have scarcely survived longer than those of the less common non-sense which they attacked.

Are we then reduced simply to the avoidance of all such subjects? We are debarred from accepting any one of the numerous solutions offered us; we are debarred even from denying all possibility of knowledge, which would be the most consoling way of getting rid of the incubus. We may of course affirm that knowledge is impossible, but the moment we begin to give reasons for this opinion we find ourselves as hopelessly involved in absurdity as any of those system-mongers whom we profess to despise. What escape is there from this *impasse*? Must we simply put our tails between our legs and run to some more agreeable subject of contemplation?

Let us at all events first take a look round. When rational knowledge dawned among the Greeks, the universe presented to the eye of the philosopher a medley of incoherent facts and events; and he knew not what logical weapons were suitable to reduce them to order. Centuries, nay millennia, had to elapse before it was finally proclaimed and accepted that OBSERVATION and EXPERIMENT were the only methods by which truth could be attained; and that truth could not be attained by instinct, or by emotion, or by diving into the recesses of one's own soul. It is difficult now to appreciate the state of mind of those living prior to this revolutionary discovery; but to them it was far from obvious that the deliverances of the senses were more trustworthy than those of "thought" or emotion, or inspiration. The organisation of human knowledge soon followed: the natural sciences differentiated out from philosophy; and justified their methods so abundantly that the conviction ultimately arose that only through the medium of science could true knowledge be attained.

The discovery of the proper methods to find truth was certainly not less momentous in the history of civilisation than was the discovery of fire. Before the initiation of this epoch, all was vague and uncertain. Men attacked the most formidable of problems with the most futile of weapons. They sought for the origin of the universe with logical instruments which could not have explained the simplest fact of common life. There was a total disproportion between the objects at which

they aimed and the methods which they used. David might have killed Goliath with a pebble; but the early philosophers thought to slay the colossus of the Universe with a grain of sand.

May it not be the case that the apparently insuperable difficulties of philosophical problems are due to a similar disproportion between the object aimed at and the weapons employed? Before the age of science, all forms of natural knowledge were classed together as philosophy. As the various sciences grew out, the appellation of philosophy ceased to be applied to them, but was still retained for those various subjects of inquiry which had given birth to no distinctive science. It did not follow, and it does not now follow, that such subjects are necessarily beyond the range of the human intellect; but only that the proper weapons have not yet been forged for dealing with them. They are still the shuttlecocks of alert imaginations, just as the problems of biology were bandied about by theology and metaphysics before they were reclaimed for their rightful science and examined by the light of approved and well-tried methods.

The position at which we have arrived, in short, is this: The range of subjects for human inquiry is infinite; human curiosity knows no bounds; while yet our instruments of discovery are few and imperfect by comparison. We are beginning also to see that it is useless to attack a mountain with a pick-axe; we are acquiring a sense of proportion between the end aimed at and the means employed. Further, we have at last become aware that the only means which offer any hope for the discovery of truth are those comprised under the general title of science. We know this empirically; no secrets of nature have ever been revealed by the so-called methods of metaphysics; the results of science and the results of metaphysics stand in too conspicuous a contrast for us to mistake the lesson conveyed. All hope of knowledge about philosophical problems depends therefore upon their incorporation into the body of science.

Now the advance of science can only be gradual. We are surrounded by infinite darkness, into which science thrusts out in all directions its tentacles of light. The great problems of philosophy loom out of the darkness, but we still cannot tell as a rule whether their solution is far or near. It is useless to stare at them in the dark; we can only wait till the growing

light of science at last reaches them, and some tentacle pushed forth at hazard suddenly lights up the scene which had been wrapped in impenetrable gloom. Until that occurs—and with many problems it may never occur—we must be content to leave these questions in the region of the Unknown, while yet chary of asserting that they are unknowable. To say that a thing is intrinsically unknowable already implies some knowledge of it. It implies that science has already advanced so far as to see that the chasm between it and us is impassable, and it often happens that we are unable consistently to affirm even that modicum of knowledge on the subject. At all events, we shall in the course of these papers on the progress of philosophy take our stand on the proposition that through the medium of natural science alone can the solutions of philosophical problems be discovered. When any such problem is presented to us, our first concern therefore must be to ascertain to what branches of science it may be amenable; and if we find it amenable to none, then we have no resource but to leave it in the darkness, hoping that at some future time the dawn may illumine that, as it has so many other of the paradoxes of philosophy.

From this point of view, let us approach the problem of the relation of mind and matter, on which various works have lately been published. The point of view of animals and of young children is purely objective; their regards are turned outwards rather than inwards; it would be true to say of them that their knowledge is of matter only, and not of mind. Later in evolution comes the consciousness of internal sensations, as apart from objective matter. At length the philosopher Berkeley pointed out that it is only through the medium of the senses that external objects are known at all. We are not directly aware of the object, but of a sensation only, which we interpret to be an external object. Hence the world consists of sensations and feelings alone; what else there may be is a matter of inference, and not of immediate experience. Thus originated the theory of idealism, which received a far more perfect expression in the hands of Hume.

The new theory quickly fell under the scourge of the disciples of common-sense. Dr. Johnson thought to refute it by kicking a stone, as though anyone had denied that this exercise would produce a sensation of hardness and external reality. More

important was the so-called Scottish school of common-sense, which urged that it was a plain fact, not to be traversed by any metaphysical subtleties, that there are two separate things, mind and matter, which can by no juggling be reduced to one. No more excellent idea of the tenets of these philosophers can be conveyed than that set forward in the work of Mr. G. A. Johnston (*Selections from the Scottish Philosophy of Common Sense*. Edited, with an Introduction, by G. A. Johnston: Open Court Publishing Co., 1915), who gives long extracts from the work of Thomas Reid, Adam Ferguson, James Beattie, and Dugald Stewart; and provides a very discerning introduction to the doctrines of these authors. The school of "realism" which they founded, however, owed its popularity far more to its harmoniousness with public sentiment than to any true value or insight of their writings. They were fully as metaphysical as Hume, whom they attacked; but they defended religion against the scepticism of that philosopher, they insisted upon a divine purpose running through the Universe, they set forth in philosophic language all that the public most wished to hear, and they carefully avoided all the genuine arguments brought forward by their great antagonist, whose philosophy was far deeper and, it may be added, far more Scottish than their own.

Realism, however, is not necessarily condemned by the arguments of the primitive realists. In our own day we have a new realism, advocated by Mr. Bertrand Russell (*Our Knowledge of the External World*. By Bertrand Russell, F.R.S.: Open Court Publishing Co., 1914), which is of quite another character from that of Thomas Reid. For us, the most interesting aspect of the new realism is that it endeavours to draw the whole problem within the purview of science. Mr. Russell proposes to treat it by mathematics and logic; there are, however, many who think that physiology offers a more hopeful prospect for the future. The conflicting views on this subject must be postponed for further discussion in a future paper.

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

THE subdivisions of any science must be more or less arbitrary. In the case of mathematics it is quite often fairly easy to separate such subjects as geometry and applied mathematics,

but there is some difficulty in deciding what shall appropriately be called "analysis" or "algebra" or "the principles of mathematics." In these short quarterly reviews, in which applied mathematics is not as a rule considered and in which only very interesting facts or new methods and views in geometry are noticed, we shall follow the plan of including under "analysis" all those considerations dealing with the usual mathematical conception of a function, under "algebra" all those parts of analysis which do not depend on this conception, and shall treat all questions about logical functions under "principles of mathematics."

An account of the life and work of F. W. Frankland (1854–1916), whose most important work was on non-Euclidean geometry, was given in *Nature* (1916, **98**, 38).

The name of Dr. George Sarton, who founded in 1913 the Belgian international quarterly *Isis*, devoted to the history and organisation of science, and edited it till he was bound to leave Belgium by the German invasion of 1914, is probably known to most of the readers of SCIENCE PROGRESS. In the *Monist* (1916, **26**, 321) he has an excellent article on the general history of science regarded from the scientific, pedagogic, and humanistic points of view, which ends with the words: "I hope that one of the great American universities will . . . organise an institute where all information on the history of science could be centralised, studied, and diffused again. Will America give this great example to the world? I earnestly hope so." Before this article was published, it was pleasant to read (*Bull. Amer. Math. Soc.* 1916, **22**, 474) that Dr. Sarton had been appointed lecturer on the history of science at Harvard University. In 1916–18 he will lecture on the origin and development of Greek science, the principles of mathematics historically considered, and the principles of mechanics historically considered.

History.—J. L. Heiberg (*Scientia*, 1916, **20**, 81) gives an admirable sketch of the position of Archimedes's work in the history of mathematics, and of the adventures of some of his manuscripts. F. Arendt (*Bibl. Math.* (3) 1915, **14**, 289) studies the development of Archimedes's terminology and the chronological sequence of his writings.

In the history of non-Euclidean geometry, Johann Heinrich Lambert's theory of parallels is of great importance, and K.

Bopp (*Sitzungsber. der K.B. Akad. der Wiss. zu München*, 1914, 361) deals with the opinions on it of Lambert's contemporaries. Another landmark in the history of non-Euclidean geometry is formed by the (much earlier) work on parallels of Girolamo Saccheri, and A. Pascal (*Giorn. di Mat.* 1914, **52**, 229) gives an account of Saccheri's life and work.

Principles of Mathematics.—Dr. B. A. Bernstein (*Bull. Amer. Math. Soc.* 1916, **22**, 458) simplifies E. V. Huntington's (1904) set of postulates, based on A. N. Whitehead's " formal laws " for Boole's algebra of logic, by reducing the number of postulates from ten to eight and the number of postulated special elements from three to one.

A new subject whose foundations can be treated by means of a system of axioms is the method of least squares. F. Bernstein and W. S. Baer (*Math. Ann.* 1915, **76**, 284) use, for this purpose, the conception of " weight " introduced by Gauss in his second formulation of the foundations of this method, and make no use of the conceptions of the calculus of probabilities.

Dénes König, the late Julius König's son, has published (*Mathematikai és physikai lapok*, Budapest, 1914, **23**, 291) some remarks on his father's last work, *Neue Grundlagen der Logik, Arithmetik und Mengenlehre* (cf. SCIENCE PROGRESS, 1916, **11**, 92). References to reviews of this work are given in *Revue semestrielle* (1916, **24** [1], 106).

K. G. Hagström (*Arkiv för Math.*, Stockholm, **10**, No. 2) claims to have proved the existence of well-ordered aggregates which are not comparable. This would of course destroy the force of the antinomy of Burali-Forti (1897).

F. Hartogs (*Math. Ann.* 1915, **76**, 438) proves, without using Zermelo's principle of selection, that aggregates which are " comparable " (that is, one of the two given aggregates has a part which is equivalent to the other) can be well-ordered. This account (*Rev. semest.* 1916, **24** [1], 32) is not quite clear: it seems possible that a given aggregate may be comparable with a certain second one, but not with a certain third one. Further, it has long been known that Zermelo's axiom, which is necessary and sufficient for well-ordering in general, is needed only in that one of the four logically possible cases of the comparison of aggregates which is that of supposed incomparability.

G. Pucciano (*Giorn. di Mat.* 1914, **52**, 19) continues the work he published in the same journal for 1913 on the open linear

and homogeneous continuum and the Archimedean geometry of the straight line.

C. Carathéodory (*Gött. Nachr.* 1915, 404) transfers to the concept of length the theories of Lebesgue for the content of point-aggregates and gets a generalised concept of length.

A. N. Whitehead (*Rev. de Métaphys. et de Morale*, 1916, **23**, 423) develops the relationist theory of space by the help of a fairly frequent use of the symbols of Whitehead and Russell's *Principia Mathematica.* On p. 427 there is an interesting argument against "transmission of action by the contiguous parts of a continuous medium," which results simply from pointing out that there are no contiguous points in a continuum. But we could in like manner show that "motion from one point on a line to the next one" is impossible; and yet continuous motion is not impossible in a continuum. However, the real objection to the denial of action at a distance does not depend on the arguments derived from that remark, but lies in the fact that the denial in question is a denial of direct relations between physical objects not occupying the same point—and this implies the negation of the theory of the space-relation. Whitehead gave a short and lucid account of that department of logic with which some of his own studies have been connected in his Presidential Address to Section A of the British Association at Newcastle in 1916. It may be noticed that, in the abridged account of this address given in *Nature* (1916, **98**, 50), this part is omitted.

Algebra (including Theoretical Arithmetic).—F. Bernstein and O. Szász (*Math. Ann.* 1915, **76**, 295 and 485) give an interesting criterion for the irrationality of infinite continued fractions.

F. Tavani (*Giorn. di Mat.* 1914, **52**, 204) sets out, in a general theory of series, from the principle of comparing the *n*th term with the $(n+1)$th, instead of the usual principle of comparison.

Major P. A. MacMahon (*Proc. Lond. Math. Soc.* 1916, **15**, 314) gives applications of general theorems in combinatory analysis to (1) the theory of inversions of permutations; (2) the ascertainment of the numbers of terms in the development of a determinant which has amongst its elements an arbitrary number of zeros.

Analysis.—Of course there is a wide difference between what we call "limits" and "bounds" (*Grenzen* and *Schranken*

in German) and Moritz Pasch's important paper of 1887 will be remembered in this connection. Lately, Pasch (*Monatshefte für Math. und Physik*, 1915, **26**, 303) has returned to and further illustrated the subject.

L. L. Silverman (*Trans. Amer. Math. Soc.* 1916, **17**, 284) discusses some points in the extension, first carried out by Paul du Bois-Reymond in 1887, of Cesàro's and Hölder's definitions of summability of series to the case of functions of a continuous variable.

In 1914 C. A. Fischer gave a definition of the derivative of a function of a surface which is analogous to Volterre's definition of the derivative of a function of a line, and proved that if the derivative is continuous and approached uniformly, the first variation of the function is equal to the double integral of the derivative multiplied by the first variation of the dependent variable. Fischer now (*Amer. Journ. Math.* 1916, **38**, 259) extends the theory to the case where there are points or curves where the derivative does not exist, and applications are made to problems of the calculus of variations.

G. H. Hardy (*Trans. Amer. Math. Soc.* 1916, **17**, 301) gives a new method, which is less elementary but considerably more powerful than those used hitherto, for the discussion of Weierstrass's continuous but not differentiable function and similar questions. Several very general results are proved concerning Weierstrass's function and the corresponding function defined by a series of sines, and towards the end of the paper are : (1) a simple example of a function represented by an absolutely convergent Fourier series which does not satisfy a "Lipschitz condition" of any order for any value of x ; (2) a proof of S. Bernstein's theorem (1914) that if $f(x)$ satisfies a Lipschitz condition of order greater than one-half throughout an interval, its Fourier series is absolutely convergent, and one-half is the least number which possesses this property ; (3) a discussion of the function which, according to Paul du Bois-Reymond (1875), was stated by Riemann to his pupils to have no finite differential quotient for any one of an everywhere-dense set of values of x : Hardy proves Riemann's assertion and a good deal more.

An example of the need which mathematicians now feel for proving "obvious" things is the demonstration, by E. Sciolette (*Giorn. di Mat.* 1914, **52**, 84), that if $f(x)$ is positive

(not zero) from a to b, the integral from a to b of $f(x)$ is also positive. Further, if Sciolette's demonstration is not possible, $f(x)$ is not integrable.

One of Weierstrass's fundamental theorems in the theory of analytic functions is that a function-element which can be continued along every path within a simply connected region defines a regular one-valued function for that region. A. Pringsheim (*Sitzungsber. der K.B. Akad. zu München*, 1915, 27 and 58) proves this theorem in a very simple way by means of approximating step-like polygons (*Treppenpolygone*).

Prof. W. F. Osgood (*Bull. Amer. Math. Soc.* 1916, **22**, 443) extends the scope of a fundamental theorem on the continuation of analytic functions of several variables and simplifies its proof. A case of the theorem was given by Kistler in 1905, and a more general theorem by Hartogs in 1906; but both these proofs involve n-ple integrals, whereas that of Osgood makes use of only Cauchy's integral-formula for functions of a single variable.

Weierstrass stated the theorem that a function of n complex variables which is meromorphic at every point of space is rational; and he understood by "space" the extended space of analysis—the n spheres of the n complex variables. But the theorem is true for other spaces, and Prof. W. F. Osgood (*Trans. Amer. Math. Soc.* 1916, **17**, 333) accordingly lays down a general definition of infinite regions which will include the cases of projective geometry, the geometry of inversion, the geometry of the space of analysis, and so on.

G. D. Birkoff (*Trans. Amer. Math. Soc.* 1916, **17**, 386) shows that the classical results of Weierstrass and Mittag-Leffler on the formation of infinite products of functions with assigned singularities admit of a natural extension to infinite products of matrices. The importance of this lies in the fact that, in a large part of the theory of functions of a single complex variable—such as for the functions defined by linear difference and differential equations—the matrix of analytic functions rather than the single analytic function must be taken as the fundamental element.

R. D. Carmichael (*Trans. Amer. Math. Soc.* 1916, **17**, 207) lays the foundations of the theory of a large class of series which include the factorial series and are suitable for the representation of functions which are defined throughout a half-plane and

have certain types of singularities at infinity. These researches are connected with some work of Poincaré (1885, 1886), Nörlund (1914), and the theory of Dirichlet's series.

H. Bohr (*Jahresber. der Deutsch. Math. Ver.* 1915, **24**, 1) gives a report on the present state of the theory of Riemann's Zeta-function from the point of view of the theory of functions. E. Landau (*Math. Ann.* 1915, **76**, 212) gives some researches relating to Hardy's discovery of an infinity of zeros of the Zeta-function with the real part ½, and obtains a great simplification of Hardy's proof.

On the theory of Riemann's Zeta-function we may also refer to a paper by H. Bohr (*Acta Math.* **40**, 67).

Dr. C. R. Dines (*Proc. Lond. Math. Soc.* 1916, **15**, 243) discusses points arising from Prof. E. H. Moore's paper of 1912 on the foundations of the theory of linear integral equations and which are connected with Moore's important and well-known "general analysis" of 1906.

Geometry.—E. M. Langley (*Math. Gaz.* 1916, **8**, 268) gives a long and valuable illustrated account of Dr. Max Brückner's elaborate work *Ueber die gleicheckig-gleichflächiger, discontinnerlicher und nicht-konvexen Polyheder.*

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

Stellar Clusters.—Several researches of considerable interest and importance in connection with stellar clusters have recently been published which are summarised below.

A second paper on the theory of star-streaming and the structure of the universe by J. H. Jeans appears in *M.N., R.A.S.* lxxvi. 1916, p. 552. In the earlier paper, summarised in these notes (Science Progress, April 1916, p. 621), he showed that the observed laws of star-streaming could not be explained or interpreted if it was supposed that the universe had reached a steady state. He therefore seeks an explanation for them in collisions between two or more clusters of stars, conceiving the universe as akin originally to a gas, each molecule of which is a compact cluster of stars. Such a collision will however not lead to a rebound as in the case of two molecules, but the clusters will intermingle and pass through one another, the stars in each being affected by the attraction of those in the other. A repetition of this process will gradually lead to the

disintegration of the clusters. In this paper he attempts to trace the early history of such a collision. Considering first the simpler case of a cluster meeting a uniform shower of stars, it is found that it will thereby be spread out so as to be almost disc-shaped, reminiscent of the form of the Ursa Major cluster. If now there are two equal collisions in perpendicular directions, star streaming is set up in the mutually perpendicular directions, and the resulting law of distribution of velocities is Schwarzschild's law. Under less simple conditions this law is still found to hold. If again the second colliding cluster is smaller than the disc on which it impinges, there will be a flat outer annulus of stars more or less unaffected by the second encounter, and the effect will be to produce a lens-shaped centre surrounded by a galactic ring, somewhat similar to what we find in our own universe.

This paper gives a remarkable view of the ways in which our universe might have been formed, explaining also the observed phenomena of star-streaming. Although but a rough model, it is extremely suggestive and comprehensive. It can of course only be accepted if we are willing to accept the island-universe theory (that our stellar system is but one of many universes scattered through space), but there are many other reasons to believe that this is a correct view; the fact also that only on this view has it been possible to formulate a logical theory of the formation of our stellar system may be regarded as considerably strengthening the theory.

A. S. Eddington, *M.N., R.A.S.*, lxxvi. p. 525, 1916, proves that in any star cluster in a steady state the internal kinetic energy is one-half the exhaustion of potential energy, a useful result which has an analogy in gas theory. This result is applied to determine the rate of dissolution of a cluster, which naturally tends to expand, taking into account the mutual attraction of the cluster. Eddington has shown that the Taurus cluster could not have existed more than 57 million years unless the scattering had been counteracted in some way; the present considerations indicate that the time taken by the cluster to increase from one-half its present size to its present size is of the order of not less than 200 million years.

A. S. Eddington, *M.N., R.A.S.*, lxxvi. p. 572, 1916, also discusses the significance of the law of density $\rho = (c^2 + r^2)^{-5/2}$ found by C. Plummer and H. von Zeipel to govern the dis-

tribution of stars in globular clusters, r being the distance from the centre. This law is found to satisfy observations very closely except at the centre and towards the edges of a cluster, and it would seem that there must be some theoretical significance underlying it. The cluster is usually compared with a sphere of gas in adiabatic equilibrium under its own attraction, each star representing a molecule. The density law will in such a case depend upon the value of γ, the ratio of the two specific heats. Plummer's law is obtained for the special case in which $\gamma = 1{\cdot}2$, and von Zeipel has shown that a very small deviation from this value is sufficient to spoil the agreement. For four special clusters examined, the values of γ determined so as to give the best agreement ranged from $1{\cdot}194$ to $1{\cdot}203$.

Eddington points out objections to Plummer's explanation of this result, that the cluster has developed from a nebula for the constituents of which γ had the value $1{\cdot}2$; it is hardly likely that the distribution would remain unaltered as the stars formed. Von Zeipel supposed that the stars behaved like the molecules of a gas. The objections to this are (1) that the law should in that case be that for isothermal and not for adiabatic equilibrium; (2) even if adiabatic, the value of γ might be expected to be $1{\cdot}67$ as for monatomic molecules; (3) and even taking into consideration double stars the value should not fall below $1{\cdot}40$.

If ϕ is the potential, Plummer's law is a particular case of a more general law $\rho = \phi^n$, where $\gamma = 1 + 1/n$, obtained by putting $n = 5$. Eddington shows that from this point of view it has some significance, because when $n < 5$, the gas is in equilibrium in a globular form with a spherical boundary, when $n > 5$ the mass of the cluster is infinite, whereas only for $n = 5$ is the mass finite, but the cluster extends to infinity. This is rather an important conclusion. The problem is then further examined from the dynamical point of view; when $n > 5$ no equilibrium is possible, for $n < 5$, there is a quite arbitrary limiting velocity, but for $n = 5$ the limiting velocity in the cluster is the velocity of escape.

After all, however, the law $\rho = \phi^n$ is itself only a very particular relation, and on examining a more general law than this, no reason is found why the Plummer's law should be obtained. Many other distributions can be obtained which

will satisfy the necessary conditions of a steady state with a random velocity distribution and with a limiting velocity equal to the velocity of escape. Other considerations are examined, but the general conclusion is that no obvious theoretical explanation can be found for the observed law.

J. H. Jeans, *M.N., R.A.S.*, lxxvi. p. 567, 1916, has handled the same problem but in a different way. Jeans had already shown that a cluster of stars, free from external influences, can only be in a steady state provided (1) that there is star-streaming, and that the figure is not necessarily spherical, or (2) that there is no star-streaming, when the figure must be spherical. Taking this latter case, a general law is obtained for the density, depending upon the form assumed for the gravitational potential. Under special assumptions this law is found to reduce to Plummer's law, but these assumptions are artificial in nature and such as to lead to a divergence at the centre and edges of the cluster, at the places in fact where Plummer's law gives the worst agreement. Jeans finds that, in general, at the outer edges the law of density should be $\rho = kr^{-4}$, which appears to agree with the counts better than does Plummer's formula. Beyond that it does not seem possible to pick out any definite features common to all clusters in the law of distribution; the law depends upon a number of constants and there is no reason why these should be given arbitrary values.

H. Shapley, *Observatory*, xxxix. p. 452, 1916, who has gathered much observational material in connection with globular clusters, discusses the counts from which Plummer and von Zeipel have deduced their density law and raises two queries in regard to them, viz. (1) are the stars counted sufficiently representative to give even a rough indication of the structure of the whole cluster, and (2) do the counts give the true distribution of the stars brighter than a definite magnitude? Photographs taken with the large 60-inch reflector at Mount Wilson have established the two following facts: (1) that there is a large range between the extremes of brightness of the stars in the clusters, probably not less than ten magnitudes at least, and (2) that as the magnitude decreases there is a corresponding decrease in the colour index. These results mean that in the counts which have so far been made the brightest stars only have been counted, which probably

constitute fewer than one per cent. of the whole cluster, and that these stars are also the reddest stars and belong largely to one spectral class. Their large luminosities may be associated with other peculiarities, such as peculiarities of mass or of motion, which may affect the observed densities and give fictitious results.

He points out also the danger of systematic errors in counting dependent upon the distance from the crowded centre and the possibility also of errors due to the photographic interaction of contiguous images, and error varying with the distance from the centre and causing the limiting magnitude correspondingly to vary.

Shapley also remarks in reference to Jeans's theory, referred to above, that observations have not as yet either proved or disproved the existence of galactic planes in globular clusters. The appearance of the few brightest stars is not sufficient to decide, because these may be uniformly distributed, but the fainter stars may be arranged in a belt. He finds some tendency in ω Centauri towards a cluster galaxy, but it is not very pronounced. We await with interest further observations in this direction to see how they agree with theory, which, for once, has outrun observation.

PHYSICS. By JAMES RICE, M.A., University, Liverpool.

Relativity and Gravitation.—The first of the two organised discussions arranged for the Mathematics and Physics Section of the British Association was devoted to the subject of Gravitation.

A considerable part of the discussion was occupied with the recent work of Einstein and Grossmann which attempts to bring gravitation within the scope of the principle of relativity. This latter principle, now widely accepted, denies the possibility of determining *absolute* motion in space by means of any physical phenomenon. The various attempts to determine the velocity of the earth relative to an assumed immovable ether and the consistent failures of these experiments to detect such relative motion either by optical or electrical methods, have accumulated a considerable body of evidence in favour of the principle.

In Einstein's earliest formulation of the principle of relativity (*Ann. der Physik*, xvii. 913 1905) he stated it in these

words :—The laws of nature are independent of the state of motion of the system of reference provided this is unaccelerated. This means in more particular terms, that if two sets of observers were stationed in two different worlds, and each set made ideally careful and precise measurements on all natural phenomena, then they would each summarise their observations in precisely the same laws, provided each of the worlds was moving *uniformly* with respect to the other. So it would appear that natural phenomena would fail to decide as to which of the two worlds was absolutely at rest in space and which in motion. Such phenomena could only yield a measure of their relative motion. Indeed, as far as purely mechanical phenomena are concerned the principle is as old as Galileo, and is employed by every student who works out a problem in Rigid Dynamics involving the use of "Moving Axes." It was the continued failure of optical phenomena to reveal any difference in the measured velocity of light, whether the light was travelling in the direction of the Earth's motion, or against it, or across it, which led Einstein to extend the principle, so as to include all phenomena, optical and electromagnetic, and in particular to enunciate the special case of relativity known as the principle of the constancy of the velocity of light, viz. that the velocities of light measured with respect to two different systems of reference will be the same if the two systems are in *uniform*, relative motion one to the other. It follows from this postulate that in transforming from a set of axes fixed in one system of reference to a set of axes fixed in the other, not only are the co-ordinates of a point with respect to the one set linear functions of the co-ordinates referred to the other set and the time (which is to be expected), but the time of the occurrence of an event at this point in the first system of reference is also a linear function of the co-ordinates of the point in the second system and the time of the event in the second system. It is this second fact which constitutes such a novel departure from our ordinary notions as regards the invariability of the time measure of an occurrence. Further it appears that even the linear relations connecting the co-ordinates in one set with the co-ordinates and time in the second set, are not those derived from the usual "principle of moving axes" as employed in works on Dynamics. In fact these relations are so modified that they have precisely the same form, whether one is trans-

forming from the first set of axes to the second, or from the second to the first, and thus, as stated above, it is impossible by means of such relations to determine which of the two systems of reference (if any) is at absolute rest; they merely indicate a relative motion of one system to the other with a velocity uniform in amount and direction. The existence of these relations leads to a number of results which seem paradoxical at first to our preconceived ideas concerning space and time; thus it appears that when a body originally at rest *in a definite system of reference* is set in motion, its dimensions measured relative to axes in this system are contracted in the direction of motion and unchanged in planes perpendicular to this; its dimensions relative to a system of reference moving with it are the same as they were originally. But this result is precisely the assumption which Lorentz and Fitzgerald introduced a few decades ago to account for Michelson's failure to detect the (assumed) influence of the Earth's motion on the velocity of light. A further deduction from these relations concerns the time registered by a clock between two definite occurrences. If this interval be measured by two clocks each one fixed in one of two systems of reference in relative motion, the two observations will not agree; in fact, if the pendulum of one of the clocks could be observed from the other system, it would appear to have its period increased beyond the value it has when measured by observers at rest relative to it. The effects referred to depend on the square of the ratio of the relative velocity of the two systems to the velocity of light, and it appears hopeless at present to obtain (say) two platforms moving on the Earth's surface with a relative velocity large enough to produce these anticipated results in large enough magnitude to be measurable. Einstein's views and the four-dimensional interpretation given to them somewhat later by Minkowski present the concepts of space and time not as two distinct modes of perception, but as two closely related aspects of reality. Very lucid and sound accounts of the principle may be found in Campbell's *Modern Electric Theory* (2nd edition) and Richardson's *Electron Theory of Matter*. A very complete exposition is given in Cunningham's *Principle of Relativity*.

The work with which the British Association meeting was immediately concerned was the recent attempt of Einstein to include gravitational phenomena within the scope of a " General-

ised Relativity Theory." The inclusion of gravitation within any of the recognised electric theories has always presented considerable difficulties, based in all probability on the fact that the known laws of gravitational action are so exceedingly simple, paradoxical as that statement may appear. As the subject left Newton's hands, no question was raised as to the possibility of the gravitational action of one body on another being an influence which requires time for its propagation from the one body to the other. With the advance of elastic solid and electromagnetic theories of the ether, such views as to the propagation of gravitation through space with a finite velocity naturally came forward, and when Einstein's relativity theory began to be accepted with its necessary restriction that nothing could travel faster than light, the question was seriously raised as to the possibility of reconciling astronomical phenomena with the view that gravitational action was propagated through space with the velocity of light, a view which would accord with the theory that gravitation is the result of an "uncompensated residue of elctrical forces" (to quote Prof. Richardson). Now Lorentz has considered this question very fully, and finds that if gravitational action is propagated in the same manner as electrical actions, it will give rise to effects practically identical with those which follow from the usual Newtonian law. The differences are too small to be detected, and they are incapable of accounting for certain recognised discrepancies in the motions of the heavenly bodies. Relativity theory would therefore treat Newton's theory as a first approximation, but without any new experimental indications as to the direction in which an extension is to be sought; as mentioned, the simplicity of the known gravitational laws permit, as it were, too much freedom; there is but little restriction on the number of possible assumptions and insufficient definition of the direction in which progress is to be made.

In his "Generalised Relativity Theory" Einstein makes an attempt to develop gravitational theory in a manner consistent with known facts, so as to bring it within the scope of electromagnetic actions; in doing so he has scored a notable success in removing one of the most serious discrepancies known between astronomical theory and fact. In the earlier relativity theory Einstein assumed the equivalence of two systems in uniform relative motion as regards the summarising of phenomena in

laws. In the new development he assumes the equivalence of a homogeneous field of gravitation and a system of reference with uniform *acceleration*. The former is a region where the observer is at rest, relative to a stellar body whose attraction causes all free bodies to move towards it with an acceleration uniform throughout the region considered. The latter is a system in which the observer is at rest, *but freed from the attraction of the star*, system and observer moving with a *uniformly accelerated velocity* relative to the star. Einstein's hypothesis is that these two systems are physically identical, and that it will be impossible to distinguish between them by the formulation of the laws concerning phenomena as observed from each system. This amounts in fact to a denial of the possibility of detecting absolute acceleration, just as the relativity principle denies the possibility of detecting absolute velocity. The equivalence of the two systems is obvious enough, so long as we confine ourselves to Newtonian Dynamics, just as a limited case of relativity was in constant use even from the time of Galileo. Thus, to take the illustration which has been frequently pressed into service to assist the beginner in grasping Einstein's idea, consider an observer situated in a closed lift, observer and lift being free from the attraction of any gravitating matter. If the lift were in uniform motion, there would be no pressure between the lift floor and the observer's feet, in fact by a gentle spring the observer could " float up " to the ceiling of the lift ; bodies placed anywhere in the lift would remain there without support —in short there would be no " up " or " down." If now the motion of the lift was accelerated all the mechanical phenomena which we associate with a field of gravitation would supervene. If the acceleration is maintained in a direction which is " upwards " (in the sense from feet to head), the lift floor would exert a pressure on the observer's feet in proportion to the magnitude of the acceleration ; bodies would " fall " (*i.e.* move towards the lift floor) with an acceleration *relative to the lift* equal in magnitude and opposite in direction to the acceleration of the lift. Now Einstein's hypothesis amounts to this : suppose the observer can observe phenomena through windows in his lift and make precise measurements, then all events, optical, electro-magnetic, etc., as well as mechanical, will follow the same course for him as they do to the observer stationed on the gravitating star. The first deduction from

this is of extreme interest, for to the observer in the accelerated lift a ray of light (which would be straight to an observer in a uniformly moving system) would appear curved, the deflection from the straight path being opposite to the direction in which the lift is accelerated. If, as Einstein supposes, this will also be the appearance to the observer fixed on the gravitating star, then we reach the important conclusion that the passage of a ray of light near to a gravitating body should produce a bending of the ray towards the body. The curvature to be expected is extremely small—amounting to a change of direction of 1·7″ in the case of a star seen close to the sun's limb—and it is impossible so far to prove or disprove the hypothesis directly. However, Einstein has succeeded by the development of the idea, thus roughly sketched, in throwing the laws of motion, of electro-dynamics, and of gravitation into a form which makes the sequence of phenomena entirely independent of any particular framework of reference, and he has been able to predict therefrom a motion of the perihelion of Mercury amounting to 43″ per century—just the amount of a motion which has hitherto been regarded as a serious discrepancy between fact and the usual Newtonian theory. Those who are interested will find an elementary account of the matter in Chap. XXII. of Richardson's recent book *The Electron Theory of Matter*. For those who can follow the mathematical development of the relativity principle, an explanatory paper by Dr. Fokker in the *Phil. Mag.*, Jan. 1915, will prove useful; full references to the original literature are given in this paper. Another remarkable consequence from Einstein's theory is pointed out by Fokker in the paper referred to. It was remarked by Mach in his *Principles of Mechanics* that we cannot speak of mass in an absolute sense—mass is a relative measure of the inertia of bodies, determined in the case of two bodies by their relative accelerations at an instant when they are mutually acting on each other, and free from other influences. This should not be confused with the idea of gravitating mass, the measure of which makes its appearance in the usual Newtonian law of gravitation. Mach concluded that we are not justified in thinking of the mass as of something absolute belonging to a body, but that it may be due to some inducing influence of bodies one upon the other. If this be so we should expect to find that the inertial mass of a particle is increased by the

heaping up of other particles in its neighbourhood, and that an accelerated body induces an acceleration of the same direction in other bodies. These early speculations of Mach follow as direct conclusions from Einstein's theory.

At the same section, Dr. P. E. Shaw gave an account of his experiments, which appear to indicate a change in the gravitation constant with the temperature of gravitating bodies. Dr. Shaw has already published a statement of his work in the *Phil. Trans.* A. 216 (1916). The experimental work has been of extreme delicacy and has, it would appear, been going on for close on ten years, some earlier types of apparatus having proved of no avail in the research and having to be abandoned. The apparatus ultimately used was of the Cavendish torsion-balance type in the form adopted by Boys in his researches on the value of the gravitational constant. The suspended system was enclosed in a vacuum vessel in which the pressure of the residual air was varied from about 1 micrometre of mercury for some experiments to as high as 15 millimetres for others. The large masses of lead whose gravitating effect on the suspended system was being observed had their temperatures altered by electric heating between limits 15° C. and 250° C., the suspended system being maintained at ordinary laboratory temperatures. The utmost precautions were taken to guard against spurious effects due to electrostatic or magnetic action, or to convection in the residual air or its radiometric pressure, or even to the radiation pressure of the lamp used to illuminate the mirror on the suspended system. In the first instance it would appear desirable to test if the gravitation constant varied according to some expression linear in the mean temperature of the attracting masses (each temperature being "weighted" by the corresponding mass in obtaining this mean). On account of the large mass of the lead spheres or cylinders in comparison with that of the suspended system, this mean temperature would be practically that of the lead bodies. Dr. Shaw is unable to assert from his experiments that such a linear relation does hold, but he states that he has been able "(*a*) to obtain consistent cyclic readings in a gravitational experiment of the Cavendish type, even though the large masses are maintained for hours above 200° C., while small masses remain at ordinary temperature; (*b*) to carry on this investigation in the centre of a city at any time by day or night, in spite of the attendant

tremors and the special disadvantage of having the torsion balance in a vacuum"; and further to conclude that there is a temperature effect of gravitation. "When one large mass attracts a small one, the gravitative force between them increases by about 1/500 as the temperature of the large mass rises from, say, 15° C. to 215° C. At present the result is provisionally stated as being $+1.2 \times 10^{-5}$ per centigrade degree; but the readings are not steady enough to justify the statement that there is a *linear* relation connecting the gravitation constant and temperature."

Dr. Shaw, in a short review of the previous work on the constant of gravitation, points out that, though much of it yields no evidence one way or the other on this point, yet a part of it does lend some small weight to the view that there is a positive temperature effect for gravitational attraction.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Electrolytic Dissociation (*continued*).—Having considered the behaviour of HCl, it is of interest to compare it with that exhibited by KCl, especially since KCl is supposed to be more normal in its behaviour in respect of ionisation-values as given by conductivity measurements. Accurate electromotive force measurements of concentration cells, involving KCl, have been recently carried out by MacInnes and Parker (*J. Amer. Chem. Soc.* **37**, 1445, 1915) at 25° C. which agree well with those carried out by Jahn at 18° C. The following table contains the values of a and γ for this substance. The assumption is made that at 0·001N, $a = \gamma$.

ACTIVITY COEFFICIENTS AND CONDUCTIVITY-VISCOSITY RATIOS OF KCl AT 18° C. OR 25° C.

Moles. KCl in 1,000 grms. H_2O.	a.	γ.
0·001	0·979	0·979
0·005	0·911	0·956
0·01	0·876	0·941
0·05	0·781	0·889
0·10	0·727	0·860
0·50	0·636	0·779

These results show that with KCl, just as in the case of HCl, the activity coefficient decreases much more rapidly than the conductivity ratio, a being 15 per cent. lower than γ in a

decinormal solution of KCl, and 9 per cent. lower than in the case of HCl at the same concentration. It will also be seen on comparing these data with those of HCl that the numerical values of both α and γ are less in the case of KCl than they are in the case of HCl. There is therefore no particular reason for employing the assumption that the true degree of ionisation for HCl is identical with that for KCl. Thus far we have not succeeded in clearing up the difficulties of the difference between α and γ, but it is just here that Ellis has brought forward some additional evidence which has a notable bearing on the problem. A comparison may be made between the values of the activity coefficients of KCl derived from electromotive force measurements and those derived from freezing-point determinations by the method recently described by Bates. With the aid of the laws of thermodynamics and of the two assumptions (1) that the two ions in the same solution have equal osmotic pressures and (2) that the true value of the ionisation is given by the conductivity-viscosity ratio, Bates found that the change at 0 C. of the osmotic pressure π of either ion with its concentration $c\gamma$ is expressed in the case of KCl by the equation $d\pi/d(c\gamma) = RT(1 - 0{\cdot}0552\ (c\gamma)^{0{\cdot}307})$. From this equation and the two thermodynamic relations :

$$-\Delta F = \int_{\pi_2}^{\pi_1} d\pi/c\gamma, \text{ and } -\Delta F = RT \log c_1\alpha_1/c_2\alpha_2$$

(where $-\Delta F$ denotes the decrease in free energy involved in the transfer of one mole. of KCl from osmotic pressure π_1 to osmotic pressure π_2), supplemented by the assumption that the activity coefficient α approaches unity as the salt concentration c approaches zero, the ***activity coefficients*** corresponding to Bates's osmotic pressure calculations are found to have the values quoted in the second column of the following table. At the same time the activity coefficients calculated directly from MacInnes and Parker's E.M.F. measurements are recorded in the third column.

Concentration of KCl moles. in 1,000 grms. H_2O.	α. Bates.	α. MacInnes and Parker.
0·01	0·884	0·876
0·05	0·772	0·781
0·10	0·732	0·727
0·50	0·640	0·636

The close agreement throughout the whole range of concentration of the two sets of a values is highly remarkable. It is to be clearly understood that the values of a in the second column are calculated indirectly on the basis that the true degree of ionisation γ is actually given by the conductivity-viscosity ratio, whilst no such assumption is involved in the data of MacInnes and Parker. *If the above agreement is confirmed by results with other substances it will afford proof that the conductivity-viscosity ratio is a substantially correct measure of ion concentration though not of ion activity.* Unfortunately sufficiently accurate freezing-point data are not available for HCl to enable corresponding computations to be made in the case of this substance. Such data are much to be desired. It will be observed that Ellis's conclusion does not bear out G. N. Lewis's contention, viz. that the conductivity-viscosity ratio is in error owing to the probability that the speed of the ions increases as their concentration increases. Of course so long as such changes are relatively small we are justified in regarding the conductivity-viscosity ratio as substantially correct. For KCl this is fairly certain. For HCl it is not yet certain.

INORGANIC CHEMISTRY. By C. Scott Garrett, D.Sc.

Analytical.—The problem of quickly providing a supply of water for drinking and culinary purposes is one which is of almost daily occurrence in these stirring times. For armies in the field, for troops in fresh training grounds, for munition factories planted out in the wilds, sea-borne supplies for expeditions or for fleet units, this provision of sterilised water is an urgent problem. As a rule the simplest and most effective method of sterilisation is by the addition of a small quantity of chloride of lime, calcium chlorohypochlorite. One objection to this method of sterilisation is the rather unpleasant taste which is produced in the water. This, however, is of small importance so long as it does not arise from the presence of free chlorine derived from the hypochlorite either directly or by its interaction with traces of free mineral acids originally present in the water supply.

Free chlorine is very deleterious and ought to be avoided at all costs, and it is on this account that the new sensitive test for its presence (Le Roy, *Compt. rend.* 1916, **163**, 226) is of

considerable value. The necessary reagent is prepared by dissolving one gram of hexamethyltri-p-aminotriphenylmethane in 20 c.c. of cold hydrochloric acid made by diluting the strong acid with an equal volume of water. The hydrochloride is then diluted ten times with distilled water and can be preserved indefinitely as a stock solution. It gives an immediate violet coloration if free chlorine is present, when a few centimetres are added to the water under test. In this way it is possible to detect three parts of chlorine in a hundred million parts of water. Traces of hydrogen peroxide are without action, and the reagent has the distinct advantage over the starch-iodide test in that it is not nearly so sensitive to extraneous nitrates. The formate is as effective as the hydrochloride and it is advantageous in carrying out the test to add a little salt and acidify the test samples of water with either acetic or formic acid.

Technological.—A problem which has in recent years received a considerable amount of attention from the industrial and technological point of view is the underlying cause or causes of the spontaneous combustion of coal under anaerobic conditions. A paper has just been published by Drakeley (*Trans. Chem. Soc.* 1916, **109**, 723) which bears on this subject, more particularly as to the part played by pyrites impurities which are found to a greater or lesser extent in most samples of coal. Drakeley has prepared mixtures of coal and pyrites, coal and ferrous sulphate, and coal and sulphuric acid, and has quantitatively measured the oxidation products as well as the rates of absorption of oxygen of these mixtures and compared the results with those for coal and pyrites unmixed. His results go to indicate that the presence of pyrites in coal increases the rate of oxidation and therefore probably considerably increases the possibility of spontaneous combustion in such coal.

Synthetical.—By far and away the most important development in the domain of synthetical inorganic chemistry is the production of nitrates or nitrogenous substances from the free nitrogen of the atmosphere. On the Continent this problem has received enormous attention and particularly by our Germanic enemies, who, it is reported, have made themselves independent of the natural occurring deposits of South America by their synthetically produced products. So important is this subject that the United States Government have

recently decided to send a Commissioner to Europe to study the processes actually at work with a view to the establishment of similar industries in America. Judging from their lack of initiative our own Government have not yet realised the fundamental importance of the establishment in this country of corresponding industries. Let us hope that wiser counsels will soon prevail if we are to maintain our rightful place in chemical industry.

In this connection the work of Zenghelis (*Compt. rend.* 1916, **162**, 914) on the synthesis of ammonia at low pressure is worthy of consideration. Zenghelis has obtained positive results by passing a correctly proportioned mixture of hydrogen and nitrogen through a tube containing a small amount of acidulated water maintained at 90° C. along with such hydrogen "atomisers" as platinum sponge and platinum black, and colloidal forms of platinum, palladium, silver, gold, mercury, and copper.

When hydrogen is passed through solutions evolving nitrogen, *e.g.* sodium nitrate and ammonium chloride, the results were again positive. Nascency or an "atomised" condition of the nitrogen is therefore a factor in the synthetic reaction.

Finally when both elements are present in the "atomised" state the results obtained were much more positive than when one only was specifically reactive. Thus when hydrogen was passed through a solution evolving nitrogen and to which metallic catalysts had been added, yields of ammonia exceeding 40 per cent. of theory were obtained.

Constitutional.—Datta and Dhar (*J. Amer. Chem. Soc.* 1916, **38**, 1303) have applied the methods of molecular solution volume and molecular refractivity to the problem as to whether the constitution of chromic acid is correctly expressed by the formula H_2CrO_4 or $H_2Cr_2O_7$. Their evidence obtained with the potassium salts is all in favour of the dichromate formula for the parent acid.

A considerable addition has been made to our knowledge of the intramolecular arrangement of water of hydration by the infra-red spectroscopic investigations of Schaefer and Schubert (*Am. Physik.* 1916, iv. **50**, 283, 339).

In the course of their examination of the specific refection maxima of crystalline sulphates and carbonates in the infra-red, these authors have found that with hydrated dichroitic and

trichroitic crystals a displacement of the maximum corresponding to the water molecule was experienced, using polarised light, according to the direction of the light beam in relation to the optical axis or axes of the crystal. The "hydrate" water paralleled in behaviour the specific optical characteristic of the crystal and was therefore anisotropic in optically anisotropic crystals. The hydrate molecules assume the symmetry of the crystals which house them. As might be expected also, the uncombined water maximum at $3 \cdot 2\ \mu$ is in general shifted when the water becomes "hydration" water.

Corrosion.—The complicated and costly phenomenon of the sea-water corrosion of ship condenser tubes has been engaging the attention of scientists for some considerable time. The three years' work of the Corrosion Committee has advanced our knowledge very considerably on this highly important subject. At a recent meeting of the Faraday Society the subject was under discussion and the conflicting views were debated. It is highly satisfactory to record that the matter has now been definitely taken up by the Government, who are establishing an experimental station at Southwick and financing the future investigations.

Some of the results so far obtained under the Committee are published in a paper by Gibbs (*Trans. Faraday Soc.* 1916, **11**, 258). Gibbs shows that the rate of corrosion progressively diminishes as copper is alloyed with zinc until an alloy corresponding with the formula CuZn is obtained. This alloy is not attacked at ordinary temperatures by sea water.

With ordinary 70 : 30 brass there is an initial minimal corrosion rate which soon disappears. Rising temperature up to 50° C. increases the rate of corrosion, which then diminishes quickly. The point of attack is initially towards copper, then general, and finally towards zinc. The intermediate formation of oxychloride seems to be a definite determinant of reaction. Hard-drawn tubes have a greater resistance to corrosion than annealed alloys, but in the former the dezincification is greater.

Polymorphism.—Some careful experiments on the transition points of the polymorphic forms of pure iron are recorded in a paper by Ruer and Goerens (*Ferrum*, 1915, 13, 1). The iron was electrolytically prepared almost free from carbon. It gave a freezing point at 1528° with transition points $\alpha-\beta$ at 769°, $\beta-\gamma$, 906°, and $\gamma-\delta$ at 1401°. They failed to confirm a

transition point sometimes recorded between 1000° and 1200°, which they state is due to the presence of oxide.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

General and Dynamic Geology.—The past climates of the earth are dealt with by C. Schuchert (*Carnegie Inst. Washington*, 1915, Publ. No. 192, 263), who regards the principal climatic variations as due to changes in topography, to variations in the amount of heat stored in the oceans, and to changes in the composition of the atmosphere affecting the storage of solar radiation.

The prismatic structure of igneous rocks is discussed by R. B. Sosman, who distinguishes three general types (*Journ. Geol.* 1916, **24**, 215). The commonest is that due to thermal contraction in the solid rock. A newly distinguished mode is due to convectional circulation of the magma whilst still liquid, forming polygonal cells like those formed in cooling wax or oil. A third type is produced by internal expansion consequent upon alteration, producing the "weather-crack" (or "onion-structure") often seen in weathered diabase.

Dr. L. L. Fermor and C. S. Fox have mapped in detail a selected area (65 square miles) of the Deccan Traps in the Chhindwara district, Central Provinces of India (*Records Geol. Surv. India*, 1916, **47**, Pt. II. 81). The lavas rest directly on Archæan granites and gneisses. Five flows were detected, each of which is vesicular at the margins, and with coarse basalt or dolerite in the central portions. The flows are separated by small thicknesses of fresh-water fossiliferous sediments, or of "green-earth," or of both together. The mapping shows that the flows have been bent into a series of gentle folds.

Petrology.—S. J. Shand has devised a simple and apparently very efficient micrometer for the measurement of the quantitative mineral composition of rocks by the Rosiwal method (*Journ. Geol.* 1916, **24**, 304). The rock slide is fixed on a sledge moved by micrometer screws fixed at both ends. The instrument is mounted on the stage of the microscope. The writer has found that an ordinary mechanical stage fitted with a vernier gives very good and rapidly achieved results in this kind of measurement.

In a paper on the building of the North Atlantic Tertiary volcanic plateau (*Geol. Mag.* (6), 1916, **3**, 385) L. Hawkes dis-

cusses the origin of the red partings between the successive basalt flows in Iceland. Microscopic investigation shows that this material is a palagonitic ash, in which the felspar and pyroxene crystals remain perfectly fresh. Mr. Hawkes regards these partings as analogous to the tuff-dust deposits upon the modern lava-deserts of Iceland; and, instead of indicating a land-surface weathering of long duration, may represent only a few days' accumulation before being sealed up by a fresh lava flow.

Genesis of Sedimentary Rocks.—A remarkably full and interesting paper by Dr. P. G. H. Boswell provides abundant data upon the stratigraphy and especially the petrography of the Lower Eocene rocks of the north-eastern part of the London basin (*Quart. Jour. Geol. Soc.* 1916, **71**, pt. 3, 536). Diagrams showing the isopachyte systems for the London Clay and the Lower London Tertiaries are given, as also many mechanical analyses of the sands, loams, and clays which constitute these deposits. The same author also shows how the mechanical analyses of the North Sea Drift and the Upper Glacial Brick-earths help to distinguish these two series of deposits ("The Petrology of the North Sea Drift," *Proc. Geol. Assoc.* 1916, **27**, 79). The detrital mineral assemblage, however, is the same for each, and is unsurpassed in British sediments for variety and beauty.

A very complete study of the origin and history of the colouring matter of "red beds" has been made by C. W. Tomlinson (*Journ. Geol.* 1916, **24**, 153, 238), with especial reference to the Permo-Carboniferous and Triassic of the Western United States. He finds that in general the ferruginous material was transported and deposited as a mechanical sediment, that changes in oxidation and hydration have been unimportant, and that while "red beds" are not in themselves indicators of aridity, red sediments should bulk more largely in the deposits of arid than of humid regions.

In a paper on the geological significance of arkose D. C. Barton has tabulated arkose deposits from all over the world with regard to their occurrence and characters (*Journ. Geol.* 1916, **24**, 417), and has arrived at the conclusion that arkose can no longer be held to indicate one or two particular sets of rigorous climatic conditions. This rock is laid down whenever a period of deposition is inaugurated over a granitic terrane,

whatever the prevailing conditions. Prof. W. Salomon ("Die Definitionen von Grauwacke, Arkose, und Ton," *Geol. Rundschau*, 1915, **6**, 398) regards arkose and greywacke as analogous deposits derived respectively from a granitic or gneissic terrane, or from an area of clay-slates, sandstones, and miscellaneous rocks. It is essential that the deposits should have suffered very little transport, and hence should consist of coarse, angular, unassorted material.

The origin of dolomite has been investigated by F. M. Van Tuyl (*Amer. Journ. Sci.* 1916, **42**, 249), who finds that most of the dolomites examined are replacement products of limestone. The facts supporting this contention are the lateral gradation of beds of dolomite into limestone, the mottling of limestone by irregular patches of dolomite on the borders of dolomite masses, the existence of remnants of unaltered limestone in dolomite, the irregular boundaries between beds of dolomite and limestone, the presence of altered fossils, the observed protective effect of shale beds, and the general obliteration of structures and textures.

Alteration of Rocks and Minerals.—An unusually perfect example of pyrophyllitisation, pinitisation, and silicification is described by A. F. Buddington (*Journ. Geol.* 1916, **24**, 130) in the rhyolites and basalts of Conception Bay, Newfoundland. The alteration is ascribed to metasomatic replacement of previously silicified rhyolites by thermal waters under conditions of dynamic stress, and takes place in fault and shear zones.

Important experimental work on the hydrothermal alteration of felspars and hornblende has been performed by E. A. Stephenson (*Journ. Geol.* 1916, **24**, 180). Dilute solutions of sodium carbonate, potassium fluoride, mixtures of these two, sodium bicarbonate, and sodium tetraborate were used on crystals of adularia, albite, and hornblende. The reactions were carried out in copper tubes heated in a special electric oven at temperatures up to 280° C., and at pressures up to 65 atmospheres, for periods from 15 to 90 days. Pure water under these conditions had no effect whatever on the minerals, but the alkaline solutions attacked the felspars with separation of silica, and generally with the formation of minute icositetrahedra of analcite. This has an important bearing on the question of the origin of analcite and other zeolites in

igneous rocks. An important negative result is that no kaolin or kaolin-like substance forms from the alkaline solutions at temperatures up to 280° C. The conclusion is drawn that kaolin probably originates by the action of acid solutions upon felspars. This is corroborated by the rarity of references in the literature to the association of kaolin and carbonates, though this association would be expected if carbonated alkaline waters were the cause of kaolinisation.

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc.

READERS of these notes may remember that during the last three years I have repeatedly pointed out that the main facts of Prehistoric Anthropology as now known are totally misrepresented by Lord Avebury's classification of the Stone Ages into Paleolithic and Neolithic epochs, Lord Avebury (then Sir J. Lubbock) having invented these terms when the science was in its infancy. The use of the words Paleolithic and Neolithic suggests that the most important break in the "Stone Age" occurs between these two periods, and that each period possesses within itself some kind of unity. The facts are, as the reader will remember, altogether different. By far the most important break in the history of man in Europe occurs between the Lower and the Upper Paleolithic, at the beginning of the Aurignacian Age, when *Homo sapiens* appears, and displaces the least ancient of the extinct species of the Hominidæ, *Homo neandertalensis*. Thus the so-called Paleolithic period has no sort of unity, and the affinities of the later "Paleolithic men" are with the men of the Neolithic and later periods, not with the older "Paleolithic men." My suggestion therefore was (see SCIENCE PROGRESS, October 1913) that the misleading term Paleolithic should be abandoned, and that the word Protolithic should be coined for the earlier part of the so-called Old Stone Age, and that the later part of what has hitherto been called Paleolithic should be described as Deutolithic. The new words are etymologically analogous to those coined by geologists in splitting up the Paleozoic era into its two parts—the Protozoic and Deutozoic.

I am now glad to see that Prof. G. Elliot Smith, of Manchester University, has been laying stress upon this same point. In a review of Prof. Osborn's *Men of the Old Stone Age* in the *American Museum Journal* for last May (vol.

xvi. pp. 319-25) he refers to it at some length. He criticises the continued employment of the word Paleolithic on the same grounds that I adopted, and he proposes a new classification. He would call the whole pre-Aurignacian story of man in Europe the "Palæanthropic Age," and all the subsequent periods, from the Aurignacian onwards, he would group together as the "Neoanthropic Age." One hopes that the point will be taken up by other scholars no less influential than Prof. Smith. My experience is that intelligent readers, who do not happen to be specialists, are constantly misled by the survival of Lord Avebury's terminology, and therefore no excuse is necessary for having referred to this matter again. Whether Prof. Elliot Smith's proposal or mine be preferred is a minor point. The essential point is that we should get rid of the word "Paleolithic."

Prof. Elliot Smith has also been recently developing in an extraordinarily interesting manner the thesis that the Pre-Columbian civilisations of America—or at least many important features in those civilisations—were not truly aboriginal, but came in a cultural wave from Asia across the Pacific Ocean, the original starting-point of the most remarkable characteristics being Egypt. The facts are set out fully in a paper entitled "The Influence of Ancient Egyptian Civilisation in the East and in America" which will be found in the *Bulletin of the John Rylands Library* for January to March 1916. Prof. Smith believes that the extremely peculiar culture of Egypt was spread eastwards by mariners, mainly Phœnicians, for several centuries after B.C. 800. To quote the author's own words, he thinks that "the essential elements of the ancient civilisations of India [the pre-Aryan civilisations], Further India, the Malay Archipelago, Oceania, and America were brought in succession to each of these places by mariners, whose oriental migrations began as trading intercourse between the Eastern Mediterranean and India some time after 800 B.C., and that the highly complex and artificial culture which they spread abroad was derived largely from Egypt (not earlier than the 21st Dynasty) but also included many important accretions" from other sources, and that after traversing Asia and Oceania, and becoming modified on the way, the stream finally "continued for many centuries to play upon the Pacific littoral of America, where

it was responsible for planting the germs of the remarkable Pre-Columbian civilisation." Another paper on the same subject by Prof. Elliot Smith may be mentioned, namely, "The Origin of the Pre-Columbian Civilisation of America," in *Science* for August 11, 1916. Much of the evidence adduced by the author of these papers appears to me to be of a convincing character.

The reader who is interested in the spread of the Egyptian, Phœnician, and other ancient civilisations should also read two recent papers by Mr. W. J. Perry. These are "The Relationship between the Geographical Distribution of Megalithic Monuments and Ancient Mines" in vol. lx., Part I. of the *Memoirs and Proceedings of the Manchester Literary and Philosophical Society*, and "The Geographical Distribution of Terraced Cultivation and Irrigation" in the same *Memoirs*, vol. lx. Part I.

To Parts II. and III. of the same volume of these Manchester *Memoirs*, a whole series of papers dealing with different aspects of this same problem is contributed by Mr. J. Wilfrid Jackson. Of these papers one of the most interesting, though not the longest, is "The Aztec Moon-cult and its relation to the Chank-cult of India."

The *Proceedings* of the now well-known Prehistoric Society of East Anglia for the year 1915–16 have been recently published. The *Proceedings* are largely composed of reports upon and discussions about the discoveries made at Grime's Graves. I have alluded to these discoveries in previous numbers of this Review. In particular, the volume now received includes the Presidential Address of Mr. A. E. Peake, dealing with the Grime's Graves, and Mr. A. S. Kennard's paper on the "Pleistocene Succession in England," to both of which I referred in the July number of SCIENCE PROGRESS. Both these papers are extremely controversial. I notice in passing that Mr. Kennard throws in some jibes at German science which are not only somewhat out of place in a learned society, even in war, but have not the merit of being true. Even now, the people of this country do not properly appreciate the fact that the remarkable strength displayed by the German nation during the last thirty months has been very largely the power of German science. Failing a much more adequate cultivation of science, there will be no great future for Great Britain.

Amongst the other papers in the *Proceedings* are "The Norfolk Sub-Crag Implements," by Mr. W. G. Clarke, and "The Pliocene Deposits of South-east England," by Mr. W. J. Lewis Abbot.

The present year has been notable for the fact that an anthropologist has been President of the British Association. Sir Arthur Evans's Inaugural Address was entitled "New Archæological Lights on the Origin of Civilisation in Europe; its Magdalenian forerunners in the South-West and Ægean Cradle." The address dealt, as will be gathered from its title, with the more recent parts of the Prehistoric Period. The President showed that recent discoveries had brought out the fact that culture was much older than had been supposed until quite recently, and that, in particular, our Magdalenian predecessors in Europe were more advanced than used to be believed. The address is reported in *Nature* of September 7.

CORRESPONDENCE

To the Editor of Science Progress

MALEDUCATION AND MALPRONUNCIATION

SIR,—In his reply to me on this subject in the current number of SCIENCE PROGRESS the writer of the original Note seems to have missed my point. He mistakenly attributes to me the view that "a wrong course is justifiable where it is easy and pleasant." My contention was that in a certain stanza of poetry the rhyming of *patriā* with *away* was correct according to long-established usage among English classical scholars; while he condemns this rhyme, and styles the stanza "doggerel," on the ground of his belief that the rhyme is "wrong" because the ancient Latins pronounced *patriā* quite differently.

The question of the ancient pronunciation of Latin is not one of "right" or "wrong," but merely one of opinion. No amount of discussion about this matter would be at all relevant to my assertion that the *rhyme* so grievously inculpated by the writer of the Note is perfectly correct, and the verse not "doggerel."

Yours obediently,

H. BRYAN DONKIN.

REPLY

SIR,—I may be excused if I have mistaken Sir Bryan Donkin's meaning, because I must confess that it is very difficult to extract. The fact that *patria* would be pronounced in the same way in Portugal, Spain, France, Italy, Rumania, and probably Greece, Germany and almost every other European nation but England, gives a statistical presumption amounting to millions to unity that the Romans pronounced the word in the same way. On the other hand, that the ancient Romans should have given the vowel *a* in this word the unique and indeed eccentric values which the modern English attach to it, is about as unlikely as that they played cricket in top hats. In fact the betting is almost infinity to unity against the correctness of Sir Bryan Donkin's supposition. He therefore asks us to reject the theory which is almost certainly true for the theory which is almost certainly false. My original Note in SCIENCE PROGRESS of July 1916 implied that the persistence of this barbarism showed how little the teaching of Latin in the schools is really concerned with poetry as distinct from grammar. I once read somewhere a satire on our pronunciation of French, which contained the couplet:

With that he said to her
"Où voulez-vous aller?"

To make *patria* rhyme with *away* is an equal monstrosity. Sir Bryan Donkin (who, I observe, completely ignores the decision of Milton on this subject which I quoted in my last letter) may, of course, pronounce Latin as he pleases; but that such a pronunciation should continue to be taught in our schools is proof only of the badness of our modern education. I think that I did quite understand Sir Bryan Donkin's contention, but consider that it is not seriously arguable.

Yours faithfully,

THE WRITER OF THE NOTE.

NOTES

The Choice

The Kings go by with jewelled crowns,
Their horses gleam, their banners shake, their spears are many.
The sack of many-peopled towns
Is all their dream :
The way they take
Leaves but a ruin in the brake,
And, in the furrow that the ploughmen make,
A stampless penny ; a tale, a dream.

The merchants reckon up their gold,
Their letters come, their ships arrive, their freights are glories :
The profits of their treasures sold
They tell and sum ;
Their foremen drive
The servants starved to half-alive
Whose labours do but make the earth a hive
Of stinking stories, a tale, a dream.

The priests are singing in their stalls,
Their singing lifts, their incense burns, their praying clamours ;
Yet God is as the sparrow falls ;
The ivy drifts,
The votive urns
Are all left void when Fortune turns,
The god is but a marble for the kerns
To break with hammers ; a tale, a dream.

O Beauty, let me know again
The green earth cold, the April rain, the quiet waters figuring sky,
The one star risen.
So shall I pass into the feast
Not touched by King, merchant or priest,
Know the red spirit of the beast,
Be the green grain ;
Escape from prison.

John Masefield.

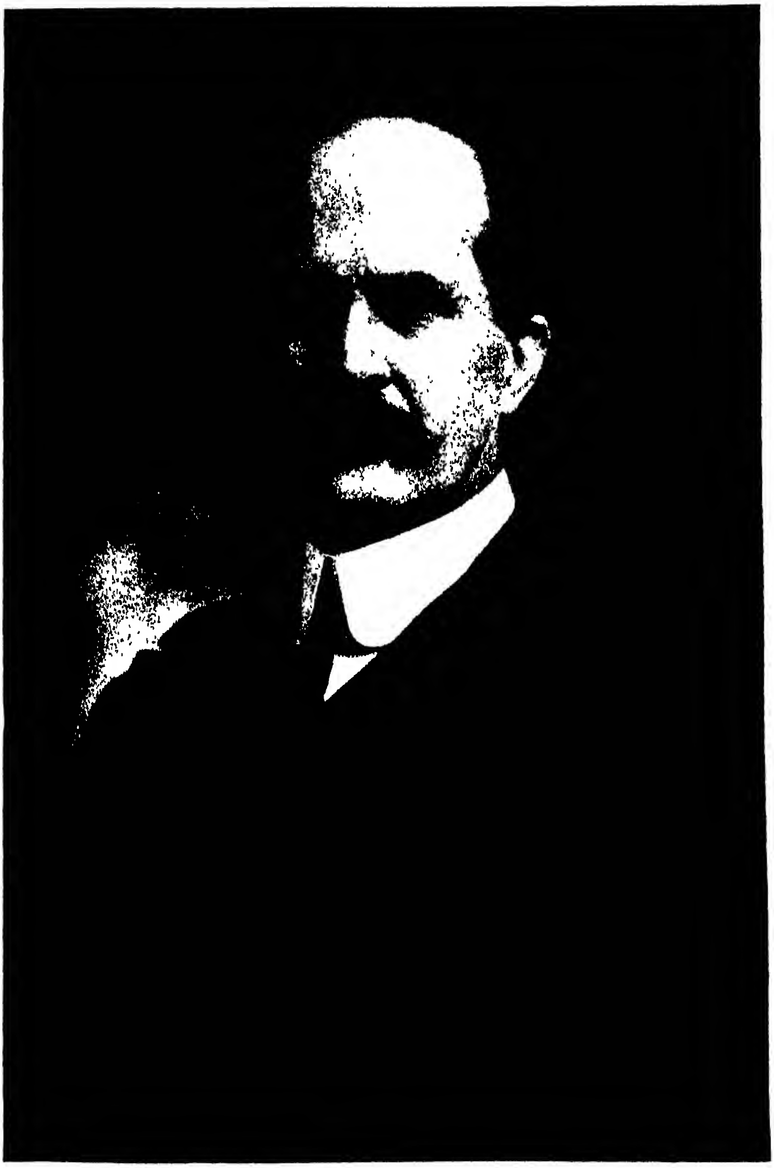


Photo: Elliott & Fry, Ltd.

PROF. W. H. BRAGG, F.R.S.

Nobel Prizeman for Physics.

The Nobel Physics Prizes, 1915

The Nobel Prize for Physics for the year 1915 was awarded to Prof. W. H. Bragg and his son W. L. Bragg, in recognition of the importance of their joint work on the application of X-rays to the determination of the atomic arrangement of crystals. Prof. Bragg was born in 1862 at Stoneraise Place, Wigton, Cumberland. He was educated at King William's College in the Isle of Man and at Trinity College, Cambridge, where he became third Wrangler in the year 1884. Two years later he accepted an invitation to the Chair of Physics at Adelaide University, and he remained there for the next twenty-two years. In 1889 he married the daughter of Sir Charles Todd, who, during his long service as Postmaster-General of South Australia, was responsible for driving the telegraph line from Adelaide to Port Darwin across the Australian continent: his grandson has the unique distinction of becoming a Nobel laureate in his twenty-fifth year!

At Adelaide, Prof. Bragg carried out research which gained him world-wide fame; in particular his work on the range of ionisation of α-particles has become a classic in the history of radioactivity. He was elected Fellow of the Royal Society in 1907, and in 1909 returned to England as Cavendish Professor of Physics at Leeds University, whose laboratories thus gained the enviable distinction of being the site of the X-ray experiments which were pursued there from 1912 up to the time when, in 1914, he was persuaded to accept the Quain Chair of Physics at University College, London. The immense importance of these experiments was speedily recognised at home and abroad, and some months before the Nobel award was announced Prof. Bragg and his son received the Barnard Gold Medal from the University of Columbia—a medal given quinquennially for distinguished scientific research. W. L. Bragg also went to Trinity College, of which he is now a Fellow, and his X-ray work was the direct outcome of his father's suggestions when, at the end of his degree course, he was seeking a fruitful line of research. In order to explain how this came about it will be necessary to relate just a very little of the history of the subject.

The nature of X-rays had been the source of much discussion from the time of their discovery by Röntgen in 1895, and

several theories awaited the test of some crucial experiment. In particular it was known that, if they were of the same nature as light waves (*i.e.* electromagnetic waves in the æther), their wave length must be of the order of 10^{-8} to 10^{-9} cm. Now this is so small that the waves could not be diffracted by any ruled diffraction grating, for the spacings in a grating need to be of about the same magnitude as the wave length of the rays it diffracts. The conjectured wave length was, however, of the same order as the distance between the atoms or molecules of a crystal, and Dr. Laue of Zurich, who received the Nobel Prize for 1914 for his share of the work, first realised the possibility of using a crystal as a three-dimensional grating for the rays. The mathematical problem is a very difficult one; but Laue was able to show that, if a narrow pencil of parallel rays were passed through a thin plate of a crystal, the emergent beam should give, on a photographic plate, not only a central patch corresponding to the undeviated beam, but a symmetrical diffraction pattern round it. This theory was tested experimentally, under Laue's direction, by Friedrich and Knipping in 1912, and with complete success, so that the problem of the nature of the rays was solved. It was, however, exceedingly difficult to deduce the arrangement of the atoms in the crystal from the pattern obtained, indeed almost impossible save in the simplest cases. Moreover, Laue had to assume that the X-rays incident on the crystal contained only a few different wave lengths, *i.e.* that they had a "bright line" and not a "continuous" spectrum, and even then many of the spots demanded by theory did not appear on the plate. The subject had reached this stage of its development when Prof. Bragg suggested to his son that he should take it up. After making a few experiments W. L. Bragg put forward a new explanation of the formation of the diffraction pattern, namely that it was due to the reflexion of waves, of a wide range of wave lengths, by the various parallel planes of atoms which exist in the crystal. This explanation is simpler and far more successful than that given by Laue; but it owes its chief importance to the fact that it embodies the idea of the *reflexion* of the waves by the successive layers of atoms inside the crystal. It follows immediately, as was suggested by C. T. R. Wilson, that, by increasing the angle of incidence of the rays on any particular set of parallel layers of atoms, they should be reflected back selectively just as light

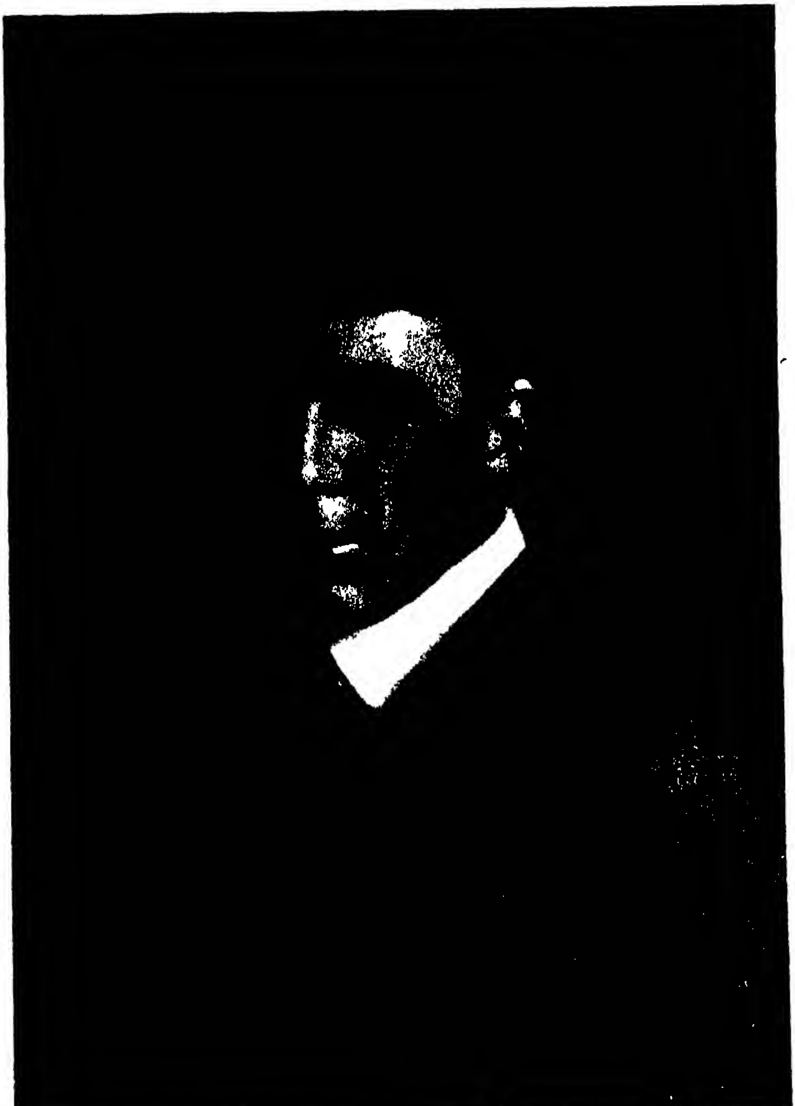


Photo : Hills & Saunders.

MR. W. L. BRAGG,
Nobel Prizeman for Physics.

waves are reflected from the alternately twinned planes of a potassium chlorate crystal or by the successive layers of an iridescent shell. The experiment was tried and its success was first announced by W. L. Bragg in a letter which he wrote to *Nature* in December 1912.

The reflexion theory vindicated, Prof. Bragg and his son at once set themselves to design an instrument suitable for the investigation of the spectra of the rays and the structure of the reflecting crystals. Their X-ray spectrometer was the result. The collimator of the ordinary spectrometer was replaced by two narrow slits, and the telescope by a small ionisation chamber connected to an electroscope and provided with a narrow slit to admit the rays reflected on to it by the crystal which takes the place of the prism. With this instrument two lines of research were immediately obvious. First, the investigation of the characteristic rays emitted by different anticathodes and secondly the determination of the exact arrangement of the atoms on the space lattices of a crystal. To this more difficult problem they devoted their chief attention, and the precise positions of the atoms in many crystals had been settled, and many important side issues had been opened up, before the war broke out. Since then some progress has been made with the general question in Germany, Sweden, Japan, and America, and in England also; but not much determination of crystal structure has been done. Nevertheless sufficient work has been carried out to show that the X-ray spectrometer is as powerful a means of scientific inquiry as its parent instrument. It has, for example, provided direct proof of the thermal agitation of the crystal molecules and, on the chemical side, has given a beautiful explanation of the difference in the behaviour of diamond and graphite. But the further investigation of these questions and the application of the spectrometer to the problems of metallurgy must, at least in the country of its origin, await in their turn the conclusion of peace.

At the outbreak of the war W. L. Bragg, who had already served for four years as a trooper in King Edward's Horse, applied for a commission and was attached to the Leicestershire R.H.A. (Territorials). He is now employed on special duty in France. His brother was in camp with the same cavalry regiment at the beginning of August 1914, and was afterwards

given a commission in the R.F.A. He lost his life in the Dardanelles. Prof. Bragg is a member of the Admiralty Inventions Board, and holds a temporary position in the Navy, to which he devotes all the energy and enthusiasm that in happier times would have been given to the further development of his scientific work.

D. O. W.

Scientific Snobbery

One reason for the neglect of science is that scientific men themselves frequently misrepresent the objects for which they work. For example, they often pretend that they perform their labours merely for their own amusement. We once heard it wittily said of such a man that he takes out his watch before dinner and exclaims, "Ha! I have half an hour before I must dress for dinner; I will just step over to my laboratory and make a discovery." But the public is not so easily deceived and therefore thinks in its dull way that the man of science really labours in the hope of making some enormous fortune or obtaining some great honour. On another occasion, we heard it said of a man who has been toiling for years in the tropics for nothing, that "nobody knows why he does it, but we all believe that he wants a knighthood." When we timidly suggested that he was guided merely by a sense of duty, we were met by a stare of astonishment. Certainly this worker has never received a penny for his work as yet and never will, and we fear that not even a knighthood will come his way. No, men of science do not work either for amusement or to make fortunes. Like artists and musicians, they often find their labours fascinating because nature imbues them with an instinct in the directions chosen by them; but they are also conscious that their work will bring them no personal profit—not so much as that which a tithe of the ability shown by them in science would have yielded them in politics, law, or grocery. Their ultimate object is to benefit humanity by adding to the store of knowledge which lifts the civilised man so far above the savage of the jungles. And that is the greatest object which any man can keep before his eyes.

Another form of scientific snobbery is the pretence that science has no practical object in view—it is so lofty a pursuit that the man of science should live among the stars and not

soil his fingers with the common earth of everyday life. Even Lord Kelvin said "that no great law in natural philosophy has ever been discovered *for* its practical application"—though no one based more patents on his own researches than did Lord Kelvin. He may have been right in one sense, but certainly not in all (and he cannot be accused of any form of snobbery). Thus geometry was really founded for the purposes of architecture and navigation. Mechanics was created to assist the engineer, and the theories of heat and of the conservation of energy were probably generated by the steam-engine; while the entire science of pathology has simply been created for its practical application as regards the prevention and cure of disease. Certainly investigations which were apparently useless at the outset have often led to valuable practical applications; but they were usually undertaken because the worker knew that he must first solve general problems before applying the solution to specific cases. We believe that all the great theorists had practical applications before them like a distant light even in the greatest darkness of their first efforts. Is it likely that Newton, or Harvey, or Faraday did not prophetically see that their work would some day benefit humanity? Nature is infinite, and it is therefore wise to toil in immediate contact with human needs and not to lose oneself entirely far away from the remotest utilitarian objects. In most cases those who lose all touch of the useful in their investigations end by becoming useless themselves. They are above the practical, and therefore become unpractical, and finally impossible.

Perhaps the worst form of scientific snobbery is the pretence that the man of science is absolutely above cash in any form. Let us distinguish. To effect discovery, a man must concentrate all his energies upon a single point; he has no time to watch the share market, or to promote companies in connection with his findings; and it will be lucky if he succeeds in making any advance at all even with all his energies bent upon the point of issue. In that sense, therefore, he must ignore cash. But even here various circumstances should influence him. If he is a bachelor, he may do as he pleases, and may live as a recluse upon brown bread and water in a monk's cell if he wish. But if he has children or other dependants, is he justified in allowing them to be brought up un-

educated in poverty? Such a thing would not be meritorious in him but a crime; for we have our duties not only to science but to our families. The scientist who pretends his indifference to money is, therefore, often only a snob. Moreover, although he himself may have no children, or may possess independent means, this need not necessarily be the case with others. His quixotic attitude merely lowers the price of science in the world and causes other and probably better men to suffer. Still further, for the most obvious economical reasons, it causes science in general to suffer, because when young men see the poverty of the most successful investigators they hesitate to enter such fields of labour and the recruiting of the voluntary army of science is naturally reduced. Certainly no scientific man has the smallest desire to be a millionaire; but moderate competence is useful to him as to others. A certain amount of money gives him a proper influence for good in society, and enables him to devote himself to those investigations which his nature tells him he is most capable of conducting. On the other hand, keep him in poverty and he soon loses his enthusiasm; he becomes a fakir sitting in rags by the roadside, and the ripest years of his life are often wasted. Is there any intrinsic reason why the greatest efforts of the best minds in the most fertile of fields should lead to poverty? Yet the history of the world proves that they generally do so—to the loss not only of Science but of the World. And why, pray? Because when Science asks for her dole, the World replies, "But those great men, Smith and Jones, are proud to labour for nothing; why then should I pay you?" Alas, poor ignorant World does not know that if Smith and Jones are genuine workers they are probably too much engrossed in their toil to bestir themselves for payment; while if, as more often happens, they are merely purveyors of others' labours, then their lofty and popular pose is adopted for a purpose. And, indeed, snobbery is often a paying cult, and those who labour for nothing do little but frequently get much!

In science as in other things, the proper and honest procedure is to pay for work done; and, to be frank, the encouragement of science, of which we hear so much nowadays, must in the end come to this—or to nothing. And in science as in other things snobbery is a false pose which brings only contempt upon those who adopt it.

Neglected Discoveries

Toilers at science generally complain that no one appreciates the importance of their labours. We spend our lives digging in the ditch in search of the priceless jewel which we imagine to be there; and the World passes by upon the road, pauses a minute, asks what we are looking for, and when we cannot tell him, goes on his way with a smile singing a common tune. But, after all, we are allowed to remain and go on digging in peace. A very distinguished man of science writes as follows: "I feel strongly that ——'s work should have been more noticed. . . . The important work in —— that I did twenty-nine years ago and at which I have been working at intervals ever since, has received no notice and was not even mentioned in ——'s book published sixteen years ago. The result has been, though, that I have collared the 'stuff' and others have not." The world's neglect is certainly bad for science and bad for the public, but it is often of advantage to the worker for the reason given. The neglect is a period of gestation which enables the discovery to leap full-armed into existence when the time is ripe, instead of having to undergo the vicissitudes of infant life. Undoubtedly the great Greek legend of the birth of Pallas Athene, the goddess of Science, panoplied straight from the brain of Zeus, was, like so many of the other magnificent Greek legends, a story told by some unknown poet or seer before the days of writing to impress humanity with a great truth. Gods and discoveries are not made in a day. It is often good that the creating mind should be forced to create in solitude and thus to perfect its work for an immortal existence.

The Weakness of Committee Government

Nearly all administration in Britain is dominated by committees in some form or another, and it is surprising that no one appears to have discussed in a scientific spirit the advantages or disadvantages of this. We are told that there is wisdom to be found in a multitude of councillors; but, on the other hand, we know that too many cooks spoil the broth; and there is also a widely prevalent idea that persons who are good at talking are seldom good at doing. Probably committees like other things have their advantages and their disadvantages

When a private individual is confronted by some difficulty, especially one which requires a knowledge of details which he himself does not possess, he generally consults his friends on such points. Here he recognises the wisdom of the first theorem mentioned above; but, be it clearly seen, he merely approaches his friends for the purpose of obtaining information and rarely allows them to decide finally upon his course of action—that is, he reserves the executive power to himself. Now the second proverb means that where a large number of people are asked, not for information, but to decide upon a course of action, they will engage in endless disputes as to the amount of salt and pepper to be put into the dish—with the result that the poor consumer may get indigestion. Here in short are the advantages and the disadvantages of committees. A number of men possess a wider field of information than any single individual is likely to have; but on the other hand a number of men would seem to be executively less efficient than a single brain may be.

Those of us who have the misfortune to be forced to attend many committees will easily understand the proposition. A director or executive officer is frequently much helped by information obtained by him at committees—so much so that if he acts without such information, he may do the wrong thing. On the other hand, the executive decision of a committee seldom assists him, and indeed often plunges him into a state of frenzy or despair, or both. Suppose that he has been considering some course of action for many months past and has examined every detail of the principle or tactics involved. He then places the matter before his committee with a carefully written note on the subject. But when he comes to the meeting he finds that few of the members have really read his note. Many of them are busy men; others are lazy men; others have preconceived notions for or against his idea; and many others want above all things to say or do something new. He is now confronted with a task like that of a boy who has to drive a herd of cows through a gate—the cows disperse on all sides, one runs through, and while he is forcing the second one through, the first one runs back, and so on! Then again the members often want to go home to have tea or to smoke. The difficulty is usually ended by the Chief Talker of the committee, Mr. Tapper Tongue, who suddenly sees an opportunity

of doing something brilliant, who has seldom understood the business, but who now proposes a resolution which will end the matter—rightly or wrongly. Next, the Chief Guineapig, Mr. Heaviside (who is always a friend of the Chief Talker), seconds the resolution—which is generally so absurd that the poor Director, who knows the business, is rendered speechless with astonishment. Before he has time to collect his wits, the committee smell tea and cigarettes and pass the resolution "nem. con." That finishes it; and the Director is not allowed to bring up the resolution again in subsequent meetings, because the Chief Talker and the Chief Guineapig will say that it has already been decided.

The popular contempt of expert talkers is generally fully justified. The art of talking to an assembly really means the art of extracting one or two salient points and suppressing all the rest of the matter. Now in practice it is generally the details, and sometimes what may at first sight appear to be the smallest details, which really count. Moreover, "the pigmy's virtue, eloquence," is not admired by persons capable of doing anything—who recognise that it is something transient and unessential. The Chief Talker of the company is therefore frequently the weakest unit of the company—useless for everything save to obscure counsel and to clog action. But it is precisely this type of man who dominates committees. We see him everywhere—with "character" written on his brow, "determination" apparent in his chin, and folly (not apparent) in his poor brain. But whatever he says the Guineapigs are sure to endorse. That is why committees are useful for collecting counsel, but generally useless for managing affairs. Committees, like chains, are weaker than their weakest link.

After all, decision as to action requires a rapid integration of all the facts, possible only to a single brain. Suppose that I have been studying a subject for months; then I am quite sure that I have a much greater grasp of it than a dozen men collected at random, even though they are more or less acquainted with the subject. Hence my decision as to action will be of greater value than theirs. Committees may have a wider field of knowledge, but each member considers a different term of the series, and none integrates the whole series. Yet the justice of the decision depends entirely, not upon single terms, but upon the sum of all.

It may be a different matter with very small committees, say of three or four persons, all experts in the subject discussed, and all bound by personal interest to find a correct solution regardless of visions of tea and cigarettes. But even with them things are apt to go wrong in a second by some chance humour of the moment.

Such considerations apply especially to the case of democratic governments. Parliaments are nothing but enormous committees. If they have to sit for long hours on the one hand, on the other hand their work covers a larger field than with most committees, and the disadvahtages of committee government belong to them as much as to smaller bodies. And these disadvantages are emphasised by the fact that members of parliament are by no means always personally interested in the rightness of a decision—often indeed quite the reverse; while they must always spread the plumes of their eloquence and the tails of their wisdom before their admiring harem of constituents. Lastly parliaments consist very largely indeed of professional talkers, all dominated by the sentiments of Mr. Tapper Tongue on committees. That is why (and it is an old observation) democracies mismanage so many things, in war, law, education, sanitation, down to the smallest social details.

What remedy is there? First and foremost to reduce the number of members. This will tend automatically to exclude the shallow talkers and the local jobbers because each member must be elected by a much larger constituency, so that the parliament is likely to consist chiefly of approved men of a higher type. Of course, even this will not exclude Mr. Tongue; but it will tend to exclude the Heavisides who are his principal support.

This war has shown that modern methods of government, autocratic and democratic, are complete failures. The war has been sprung upon a public which had no desire for it, simply by mismanagement—chiefly by the brigands of Potsdam, but also not a little by the demagogues of Westminster and Paris. It is worth while studying why the modern methods of government have failed and where exactly a remedy may lie. But this is a subject upon which a book could be written. Unfortunately few would read it.

A Criticism of the Financial Operations of the Carnegie Trust for the Universities of Scotland (Frederick Soddy, M.A., F.R.S., Professor of Chemistry in the University of Aberdeen)

MR. ANDREW CARNEGIE on July 7, 1901, signed a trust deed bequeathing £2,000,000 to the Scottish Universities, which was recorded in the Books of Council of Session on July 9, 1901. The Trust Deed opens as follows:

"I, Andrew Carnegie, of New York, and of Skibo, in the County of Sutherland, having retired from active business, and deeming it to be my duty and one of my highest privileges to administer the wealth which has come to me as a trustee on behalf of others, and entertaining the confident belief that one of the best means of my discharging that trust is by providing funds for improving and extending the opportunities for scientific study and research in the Universities of Scotland, my native land, and by rendering attendance at these Universities and the enjoyment of their advantages more available to the deserving and qualified youth of that country to whom the payment of fees might act as a barrier to the enjoyment of these advantages; and having full confidence in the Noblemen and Gentlemen afternamed, . . ."

A list of Trustees follows, to whom the donor undertakes to entrust "Bonds of the United States Steel Corporation of the aggregate value of Ten Million Dollars, bearing interest at 5 per cent. per annum, and having a currency of fifty years."

It is only with the first of these objects, the improvement and extension of the opportunities for scientific study and research, that this criticism is concerned.

In a document signed by Mr. Carnegie, entitled "Constitution of the Trust referred to in the foregoing Trust Deed," the two objects of the Trust are referred to under Clauses A and B respectively, and a third clause, C, provided for any surplus income.

Clause A opens:

"One-half of the net annual income shall be applied towards the improvement and expansion of the Universities of Scotland, in the Faculties of Science and Medicine; also for improving and extending the opportunities for scientific study and research, and for increasing the facilities for acquiring a knowledge of History, Economics, English Literature, and Modern Languages, and such other subjects cognate to a technical and commercial education as can be brought within the scope of the University curriculum, by the erection and maintenance of buildings, laboratories, class-rooms, museums, or libraries, the providing of efficient apparatus, books and equipment, the institution and endowment of Professorships and Lectureships, including post-graduate Lectureships and Scholarships, more especially Scholarships for the purpose of encouraging research, or in such other manner as the Committee may from time to time decide. . . ."

The two passages cited from the official copy, issued by the Carnegie Trust, of the Trust Deed and the Constitution of the Trust referred to in the foregoing Trust Deed, respectively, contain all that is germane to the present criticism.

But a reasonable interpretation, and the one initially followed in the two larger of the Scottish Universities, Edinburgh and Glasgow, would seem to be that the money was given for the primary purpose of encouraging scientific study and research, including, of course, medicine, and that history and other subjects cognate to a modern education were legitimate ancillary beneficiaries under the Trust, and, lastly, that the older subjects of a classical education were entirely excluded from participating.

Thus for the first period of ten years and nine months, up to September 30, 1913, covered by the first two quinquennial and interim distributions, in Edinburgh

62 per cent. and 15 per cent. and in Glasgow 67 per cent. and 19 per cent. of the total sums received were allocated by the Trustees to what have been termed the primary and ancillary objects respectively. The remaining 23 per cent. and 14 per cent. in the two institutions have gone mainly to the maintenance of the libraries and other purposes in which the two sides share more or less indefinitely. In neither institution was any money given definitely to benefit what have been termed the classical group of studies.

If this had been the interpretation adopted generally, and subsequently to 1913, by the Carnegie Trustees, certainly no one would have been disposed to criticise them, or submit the legality of their operations to the test of the powers responsible for the observance of the Trust Laws of Scotland. Neither would there have been any disposition to examine with a microscope the exact apportioning of the moneys between the two sides. If they had secured a broad common-sense distribution among the primary and ancillary objects, the gift was handsome enough in amount not to necessitate the making of fine distinctions. But this interpretation has not been followed, either universally, or subsequently to 1913. In the University of Aberdeen for the whole period up to September 30, 1918, covering the first three quinquennial and interim distributions, only 23 per cent. has been allocated to the primary object, while 46 per cent. has gone to the ancillary object. The maintenance of the Library has taken 12 per cent., and there remains 19 per cent. This has been allocated for the erection of new buildings and examination hall for Arts subjects and an extension of the Library, objects which, in so far as they are not illegitimate, are ancillary. So also, since 1913, it is in Glasgow and Edinburgh. The former is given 90 per cent. of its total allocation for five years to "Buildings for Faculty of Arts and Department of Zoology," and Edinburgh 65 per cent. to "Chemical Department and Arts accommodation." As regards St. Andrews and Dundee, the position in the main is between that of Aberdeen, on the one hand, and Edinburgh and Glasgow on the other. But the practice of slumping legitimate and questionable expenditure under one head, illustrated above in the case of Glasgow and Edinburgh, and the payment of debts previously incurred, make a detailed analysis difficult to the outsider.

In addition to payments to the four Universities, and relatively small grants to Technical Colleges and other institutions, the Carnegie Trustees administer themselves a scheme for the endowment of Research. Of a total in round figures of some £621,400 spent under Clause A to September 30, 1915, £86,000 or some 14 per cent. have been spent on the research scheme, that is, £27,000 on Fellowships, £30,000 on Scholarships, £21,000 on Grants in Aid, and some £8,000 on a Research Laboratory of the Royal College of Physicians of Edinburgh. One might fairly have expected that something more than 14 per cent. would have been spent on the research scheme. The answer may be that initially Scotland was ill-equipped with scientific laboratories, and these had first to be provided. But now that these laboratories have been provided, the money is going to provide buildings for Arts subjects to a very questionable extent, instead of to promoting scientific study and research.

But even what has been done has not been done for research so much as for the teaching of research, a highly important and worthy object enough, but only to be confounded with scientific research by those who have never done any or even been taught the methods. Research Scholarships and Fellowships are excellent in themselves, and will be even more so if, as a result of the war, something less like starvation awaits the holders at the end of their research training.

Grants in aid of research are again excellent, and would be more so if they were given when they were wanted; whereas, to suit the conditions laid down by the Trustees, the money has to be applied for before a definite date in the year before it is wanted. But of the three indispensable requirements for getting research done, these two, the training of the apprentice and the provision of money for instruments, are preliminary. The third indispensable, letting the trained man with the instruments do the research, is the one this country has not yet thought much about.

At the bottom of the ladder, the Research Scholar or Fellow at the end of his training has had to abandon the work for which he was training and seek a livelihood. If he is lucky he will get a teaching position, and if, again, he is lucky he may find odd moments to continue his researches. If he is not so lucky he has to begin late in life the study of the art of earning a living. The Professor at the top, nowhere more than in Scotland, finds that he must now be content to do his research by deputy, and the most he can hope for is to train clever apprentices. Some subjects, naturally, lend themselves to this requirement very much better than others, and what is possible in them is not possible in general. The real business for which the Professor is paid, again nowhere more than in the land to which Mr. Carnegie gave his millions, is to teach. Instead of being treated as a life-business, requiring years of devoted training and study for the preparation, and equally devoted and uninterrupted application for its pursuit, research is treated as a hobby to be followed by busy teachers in the intervals of their regular duties. This is not the way to foster perhaps the most important and repaying of all the State's numerous activities. The Carnegie Trustees have not even attempted to meet this difficulty.

The Annual Reports issued by the Carnegie Trust do not contain the names of the Trustees. The original list in the Trust Deed consists of fourteen nominated members, two of them, Lord Kelvin and Sir Henry Roscoe, having in the past contributed to the advancement of natural knowledge, four ex-officio members (the Secretary of State for Scotland, and the Lord Provosts of Edinburgh, Glasgow, and Dunfermline for the time being), and four members elected by the Universities. The vacancies in the nominated members are filled up by the Trustees remaining.

The nominated Trustees apparently hold office for life, and consist almost entirely of eminent public men, more or less universally known, many of them distinguished in History, Literature, Philosophy, and the Law, that is, in the ancillary or illegitimate rather than the primary group of studies. Moreover, the branches of the ancillary subjects in which they are distinguished are not those cognate to a technical or commercial education. The two original scientific members are dead, as also is Sir Arthur Rucker, who replaced one of them. In the case of all three, their career of active scientific investigation had practically closed before they were appointed. In no case, so far as the writer is aware, has an active scientific investigator been a Trustee. At the present time there does not appear to be a single scientific man on the Trust. Of the four Trustees elected by the Universities, two are distinguished members of the medical profession; a third, Sir William Turner, having lately died. The legal profession, past and present Cabinet Ministers, and public administrators supply the whole of the present nominated members. Sir Henry Roscoe's death removed the only scientific member. The others are: Earl of Elgin and Kincardine, Earl of Rosebery, Lord Balfour of Burleigh, Lord Kinnear of Spurness (ex-Senator of the College of Justice), Lord Reay of Reay, Rt. Hon. A. J. Balfour, Viscounts Bryce and Morley, Lord Shaw, Rt. Hon. H. H. Asquith, and W. J.

Dundas (Crown Agent for Scotland). It is mere hypocrisy to expect from a body so constituted, to the majority of whom the words science and scientific research mean little more than the letters out of which the words are composed, an equitable balance between science and the other subjects cognate to a technical or commercial education. Either they should be totally neutral as regards the two competing beneficiaries, or they should be reconstituted to give a representation to each side in accordance with the intentions of the founder of the Trust.

In the general awakening to the national importance of giving fair play to science, and especially to scientific investigation in the Universities, it is to be hoped that the composition of the Carnegie Trust and its record of work under Clause A will not escape unchallenged.

It would indeed be strange if out of between one-half and three-quarters of a million pounds interest available for the promotion of scientific study and research, science had not benefited at all. That of course is not alleged. But the almost total lack of representation of living science on the Trust, and the over-representation of the humanistic element, has made for fatal timidity and lack of imagination and originality in the application of the moneys, so far as the primary object of the benefaction is concerned. There is no automatic retiral of members annually, or provision making them ineligible for re-election till after an interval, which has been found to be necessary, from experience, for good and effective management. Of a body so constituted, probably the best and worst that could be said is that they were given a unique opportunity to promote scientific study and investigation, and even if they had had the best will towards science in the world, they could not have grasped it, because that is a branch of human endeavour which the overwhelming majority had not explored for themselves. In these circumstances a secretary who had some acquaintance with scientific study and investigation might have been of service to them.

No doubt their difficulties were enormous in connection with the peculiar relations to the Universities in which they were thrown, but the difficulties have proved the master. The nation should look for something more real in the promotion of scientific study and research in the future from the million pounds which Mr. Carnegie gave for the purpose.

It would not be fair to saddle the Carnegie Trustees with the responsibility, at least before it has been pointed out to them; but their attention and that of the public may be directed to a very important cognate question, How much of the grants from the Carnegie Trust nominally given to science is diverted from that object? Special information, not contained in every case in the financial statements of the Universities presented to Parliament, is needed in this inquiry, and this must excuse the writer's inability to consider any but his own University, and indeed little more than his own department, of which naturally he has the fullest information.

The one scientific post in Aberdeen endowed by the Carnegie Trust is the Lectureship in Geology. The endowment, £12,632, and an annual grant of £1,000 towards equipment of the laboratories, practically exhaust the Trust's scientific allocations in this University. In the early years a total sum not exceeding £2,500 in addition went in small increases of from £75 to £50 in the salaries of some half-dozen science lecturers and assistants. In the published accounts, the interest of the Geology endowment to the extent of £400 is stated to have gone to the payment of the Lecturer's salary, and the part payment of that of an assistant. But, taking 1913–14, the year before the war, the students' class fees, £505—

mainly derived from the second Carnegie million, administered under Clause B—alone, without counting an equivalent proportion of examination and degree fees, more than paid the total salaries of the Geology staff, £475. If the examination and degree fees are included and the external examiner's salary deducted, there remains a balance of £173, which is more than enough to wipe out the item of £128 which the department is credited with receiving from the Carnegie Trust out of the annual grant of £1,000 for equipment. Thus so far as the main provision for science by the Carnegie Trust in Aberdeen is concerned, the money is diverted to other purposes.

The Chemistry Department, when the writer was appointed to the Professorship, was credited in the 1913-14 accounts with the receipt of £534 of public money—that is, £149 from the Carnegie Trust out of the annual £1,000 grant for equipment, and £385 from the Exchequer. Nevertheless, counting in an old endowment which brought in £194, it was entirely supported by the class and examination fees paid by the students taught, without this £534, which was diverted to other purposes.

By the Act of 1889 all financial control and responsibility was taken out of the hands of the Professors, and vested in the University Court, who were enjoined by Ordinance 26, Clause V, to keep a separate account of the fees, distinguishing those drawn from each class, and by Clause XI, in providing for the educational needs of the several Faculties, to have due regard, *inter alia*, to the contributions made by the Faculties respectively to the funds of the University.

Latterly the accounts have ceased even to attempt to conform with the first of these obligations, and for lack of this information it is impossible to say where the moneys nominally given to Chemistry and Geology really go. It is not suggested that they go to Arts or Law, or any particular Faculty, specially. The Court alone can give the necessary information.

A questionable system seems to be in vogue, euphemistically known as "saving the General Fund," whereby grants of public money are given not directly only to such departments as are spending more than they earn, but even to those like Chemistry, which are earning what they spend. The Court is under obligation to report to the Government and the Carnegie Trust annually the manner in which the grants have been expended, and the nominal purpose reported is not in all cases the real one. It is not a question of principle, whether a flourishing department ought to support one that is not, but of straightforward bookkeeping. Moneys are given to a department A, the effect of which is to transfer the equivalent amount of fees to another department B. A is credited in the annual statements with the receipt of the money, but B gets it. Why is not B given the money directly instead of A, and the transaction recorded in the accounts? The answer is that though A, by the terms of the gift, is necessarily a proper recipient, B may or may not be.

Whatever may have been the abuses of the régime before the Act of 1889, the fact that such a subject as Chemistry at least would have been better off, if it had been left as it was, in spite of all the wealth from the Government and the Carnegie Trust, which has since come to the coffers of the University, is a sufficient indictment of the present system.

Enough has, perhaps, been said to show that some inquiry not only into the Carnegie Trust, but also into the manner the financial system of the Scottish Universities is operating, is called for. It is not mainly a question of money. Money is merely the measure. Here is a department, original investigation in which, it has been shown, is vital to the future prosperity of the country. It

supports itself by hard teaching. It is stated in the published accounts to receive sums which it in fact does not receive, and which if it did receive would enable the teaching staff to be increased and some time allowed for research. It is idle for any public or private benefactor to give money for a specific object, such as the improvement and extension of the opportunities of scientific research, until the system is overhauled which makes it possible for moneys so given to be diverted.

The University fees go almost wholly into the one "General Fund" created by the Act of 1889. The departmental expenses are borne by grants from this fund, from the public money provided by the Exchequer, and by the Carnegie Trust. Hitherto the giving of a grant to a department has often meant merely the diminution of its grant from the General Fund. If the departments are all stereotyped as regards the amount of tuition performed, it is obvious that the simultaneous gift of public money and the withdrawal of the same amount of fees would not benefit the department in the slightest, nor lessen its burden of tuition. But if, as is the case with a subject like Chemistry, the fees earned and burden of tuition, of which they are to some extent the measure, are rapidly growing relatively to the rest of the University, each year must increase its burden and lessen its power of original production, its increased earnings all the time going to make up corresponding losses of fees in other departments. This has got to the point with the Chemistry Department of Aberdeen that it has actually become self-supporting, though nominally receiving large grants of public money. It would be better off if it had been left as it was before 1889 in possession of its own earnings, and without the sort of assistance it receives from the Carnegie Trust and the Government. Until this matter is looked into, it is useless for the Chancellor of the Exchequer or the Carnegie Trust to grant further moneys to the Universities if their object is to foster those departments which are becoming of increasing national importance, and for which there is growing up an increasing demand.

The British Association Meeting, 1916

The precedent in regard to the curtailment of the customary social side of the British Association Meeting and of the visits to works set at the previous meeting was followed again last autumn at Newcastle, for, as the Lord Mayor humorously reminded the visitors in welcoming them, the town was a "prohibited area." In spite of this and of the somewhat small attendance the interest of many of the papers and the usefulness of the discussions should be a sufficient answer to those who had maintained that the meeting should not be held. The war, the one business of the country at the present time, naturally influenced the character of the proceedings in practically all the sections. The discussions as a rule dealt with work of national importance, the application of scientific knowledge and methods to the nation's needs, etc., either in progress or in contemplation. At such an annual gathering it was impossible to avoid thinking of those met in former years, who had laid down for their country a life full of promise.

In his address the President dealt with "New Archæological Lights on the Origins of Civilisation in Europe; its Magdalenian Forerunners in the South-West and Ægean Cradle." It was a well-thought-out and attractively written account of the old civilisation of Europe, a subject on which few men have ever been better qualified to speak than Sir Arthur Evans. After referring to the Roman occupation of Britain, and paying a well-deserved tribute to the good work done by the local antiquarian society, the President glanced briefly at the Minoan civilisation which may be regarded as the forerunner of that of Greece and Rome.

The quest goes to more and more remote times, to the famous Caves of Altamira, of Marsoulas, and of Font de Gaume, which were in use "at a modest estimate some ten thousand years earlier than the most ancient monuments of Egypt or Chaldea." Here are pictures in colours, monochrome drawings and sculptures of the men of the Reindeer Age that reveal an astounding level of artistic culture. Back into these ages one can trace characteristics of religious belief and of mundane life formerly held to be representative of much later periods. Evidence is forthcoming to show that such culture was not isolated, but spread from Poland and Russia to the Danube, the Rhine and parts of Spain and Britain. Later, in the rock shelters of Alpera, are depicted Palæolithic bowmen taking part in the chase accompanied by dogs, and also two rival companies of archers.

Continuity of civilisation from this time onwards can be traced with some certainty, although the author opines that the junction of Palæolithic and Neolithic has not yet been recorded. Although more was known of the Neolithic much has been added of recent years, and upon it the later civilisations were undoubtedly built. The whole story is one of a wonderful interest, and its fascination is kept up throughout.

In Section A (Mathematical and Physical Section) the presidential address, delivered by Prof. A. N. Whitehead, concerned the "Organisation of Thought." Stress was laid from the outset upon the intimate relation between theory and practice, and science likened to a river coming from these two sources, both of which are equally important. Science itself is defined as the thought organisation of experience, and a criterion of its success is its ability to formulate successfully empirical laws. From its nature science is logical, and after ascribing the barrenness of logic for the past three or four centuries to its worship of authority, the author proceeds to point out that there are four departments of logic. These are characterised by their main aspect as arithmetic, algebraic, of general functions, and analytic, the last including mathematics. The address closes on the note of the interrelatedness of observation and logic, for "neither logic without observation nor observation without logic can move one step in the formation of science."

The present position and future prospects of chemistry form the main theme chosen by Prof. G. G. Henderson for the presidential address to Section B (Chemical Section). The main subject provided a number of side lines on which it was possible to make a trenchant criticism of the laxity of the nation in regard to things chemical, and more particularly the deadening attitude towards them adopted by persons in power in the government of the country. A very brief and general review of the progress in pure chemistry is given, noting particularly the discovery of the Argon family of gases and radio-active elements. Applied science has also made remarkable strides in the economic production of materials at high and low temperatures, and the employment of electrolytic and catalytic methods. Particularly important is the invention of the electric furnace, which has enabled many useful substances to be fairly readily produced. In all these matters England has taken her fair share. Certain proposals affecting the future are also briefly dealt with. Some of the recommendations of the Advisory Committee for Scientific and Industrial Research are approved, as is also the suggestion to establish an Intelligence Bureau of the Board of Trade. A general satisfaction with the possibilities of training young chemists is expressed, but attention is directed to the need for the chemist to have a training in research, and also in the desirability of manufacturers, individually or in association, founding industrial research scholarships, thereby bringing the academic and manufacturing worlds into closer contact.

The underlying note in the address to Section C (Geology) by the president, Prof. W. S. Boulton, is the same as in the foregoing—the urgent need for the organisation of our national resources, and also the need for more sympathetic and generous assistance from the Government. The particular subject dealt with is the outlook in economic geology at the present time. The general criticism is put forward that this country has lagged behind our Colonies and America, and that it does not occupy the position which its tradition and opportunities should have given it. The important Special Reports on our mineral resources are very welcome, and should be continued and increased. A more systematic economic survey of the country is much to be desired, and can only be carried out by means of the assistance of deep borings. Three of the main requirements, the coal, the petroleum, and the underground water, are reviewed in some detail.

In Section D (Zoology) the president, Prof. E. W. MacBride, dealt with the organ-forming substances in the developing eggs of animals. The opening part of the address contained an historical *résumé* of the early workers in the field of cellular embryology, His, Roux, Hertwig, and Dreisch. An account was given of the experiments of Crampton and Wilson on molluscan eggs, in which we have the first definite evidence of the existence of such organ-forming substances. By various means, *e.g.*, centrifuging developing eggs, it is possible to upset the distribution of these bodies within the egg cells, and consequently alter the proportions in which they are handed on to daughter cells at division. This leads to such a disorganisation of the processes of development that at present, at any rate, it seems most easily explained by postulating the existence of certain substances in the egg that to a certain extent control the course of development.

In Section E (Geography) the president, E. A. Reeves, delivered an address on "The Mapping of the Earth—Past, Present, and Future." The need for some kind of mapping to decide boundaries or indicate the position of distant objects was urgent even to man in his savage state. This proposition is followed by a brief history of the means of obtaining bearings from the time of Thales in the seventh century, who taught the sailors of Ionia to steer by the Little Bear, and onwards. Short but interesting accounts are given of the various instruments used by the Greeks, and the subsequent improvements on them down to the wonderful ones available to-day. In each case some idea of their accuracy is provided, and reference is also made to some of the errors still remaining that can be allowed for to a certain extent, but not controlled. Two maps of the world, one of 1860 and one of 1916, are given, on which the mapped areas are indicated by different shadings to indicate the degree of accuracy of the maps. This enables the reader to appreciate readily the expansion of modern surveying and also the statistics that are given concerning it. A tribute is paid to the work of the Royal Geographical Society in the assisting of explorers in many ways. As regards the future, the surveyor will have great advantages over those of the past, not only in the fact that he is in possession of instruments of much greater precision, but the triangulations already existing in many places form a good basis for future work.

"Some Thoughts on Reconstruction after the War" was the subject chosen for the president's address to Section F (Economic Science and Statistics), by Prof. A. W. Kirkaldy. A brief review of the effect of two years' war on the various aspects of life in this country leads directly to a consideration of what will take place afterwards. Both ourselves and the Central Empires will have certain advantages in the struggle for recovering stability, the more substantial of these

remaining with us. In order that we should utilise these advantages to the full, it is, in the Professor's opinion, absolutely necessary that there should be much more complete national organisation of the various trades than exists at present. On the whole, the author does not favour too much intervention by the State, but looks rather to a more efficient expansion of trades unions on the one hand and employers' associations on the other. Not merely should they be associations of one trade, but local and national federations of trades on both sides. It is imperative, as all will agree, that both employers and employed should insist on all members of their respective associations carrying out honourably all agreements. As a court of appeal and a general board of control would be an industrial council similar to that established in 1911, but with more power, and of such constitution as to ensure the respect and confidence of all parties.

The address to Section G (Engineering) by the president, G. Stoney, is mainly a criticism of the faults that have characterised workmen and employers in the engineering and allied trades in the past. The Government also has its share of the blame for its niggardliness in supporting scientific research. Reform is looked for in the direction of better relations between master and man, between different firms in the same industry, and an alteration of our educational system, including, among other things, better provision for the young man in a works to continue his education.

Section G (Anthropology) listened to an address on "Anthropology and University Education" by the president, R. R. Marett. A tribute was paid to the memory of Sir Laurence Gomme, who should have been president. It is a remarkable thing that a science of such standing as Anthropology should at the present time be so treated that it is necessary to enter a plea for its inclusion in University curricula. The whole address is devoted to a definition of the aim and scope of the science, and its various auxiliaries, and a claim that it should be more completely recognised by the Universities. Its value as an education for producing an "all-round man" is insisted on, and its requirements in the way of equipment and organisation pointed out.

Prof. A. Cushny, president of Section I (Physiology), dealt with the analysis of living matter through its reactions to poisons. The action of drugs upon the body is selective, that is, a certain drug may act on only certain very limited parts. In a general way certain substances act only on the central nervous system, but again some of them only affect very limited parts of the individual nerve cells, or their connections with other cells or tissues. Again, the same compound may act on two or more different parts, between which there appears to be no morphological connection, thus probably indicating some chemical or structural similarity between the two. The degree of susceptibility of certain cells may indicate the amount of a certain substance it contains. All these various points suggest lines of attack on the complex problem of the ultimate structure of protoplasm.

The part that can be played by Botany in the improvement of our crops, etc., and the further exploitation of the economic wealth of the Empire, was treated by A. B. Rendle, the president of Section K. There is need of closer co-operation between the practical man and the botanist, and this might readily be met by an extension of the scheme inaugurated by the Royal Horticultural Society at Wisley. More schools for the efficient teaching of botany, not to those intending to be professional botanists, but practical men requiring some scientific training, are needed. There is also room for a more intensive study of plant diseases, the anatomical study of timber, and the chemical investigation, not only of wood, but of plant products in general. Most of these could be more easily done if

closer co-operation between various institutions and colleges could be brought about.

The presidential address to Section L (Education) was given by W. Temple. After a protest against the tendency to mould our national educational system on German lines, he passes on to give his idea of education. The address deals mainly with public schools and university, and the various components of a complete education, classics, sciences, physical development, and moral training are considered. A strong claim is made for religious education on the ground that only two educational systems are possible, "one is the religious, the other is atheistic."

In view of the conditions that the war has brought about in this country, it would be hard to find a more fitting subject for the presidential address to Section M (Agriculture) than was chosen by E. J. Russell. A short history of the development of agriculture is given and followed by a critical survey of the present position of that industry. The scientific investigations into the question of the rotation of crops, manuring, and the treatment of various soils were all passed under review. In all cases immediate lines of improvement in view of our present knowledge were suggested, and what is perhaps more useful from the scientific point of view, the various gaps left and the difficulties to be faced, some of which can be tackled straight away, were clearly indicated. It was also claimed that much can be done to bring scientist and farmer into closer relations.

Many of the sections held discussions on subjects of national and economic importance. The dominant note running through all these was the same, a considerable alteration in the present conditions is absolutely necessary for the future welfare of the country. The Government on the whole has been terribly negligent of science and its advice, there is too great a gap between theory and practice, and there must be a great deal more co-operation and co-ordination in all our activities. Whether these warnings will be taken and the exceptional opportunities for improvements now available be utilised to the full, only the future can tell. If not, it will indicate a state of ignorance or apathy on the part of the country at large with which it will be hard to cope, and which bodes ill for the future of the Empire.

C. H. O'D.

Geological Notes of Queensland

From the Gulf of Carpentaria to the Darling Downs, north to south, the fossil remains of extinct mammalia have been found in indurated muds, the beds of old watercourses. The fossils are *Diprotodon australis*, *Macropus titan*, Thylacoles, Phascolomys, Nototherium, crocodile teeth, etc. The Diprotodon inhabited the Queensland valleys freely, and the *Crocodilus australis* had a great range inland. The Diprotodon remains are found chiefly in the most permanent waterholes. No human bones, flint flakes, or any kind of native weapons have yet been discovered with the extinct mammalia of Queensland.

Desert sandstone is the most recent widely spread stratified deposit developed in Queensland. Since it became dry land the denudation of this formation has been excessive, but there is still a large tract *in situ*. Probably this desert sandstone covered the whole of Australia at one time. (It is possible that desert sandstone in Queensland has value for free gold.) On the vast plains west of the dividing range cretaceous strata are found; hot alkaline springs occur in these plains, and the discovery of these suggested the possibility of the existence of

artesian water long before the bores were sunk from which flow "Queensland's rivers of gold."

The whole of Queensland is a vast cemetery of fossilised species—on the surface, buried in drifts, or hidden in clays. The plains of the Flinders river disclose great deposits of marine fossil shells, belemnites and ammonites, and remains of extinct animals. In the Gulf of Carpentaria, 40 or 50 feet below the alluvial deposits forming the banks of rivers, firmly embedded in the hard cement—water-worn stones in an ironstone clay—are the bones of innumerable extinct gigantic animals that, far back in some prehistoric age, roamed over the Gulf country: Diprotodon, Nototherium, and Zygomaturus—grass-eaters and flesh-eaters. The utter extinction of these creatures can only be explained by a great change of climate and great and lengthy droughts. The fossils are from animals of immense size; the teeth found are twice the size of an ordinary bullock's. Gigantic alligators and turtles and marsupials abounded in those days, suggesting a luxuriant and abundant herbage.

From an economic point of view one may say that three-fourths of the area of Queensland forms good pastoral land. Of this, 60,000 square miles contain valuable mines of gold, with outcrops of copper and lead ores, as well as rich deposits of tin; 24,000 square miles are capable of producing illimitable supplies of iron and coal. It may be safely asserted that in Queensland is a wealth of material resource comparing favourably with any other part of Australia.

—THE LONDON CORRESPONDENT OF "THE NORTH QUEENSLAND REGISTER."

Poland

A series of eight pamphlets issued by the Polish Information Committee and published by George Allen & Unwin place before the reader a complete picture not only of Poland's past history when she was autonomous, but her condition and struggles after the partition. This account arouses one's admiration for the vitality and strength of purpose in the Polish people who, against almost insuperable odds, have preserved their language, nationality, intellect, and desire for freedom. The fifth paper of the series, by A. E. Gurney with a preface by Ludwik Janvoski, gives facts to prove that the Polish population has increased rather than diminished in spite of the strenuous efforts of Germany, Austria, and Russia to absorb it. The eighth pamphlet, by L. Litwinski, with a preface by Lord Bryce, furnishes an interesting description of Poland's intellectual activities, particularly in the realm of science, carried on in the face of every difficulty with which her conquerors could hamper her. Similar literature is being published to-day by the Jugoslavs, also crying out for racial nationality, all of which seems to point to the idea that extension of frontier by any one country is to the world a loss rather than a gain. Much effort is put fruitlessly forth by the conqueror to absorb the conquered, which of necessity calls forth corresponding efforts of resistance—unproductive energy on both sides which could be so much more profitably expended in the furtherance of science, literature, and art.

"The Australian Manufacturer."

The *Australian Manufacturer*, an excellent illustrated paper, published weekly at the price of sixpence, contains interesting information for the manufacturer and general reader alike. Although primarily intended for Australian consumption, much of its sound advice is equally applicable to the Mother Country. This is particularly true of the first article in the issue for June 17 last, "How to save

Australia," in which Australians are exhorted to make the art of talking subservient to the art of doing. The writer says, "We have to realise that our ultimate salvation depends, not on high duties, or on loud oratory, or on vigorous flag-waving, but on the patient and noiseless application of sound scientific methods to our business and our commerce. Some people talk as if all that is necessary in order to ensure prosperity is to build a tariff wall and then sit down in its shadow and wait for the good times that are sure to follow. In the opinion of this paper, even were the tariff wall as high as heaven, it would avail nothing, unless on this side of the wall there happened to be an abundance of industrial energy, guided by science and sense." The writer also deplores the "pathetic trust in politicians," maintaining that this "belief in the magical effects of Parliament and legislation is one of the superstitions of the age." In a short article entitled "The Philosophy of the Drink Question," the introduction into public-houses of the sale of tea and coffee and light refreshments as well as alcohol is brought forward as a possible remedial agent for the present pernicious influence hitherto exercised by these institutions. Dr. R. Greig-Smith's Presidential Address read before the Royal Society of New South Wales is introduced under the title of "Science and Industry: What one might do for the other," and amongst much valuable matter an excellent suggestion is made that the public who are unable, through lack of special training, to read scientific papers in the form in which they are originally written, should have their main facts presented to them by a "scientific journalist," who should "'write up' the proceedings of the scientific societies for the newspapers." The manufacturer and commercial man are catered for in the following articles: "Activated Sludge; a Modern Miracle," in which it is claimed that a practical system of converting garbage and sewage into land fertilisers has at last been found; "Handling Materials in Manufacturing Plants: The Wool Industry," etc. Many pages are also devoted to Current Tenders and a Buyers' Guide, and the reader is furnished with a Table of Contents and an Index to the Advertisements.

This and That (The Editor)

During the last quarter many events of the first importance as regards the business affairs of science have occurred. The proposal of the late Government to consider the whole subject of **Electoral Reform** is evidently fundamental for the proper development of science in this country. In this number of SCIENCE PROGRESS Mr. Cowan gives us a carefully reasoned and valuable study of the subject from the Parliamentary point of view, and I am convinced that most scientific men will agree with his conclusions. His suggestions for reform are simple and immediately practicable, and would place the whole government of the country upon a better and more truly democratic basis. But some of us think that the reforms should go further, and should strike at the very root of the whole system of party politics, so distinctly opposed both to reason and to good administration. It is not possible to consider such points here, but, personally, I think that the party system should be rendered directly illegal by the suppression of party caucuses, party funds, and the clearly unwise methods of touting for votes now used. There is no genuine democracy in this country at all, and the voter is able only to choose alternative names from among the lists of candidates presented to them by the caucuses—so that, in fact, the caucuses, which have no real standing in the constitution, are practically able to prescribe to the voters whom they shall send to Parliament. This and the large expenses of elections suffice to keep out of Parliament numbers of the very best minds in the country, while many persons

who really have small qualifications to enter it can easily do so. Thus, in my opinion, the whole standard of government and of legislation is lowered.

On December 1 a large scientific deputation headed by the President of the Royal Society (Sir Joseph Thomson, O.M.) waited upon Lord Crewe, the President of the Board of Education, at the Institution of Civil Engineers, in order to hear a declaration from him on the policy of the late Government in respect to **Industrial Research.** Lord Crewe was accompanied by members of the Committee of the Privy Council for Research and of the Advisory Council. Sir Joseph Thomson introduced the deputation with a very felicitous and wise speech, punctuated with several brilliant epigrams—such as the statement that "applied science can lead to reforms, but research in pure science leads to revolutions." He pointed out also that assistance could not easily be given to pure research by granting sums of money to individuals for current work—a common fallacy; and showed that a good way to pay for research was simply to help those who are engaged in teaching to undertake it. The fact is that it is often just as absurd to pay a man for future researches as it would be to pay a poet for future masterpieces. As Sir Joseph Thomson pointed out, the results of research are always problematical, and will in fact be frequently almost entirely negative, and I therefore think that payments by the State for such are likely not only to be very often a waste of money, but also to put the person paid in a false position. As a matter of fact, most men of science are engaged in teaching, and a good way to encourage research is to pay teachers of science so well that they can avoid the necessity of adding to their funds by other means and may devote their spare time to investigation.

Lord Crewe's declaration justified the hopes which were expressed in our last issue, when he was appointed head of the Board of Education. The scheme which he shadowed forth was nearly but not quite complete. Greatly increased funds are to be given for industrial research, and money will also be available to help workers in "an area of research which paid nobody in the pecuniary sense for the time being," and "individual research workers who need grants to enable them to go on at all." Exemptions from income tax will be allowed on contributions by traders to industrial associations which may be formed for the purpose of scientific research for the benefit of various trades. A Royal Charter has been granted to the official members of the Committee of the Privy Council for Scientific and Industrial Research, under the title of the "Imperial Trust for the Encouragement of Scientific and Industrial Research." The Trust is empowered to deal with funds, including sums voted by Parliament to that end, and can take and hold land, etc. Dr. H. Frank Heath, C.B., has been appointed permanent Secretary of this new Department, and all correspondence should be addressed to him to the Department of Scientific and Industrial Research, Great George Street Westminster, S.W.

When I heard Lord Crewe's speech, I remained in doubt regarding the exact meaning of his words about the "area of research which paid nobody in the pecuniary sense." Would they apply only to future work, or also to past work which has not been remunerative to the worker but has been of advantage to the public? I therefore wrote directly to the Secretary of the new Department, and he replies that "As regards your question about past work, I think I can supply the answer, which, from what you tell me, will, I fear, be disappointing to you. It will not be within the province of the Department for Scientific and Industrial Research to institute a scheme of rewards for scientific investigations already completed. The purpose of the Department is to promote further work whether by individuals or by associations."

I must agree that this is disappointing, because I am perfectly convinced that much the best and most economical way of encouragingreally important scientific investigations is for the country to pay the worker after he has actually achieved results—much better than to pay workers prospectively for what they are going to do but what they will probably never succeed in doing. This point is really the centre of the whole idea of State encouragement of research, and I am sorry that it was ignored in Lord Crewe's otherwise excellent scheme. The plain truth is, as we have pointed out in SCIENCE PROGRESS over and over again, that numbers of the best minds are excluded entirely from undertaking research of any real value by the fact that even when it is successful it brings them no remuneration for their loss of time or money, thus forcing them to sacrifice either their scientific ability or their children and themselves. As pointed out elsewhere in this number, it is only a form of scientific snobbery that leads persons to pretend that remuneration for their work is of no consequence to themselves. Thus, I will venture to prophesy that Lord Crewe's scheme as it stands at present will scarcely reach the object at which it is aimed, and the country will be paying, not for great researches, but for an infinite amount of petty soil-scratching and pot-boiler work, such as that which now fills most of our scientific publications. I would beg the newly appointed Department to take this matter into its earnest consideration.

Another address of great importance upon this subject, which I am sorry I have no space to deal with in this number, was that given by Dr. R. T. Glazebrook, C.B., F.R.S., Director of the **National Physical Laboratory**, to the Birmingham and Midland Institute on December 4th. Dr. Glazebrook dealt largely with the valuable work of the National Physical Laboratory during the past ten or fifteen years. He complained that the payments of the workers were not sufficient to keep them at the laboratory, and showed how much more the Americans and Germans pay for their similar institutions. We may all express the hope that Lord Crewe's scheme will be of great advantage to this invaluable institution.

The scandals revealed in the trials before Mr. Justice Low in September suggest that a large amount of **corruption** still exists in this country in spite of the war; and those who are acquainted with British administration have long had little doubt of its prevalence. Direct bribery is perhaps not very common among the more educated people, but nepotism, favouritism, good-will, and every form of what is almost the same thing are probably very prevalent. Apart from this however, feeble and lax administration leads to much the same results. The revelation made by Prof. Soddy in this number of SCIENCE PROGRESS as regards the Carnegie Gift for Science to the Scottish Universities provides a case in point. We do not suppose that there is anything like corruption here, but still the money appears to be leaking away from the coffer in which it was originally intended to be placed. In my opinion no reform will be made in this matter until our administration becomes much more draconian. The remedy suggested by Lord Cromer was merely that salaries shall be increased to remove temptation. Perhaps so; but at the same time strict punishment should be meted out for all forms of jobbery and intriguing, and the whole wink-and-vote system which seems to govern us so largely to-day.

As regards science, one of the most urgent reforms required is a root-and-branch reform of our antiquated **learned societies**. For years past many of these bodies have been useful only for the publication of papers, and do little either for science in general or for science in education or for the workers at science, while they seem often to be "run" by a few individuals, while the body of members

have little or no real voice in their affairs. Of course purely private societies may do as they please; but I think that societies which receive Government funds should often be entirely reconstituted by law.

The British Science Guild is proceeding in its activities, amongst which I may note the calling of a conference to consider what amendments, if any, should be effected in the Act of Parliament which regulates experiments on animals; an attempt to abate the disgusting dog nuisance, which results in the universal defilement of the streets in big cities and towns; and the formation of a joint committee with the British Medical Association to consider the payment of medical experts by Government and Municipal Departments. The memorandum lately issued by the Guild for the betterment of science in general has been largely accepted by Lord Crewe in his scheme mentioned above.

Regarding **experiments on animals**, we all agree that they should be regulated by law provided that similar regulations are enforced in the cases of sport and of the food supply, and also provided that such regulations do not cast unnecessary impediments in the way of genuine research. The present Acts, however, do impede researches not a little, owing to the bad organisation of details. Thus, I hear that the Act is causing considerable mischief as regards the proper diagnosis and treatment of cases in the military hospitals, because it hampers hospital pathologists in using animals in a way which modern medical science suggests and requires. This means that our sons who are fighting for the country are not always being treated with the efficiency that they have a right to expect. Another case of bad working of the Act has recently come to my notice. A worker who had not correctly interpreted the sometimes-obscure wording of the Act and of the printed licence, allowed an animal to recover from a slight experiment, although he did not possess "Certificate B," which is required by the Act. In this he was admittedly wrong; but what are we to think of the fact that a Government Inspector was present during the whole of the operation and actually saw the animal recovering from the anæsthetic without warning the said worker that this was not allowed; and then reported him for his error—with the result that the worker was deprived of his licence by the Home Office? I understand that these inspectors are paid by the country for seeing that the Act is properly complied with, and would like to know why this gentlemen took it upon himself to allow the Act to be infringed under his very eyes. It will be interesting to see what the Home Office does in the matter.

ESSAYS

On the Relation of the Theory of Integral Equations to the Subject of the Calculus of Operations and Functions (H. Bateman, M.A., Sc.D., Lecturer at John Hopkins University, Baltimore, U.S.A.)

It has been pointed out by Pincherle[1] that many of the theorems of a general nature in the theory of integral equations are simply illustrations of theorems belonging to the general theory of distributive operations, and this is one of the leading ideas in the development of E. H. Moore's "General Analysis."[2] Moore puts the matter in this way: "The existence of analogies between central features of various theories implies the existence of a general theory which underlies the particular theories and unifies them with respect to those central features." In other words, in the general theory of functional equations there are certain fundamental ideas of supreme importance for the development of all the branches of the subject. What are these fundamental ideas? This is the question we shall now endeavour to answer.

In the first place there is the idea of *iteration*, or the repetition of a single mathematical operation such as multiplication by a certain fixed number. This idea was used by Michael Dary[3] in his method of solving equations (which was described in SCIENCE PROGRESS, October 1915, and January and April, 1916).

Let $x = F(x)$ be the equation to be solved and let x_0 be a number which is somewhere near the root. If we draw the curves $y = F(x)$, $y = x$, we can frequently approach a point of intersection by a series of steps represented analytically by the series of equations.

$$x_1 = F(x_0),\ x\ = F(x_1),\ x_3 = F(x_2),\ \ldots\ldots,$$
$$x = \lim_{n \to \infty} x_n.$$

In this case the operation which is repeated is that of forming a given function of the quantity x. In the theory of integral equations the operation which is repeated is of the following type:

$$f_1(s) = \int_a^b k(s,t) f_0(t)\,dt,\quad f_2(s) = \int_a^b k(s,t) f_1(t)\,dt,\quad f_3(s) = \int_a^b k(s,t) f_2(t)\,dt,\ \ldots,$$

and if we introduce a parameter λ, Dary's equation

$$x = \lambda F(x)$$

has as its analogue the homogeneous integral equation of the second kind.

$$f(s) = \lambda \int_a^b k(s,t) f(t)\,dt.$$

[1] *Rend. Lincei*, 1905; *Bologna Memoirs* (6), 1906, **3**; 1911, **8**.

[2] *Introduction to a Form of General Analysis*, New Haven Mathematical Colloquium (1906), New Haven, 1910; *Rome Mathematical Congress* (1908), *Atti*, 1909, **2**, 98; *Bulletin of the American Mathematical Society*, April 1912; *Cambridge Math. Congress* (1912), **1**, 230.

[3] August 15, 1674. An account of Dary's work is given by W. Stott, SCIENCE PROGRESS, October 1915.

Since the operations are of very different types, the analogy between the two equations is not very close and is confined almost to the fact that the method of iteration can be used to obtain the solution in certain cases, but the details of the analysis are quite different.

The use of symbols to denote operations was advocated by Leibniz,[1] who showed that the operator $D = d/dx$ can in some cases be treated as an algebraic quantity, as for instance in the equation

$$D^m(D^n y) = D^{m+n} y$$

and in the well-known expression for the n^{th} differential coefficient of the product of two functions.

Although this calculus of symbols was developed by Lagrange[2] (1772), the first great step in the theory was made by Servois,[3] who showed that the analogies between the calculus of symbols and ordinary algebra depend on the fact that the various symbols Δ, d/dx, of the differential calculus and calculus of finite differences possess the commutative, distributive, and associative properties: the first two terms were in fact introduced by Servois. Two symbols A, B, are said to obey the *commutative* law if

$$AB = BA;$$

they are said to obey the *associative* law if

$$A(BC) = (AB)C,$$

where C is any other symbol of the same type. If the symbols A, B denote operations it is convenient to interpret AB as the result of first operating with B and then with A. If the two operations obey the *commutative* law, the order in which they are performed is immaterial.

In order to define the distributive law we must introduce the idea of objects or functions on which the symbol or operation acts. If α, β, . . . denote different objects, an operation A is said to be *distributive* when

$$A(\alpha + \beta + \dots) = A(\alpha) + A(\beta) + \dots, \quad A(c\alpha) = cA(\alpha),$$

c being an arbitrary constant.

General analysis can be divided into two main branches according as the operations which are considered are distributive or not. When we limit ourselves to distributive operations we practically abandon the interesting branch of analysis which is generally called "the calculus of functions." The analogies which exist between this subject and the theory of distributive operations depend chiefly on the idea of iteration and the commutative and associative laws.

In the calculus of functions the commutative law suggests the interesting functional equation

$$f[\phi(x)] = \phi[f(x)],$$

which may be regarded as an equation for ϕ when f is given. In the theory of integral equations the corresponding equation is

$$\int_a^b f(s,x)\phi(x,t)\,dx = \int_a^b \phi(s,x)f(x,t)\,dx,$$

[1] *Berol. Miscell.* 1710, 1, 160. Cf. the correspondence of Leibniz and John Bernoulli in Leibniz's *Mathematical Works* (1), 3, 175.

[2] *Œuvres*, 3, 441.

[3] *Gergonne's Ann.* 1814, 5, 93.

which may be regarded as an equation for ϕ when f is given. The theory of permutable functions,[1] a recent development of the theory of integral equations, is based on the last equation. The calculus of functions has been developed largely by Babbage,[2] J. F. W. Herschel, Abel, De Morgan, Boole, Leslie Ellis, and other writers. Babbage uses the notation $f[f(s)] = f^2(s), f^n(s) = f[f^{n-1}(s)]$ and raises the question as to the meaning of $f^n(s)$ when n is not an integer. A similar question has been raised as to the meaning of $D^n y$ when n is not an integer and when D denotes the operator d/dx or some other operator. A theory of fractional differentiation and integration has indeed been developed by Liouville,[3] Boole,[4] Riemann,[5] Heaviside,[6] and other writers, and is of considerable interest in the present subject because it is closely connected with the theory of integral equations. Liouville, in fact, uses definite integrals of the types which occur in the integral equations of Abel and Fourier to define his fractional operations, and it is easy to see how integral equations can be used to solve equations involving fractional derivatives and *vice versa*.

In like manner integral equations can be used to solve functional equations, as the following example due to Leslie Ellis[7] will indicate:

Solve the equation

$$\int_{-\infty}^{\infty} \phi_m(x)\phi_n(a-x)dx = \phi_{m+n}(a), \text{ where } \phi_n(x) \text{ is an even}$$

function of x.

Assume $\psi_n(x) = \int_{-\infty}^{\infty} \operatorname{Cos} ax. \quad \phi_n(a)da$, then

$$\psi_m(x)\psi_n(x) = \int_{-\infty}^{\infty}\int_{-\infty}^{\infty} \operatorname{Cos} ax \operatorname{Cos} \beta x. \quad \phi_m(a)\phi_n(\beta)da d\beta$$

$$= \int_{-\infty}^{\infty}\int_{-\infty}^{\infty} \operatorname{Cos} (a+\beta)x. \quad \phi_m(a)\phi_n(\beta)da d\beta$$

$$= \int_{-\infty}^{\infty}\int_{-\infty}^{\infty} \operatorname{Cos} \gamma x. \quad \phi_m(a)\phi_n(\gamma - a)da d\gamma$$

$$= \int_{-\infty}^{\infty} \operatorname{Cos} \gamma x. \quad \phi_{m+n}(a)da = \psi_{m+n}(x).$$

Hence $\psi_m(x) = [\chi(x)]^m$, where $\chi(x)$ is independent of m. Inverting the equation

$$[\chi(x)]^m = \int_{-\infty}^{\infty} \operatorname{Cos} ax. \quad \phi_m(a)da$$

[1] V. Volterra, *The Theory of Permutable Functions*, Vanuxem Lectures, Princeton (1912), published in 1915; *Rend. Lincei*, April 17, 1910. See also H. Bateman, *Cambr. Phil. Trans.* 1906.

[2] *Phil. Trans.* 1816; *Memoirs of the Analytical Society*, 1813; *Cambr. Phil. Trans.* 1820.

[3] *Liouville's Journal* and *Journal de l'École Polytechnique*, 1832–7; *Crelle's Journal*, 1834, 11, 1; 1834, 12, 273; 1835, 13, 219.

[4] *Camb. Math. Journ.* 1845, 4, 82.

[5] *Werke* (Weber, 1876), 331.

[6] *Electrical Papers*, 2.

[7] *Camb. and Dubl. Math. Journ.* 1852, 7, 103.

by Fourier's theorem we obtain

$$\phi_m(a) = \frac{1}{\pi}\int_0^\infty \operatorname{Cos} ax\,[\chi(x)]^m dx,$$

where $\chi(x)$ is an arbitrary function subject to certain conditions.

Herschel[1] has shown that a number of very general types of functional equations can be reduced to *difference* equations by means of an ingenious artifice, and it seems likely that this method can be extended so as to give a reduction to equations involving distributive operations of other types. A few of Herschel's examples will now be given to illustrate his method.

(1) If P and Q are given functions of x, the functional equation

$$\psi[x, P(x)] = \psi[x, Q(x)]$$

is satisfied by

$$\psi(x,y) = f(x) + [y - P(x)][y - Q(x)]\chi(x,y),$$

where $f(x)$ and $\chi(x,y)$ are arbitrary functions.

(2) To solve the equation $\psi(x, x) - \psi(x, 0) = a$, where a is a constant.

Assume $$\psi(x,y) = \phi(x, h + y/x),$$

then the equation becomes

$$\phi(x, h+1) - \phi(x, h) = a,$$

the solution of which is $\phi(x, h) = ah + F(x, h)$, where F is a periodic function of h of period 1. Substituting for ϕ and putting $h = 0$ we get

$$\psi(x,y) = a\frac{y}{x} + F\left(x, \frac{y}{x}\right),$$

where $F(x, 1) = F(x, 0)$. According to (1) the solution of this is

$$F(x,z) = f(x) + z(z-1)\chi(x,z).$$

Hence, finally, we get

$$\psi(x,y) = a\frac{y}{x} + f(x) + y(y-x)G(x,y),$$

where f and G are arbitrary functions.

(3) Solve the equation $F\{x, \psi(x,P), \psi(x,Q)\} = 0$.

Let $\theta(x,y)$ be a function such that $\theta(x,P) = 0$, $\theta(x,Q) = 1$; these conditions may be satisfied by putting

$$\theta(x,y) = \frac{y-P}{Q-P} + (y-P)(y-Q)\,\chi(x,y).$$

Now write $\psi(x,y) = \phi[x, h + \theta(x,y)]$, then the functional equation is replaced by the difference equation

$$F\{x, \phi(x,h), \phi(x, h+1)\} = 0.$$

For further information on the calculus of functions we may refer to Herschel's *Calculus of Finite Differences* (Cambridge, 1820); Boole's *Finite Differences* (1860); D. F. Gregory's *Mathematical Writings* (1865); G. Oltramare, *Calcul de généralisation* (Paris, 1899); Abel, *Crelle's Journal*, 1826, **1**, 11; 1827, **2**; A. R. Schweitzer, *Bulletin of the American Mathematical Society*, 1912, **18**, 299; 1912, **19**, 66; 1914, **21**, 23.

The theory of distributive operations is too extensive to summarise here.

[1] *Cambr. Phil. Trans.* 1820.

Reference may perhaps be made to the work of Pincherle, Volterra, Fréchet, Moore, and other writers. The analogies with systems of linear equations and geometry in a space of n dimensions are useful. The method of successive substitution is frequently used. Symbolically, if

$$f = \phi - \lambda D\phi,$$

where D is a distributive operation, then the series

$$\phi = f + \lambda Df + \lambda^2 D^2 f + \lambda^3 D^3 f + \ldots .$$

frequently converges and gives the solution. The inequality of Poisson and Schwarz (Poisson, *Connaissance des Tems*, 1827, gives a special case):

$$\int_a^b [f(x)]^2 dx \,.\, \int_a^b [g(x)]^2 dx \geq [\int_a^b f(x)g(x)dx]^2$$

is often useful and has been extended by Moore in various ways. The solutions of the homogeneous equation $\phi - \lambda D\phi = 0$ are of considerable interest.

Humanistic Culture through the Study of Science (G. N. Pingriff, B.A., B.Sc., Chemistry Master, University College School, London, N.W.)

So much has been written of late about the two main educational advantages of the study of science that other almost equally important advantages are apt to be overlooked. The two main advantages are, I take it, its direct utilitarian value and its mental training value. The one which it is hoped to emphasise here has to do rather with its moral and æsthetic value.

Culture, a much-maligned word in these days, is an idea of Greek origin. The word may be taken to refer to a certain attitude of mind rather than any actual possession, a sympathy with and susceptibility to things intellectual and æsthetic. This being so, to be cultured our interest must be not only in things of the past, but also in those of the present. Both the classical grammarian or archæologist and the technical chemist may be equally beyond the pale. Humanism, on the other hand, is a word generally associated with purely literary studies, and chiefly with ancient literature. But this is not as it should be. The word dates, I believe, from the time of the Reformation. After the spiritual gloom and general ignorance of the dark ages the new study of the past was found to have so fine an effect on the mind that it was said to "humanise." But why should not a study of the past in the form of the history of science also be capable of some humanising influence? We are told by Pope that "the proper study of mankind is man." Why should we not answer now that the proper study of mankind is nature and the laws of nature, including man and his own petty laws, as one division of the greater scheme?

Our other reason then for teaching science is that the study of science is, in itself, a beautiful, satisfying, and uplifting activity. It can bring joy to its follower as great as that which the scholar finds in noble literature or the artist in a beautiful scene. It can give equal culture. But for this to be so we must be careful in teaching science, especially in schools, not to divorce our science from human interest. Every boy does not feel a keen delight in the study of inanimate nature, though many do and most can if the subject-matter be properly presented and especially if the added human interest be occasionally invoked. Very few boys probably fail to experience a certain measure of joy on carrying out successfully such a simple operation as the crystallisation of blue-vitriol or saltpetre, and

all can share the delight of the old workers in their discoveries as related in their own words. By so presenting the subject we may also be incidentally imparting valuable moral lessons. Remember the steadfastness of purpose and noble lives of so many of the great men who have accumulated our modern stock of scientific knowledge, from the time of Archimedes to the present day. What could form a better antidote to the commercial spirit of our times than the answer of Pasteur when Napoleon III. expressed surprise that the investigator did not try to make his researches a source of personal profit? "In science," Pasteur replied, "men of science would consider that they lowered themselves by so doing;" or the reply of the naturalist Louis Agassiz, on a similar occasion, "I have no time to make money." Or where shall we find a better type of courage than that of the physician (Dr. Klein) who swallowed a spoonful of a pure culture of cholera bacillus in order to put to test the theoretical results of the bacteriologist? Or consider that eminently Christian virtue, humility. The stories of the lives of many of our greatest and most painstaking scientists do much to teach the imperfections and almost insignificance of man. Kelvin told us that the one word which most characterised his life-work was "failure." Newton, shortly before his death, said, "I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the sea-shore, and devoting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me." Yet how much do some of those pebbles and shells mean to civilisation to-day. The beauty and joy which science has brought to its followers may be pointed out to and experienced by the student. The moral lessons are best left to be gradually absorbed, perhaps in after years. By these paths also the student will be led from a consideration of the perseverance and noble effort of the great pioneers to a true appreciation and respect for science itself—for those great secrets wrested from Nature, which now form perhaps the greatest heritage of man. Certainly there need be no lack of human interest in the study of science, and no scarcity of valuable lessons to be drawn, if we are prepared to use to the full the records of the past.

The humanistic culture of the Greeks and Romans had many noble ideals in view, but it is easy to see its limitations. The Roman had no sympathy with the weak; he despised the spirit of toleration; scenery made little appeal to him, and of science he had none to admire. The humanistic culture of science is of all time. Its ideals rank with the noblest of those of Greece or of Christendom. It is concerned in the search for the increase of knowledge and man's never-ending quest of truth.

G. N PINGRIFF.

REVIEWS

LOGIC

Collected Logical Works. Vol. II. The Laws of Thought. By GEORGE BOOLE. [Pp. xvi + 448.] (London: The Open Court Publishing Company, 1916. Price 15*s*. net.)

THIS volume is a reprint of *Boole's Laws of Thought*, originally published in 1854. It is an extension to logical reasoning of the symbolic processes employed in mathematics.

The earlier chapters deal with Primary propositions which express relations between things, as distinguished from Secondary propositions which express a relation between propositions. In primary propositions the terms are symbolised by letters. Thus if x denote the class "white things" and y the class "sheep," xy is said to denote "white sheep." Since "white sheep" and "sheep that are white" mean the same thing, $xy = yx$. Hence the logical like the algebraical symbols are commutative. The signs + and − being taken to denote "along with" and "except" respectively, it is easily established that $a(x + y) = ax + ay$, and that a term may be removed from one side of an equation to the other by changing its sign. Also each side of an equation may be multiplied by the same symbol. If all metals are elements, then obviously all white metals are white elements. Although, however, certain members of one class which have a particular quality may be identical with certain members of another class which have the same quality, we cannot infer that the whole of the former class is identical with the whole of the latter. There is, therefore, in logic no process analogous to the algebraic division of both sides of an equation by the same symbol.

A contrast still more striking becomes apparent when we try to find if there is anything in logic corresponding to the algebraic x^2. If to the quality "white" we apply the same quality we get "white white," which expresses no more than "white." Hence in logic xx or $x^2 = x$. This is a fundamental law, and is called the Law of Duality. Now we know from algebra that the only quantities which satisfy the equation $x^2 = x$ are 0 and 1. It follows that the logical symbols cannot be regarded as quantities other than 0 and 1. In fact, throughout the treatment of the subject we find that no numbers occur except 0 and 1, and it is shown that, while 0 has its usual signification, 1 must represent the universe, or rather that part of the universe which is under discussion. If then x denote "men" $1 - x$ will denote all that are "not-men," and since "men" who are also "not-men" do not exist, it follows that $x(1 - x) = 0$. Hence the Law of Duality is really the symbolic expression of the Law of Thought that a thing cannot both be and not be.

We are now in a position to express all the divisions of things with reference to their possessing or not possessing certain qualities. Let x = white, y = sheep, then

$$xy = \text{white sheep},$$
$$x(1 - y) = \text{all white things which are not sheep},$$
$$(1 - x)y = \text{all sheep that are not white},$$
$$(1 - x)(1 - y) = \text{all things that are neither white nor sheep}.$$

If we had these expressions together we find that they amount to 1 or the universe.

The expansion of a logical function is interesting and important.

Let $f(x)$ be the given function.

Assume $f(x) = ax + b(1 - x)$, where a and b are to be determined.

Since x may be regarded as a quantity susceptible only of the values 0 and 1, give it those values successively.

$$\text{Then } f(0) = b, f(1) = a,$$
$$\therefore f(x) = f(1)x + f(0)(1 - x).$$

$f(xy)$ can be developed in a similar manner by expanding it first as a function of y and then developing the expanded function as a function of x. The result is:

$$f(xy) = f(1, 1)xy + f(1, 0)x(1 - y) + f(0, 1)(1 - x)y + f(0, 0)(1 - x)(1 - y).$$

The following is a simple illustration of Boole's method:

Taking the biblical definition of clean beasts as those which chew the cud and divide the hoof, find a description of unclean beasts.

$$\begin{aligned} \text{Let } x &= \text{clean beasts,} \\ y &= \text{chewing the cud,} \\ z &= \text{dividing the hoof,} \\ \text{then } x &= yz, \\ 1 - x &= 1 - yz. \end{aligned}$$

Expanding the right-hand side by the formula we get—

$$1 - x = y(1 - z) + (1 - y)z + (1 - y)(1 - z).$$

Unclean beasts, therefore, are those which chew the cud and do not divide the hoof, those which divide the hoof and do not chew the cud, and those which neither chew the cud nor divide the hoof.

In translating a proposition into an equation it will naturally occur to the logician to inquire what becomes of the Quantification of the Predicate. If x = men, y = mortal beings, we cannot express that all men are mortal by $x = y$, because all mortal beings are not men. Boole meets the difficulty by introducing a symbol to denote an indefinite class. He expresses the proposition "all men are mortal" by $x = vy$, where v represents a class about which we know nothing except that some of its members are y.

The indefinite symbol v is easily eliminated, for in logic we can eliminate any class symbol from a single equation with the help of the Law of Duality. The result of eliminating x from the equation $f(x) = 0$ is

$$f(0)f(1) = 0.$$

To prove this expand $f(x) = 0$.

We have $f(1)x + f(0)(1 - x) = 0$.

$$\therefore x = \frac{f(0)}{f(0) - f(1)}$$
$$1 - x = \frac{-f(1)}{f(0) - f(1)}.$$

Substituting in $x(1 - x) = 0$, we get

$$f(0)f(1) = 0.$$

Suppose now it is required to eliminate v from the equation $x = vy$ and to interpret the result.

$$\text{Let } x - vy = f(v),$$
$$\text{then } f(0) = x, f(1) = x - y.$$

∴ by the rule just proved

$$x(x-y)=0$$
$$x^2-xy=0$$
$$x-xy=0$$
$$x(1-y)=0$$

If x denote "men" and y "mortal beings," the interpretation of our result is—

"Men that are not mortal do not exist."

In the space available for this review it is impossible to give more than a glimpse of the method and scope of the work. Boole proceeds to extend his method to Secondary propositions, where the symbols are taken to represent propositions. He further applies it to an examination of the theological arguments of Dr. Samuel Clarke and Spinoza, and finally to the theory of Probabilities. The subject is one of absorbing interest, and well merits the attention of the student. There are no difficulties which need deter him. Here, as in his mathematical treatises, Boole is at great pains to make his meaning clear, even at the cost of a certain amount of repetition. I must add that the present reprint is in every way worthy of the work and its author. The type and paper are excellent, and, notwithstanding the intricacy of some of the mathematical processes, I have not discovered a single *erratum*.

W. H. WINTER.

MATHEMATICS

Historical Introduction to Mathematical Literature. By G. A. MILLER, Professor of Mathematics in the University of Illinois. [Pp. xiv + 302.] (New York: The Macmillan Company; London: Macmillan & Co., Ltd., 1916. Price 7*s*. net.)

THE author of this book states in the preface that his main object is to guide the reader to "points from which he can overlook domains of considerable extent in order that he may be able to form a somewhat independent judgment as regards the regions which he might like to examine more closely" (p. v). The first chapter consists of general observations on the changes brought about in the nineteenth century in the teaching of mathematics, mathematical production, and the place assigned to the history of mathematics; also the author deals with periods in the history of mathematics and the growth of mathematical journals, and gives a sketch of mathematics in America from about 1870 onwards. The second chapter deals with mathematical societies, congresses, and tables, and also with periodicals, encyclopædias, and other works of reference; a list of books of the kind last named being given in the Appendix. The third chapter deals briefly with the nature and dominant concepts of mathematics, mathematical notation, and mathematics as an educational subject. The fourth, fifth, and sixth chapters deal with fundamental developments in arithmetic, geometry, and algebra respectively. The seventh chapter contains short notices on the life and work of twenty-five prominent deceased mathematicians.

The book will be found far more useful as a book of hints to a teacher who knows how to select and construct than to a student. There are many minute and valuable details, such as a notice of errors in some collected works (p. 77), the *Encyclopædia Britannica* (p. 281), and expositions of the theory of groups (pp. 97 109, 114, 263), which will be of great service to a teacher or a writer; but the

sketches of mathematical progress are hardly full enough to make them valuable for a learner, while such remarks as those on the honours and personal appearance of Poincaré (p. 272) can hardly be thought relevant or inspiring for a history which should presumably be a history of a branch of thought.

When dealing with the definitions that have been proposed for mathematics (pp. 78–81), there is the usual failure to distinguish between attempts, such as that of Russell, to define the logical nature of the subject-matter of mathematics and attempts to describe the processes of thought used in mathematics. The misunderstanding of Russell's definition, which was meant to describe humorously a profound truth about implication and variables, is rather amusing: it may, I think, be traced back to another American, Prof. J. W. Young. There are other statements which seem to be open to criticism. On p. 92 we read: "In our own day the theory of aggregates tends more and more to cease as a distinct subject"; on p. 96 John Bernoulli is said to have been the first to use the word *function* "with its precise actual meaning"; on p. 186 the fundamental theorem of algebra is said to have been first proved by d'Alembert, "which [proof] was, however, not entirely correct." D'Alembert, as a matter of fact, entirely missed the point of an existence-theorem. On p. 207 the work of Boole on the early theory of invariants might have been referred to.

The reference to Hamilton's work on the groups of the regular polyhedra (pp. 173–4) is very useful, and so is the list of references on the history of mathematics in the nineteenth century (pp. 7–8). The remark about economy of thought in mathematics (bottom of p. vii) is profound and true. Finally, surely there must be some mistake in the Preface (p. ix)—written in October 1915—that "with our gradual evolution from the state of barbarism the history of war and bloodshed is being slowly replaced by that of political and intellectual movements."

PHILIP E. B. JOURDAIN.

(1) **Guida allo Studio della Storia delle Matematiche.** By GINO LORIA, Professor at the University of Genoa. [Pp. xvi + 228.] (Milano: Ulrico Hoepli, 1916. Price 3 lire.)

(2) **Biblioteca Scientifico-Politecnica Internazionale:** Bibliografia delle più importanti opere italiane e straniere sulle scienze esatte, dell'ingegnere, le arti belle e le arte utili, con speciale riguardo alla lore applicazione pratica industriale, pubblicate dal 1905 al 1915, disposte in ordine alfabetico delle materie (con indice alfabetico degli autori). Proemio del Senatore GIUSEPPE COLOMBO. By ULRICO HOEPLI. [Pp. xxiv + 468.] (Milano: Ulrico Hoepli, 1916. Price 3 lire.)

(1) THIS seems to me quite one of the most useful for its purpose of the admirably produced "Manuali Hoepli." Prof. Loria's idea, which was first published in 1908, that it would be a good thing to compile a manual for the use of those who wish to make a study of the history of mathematics is here actualised in a satisfactory manner. The first Part contains: (1) Generalities and references to works on the historical method, the early history of philosophy and the arts, and literary history; (2) a critical list of the principal works on the history of mathematics; (3) a summary of the contributions made in periodical literature. The second Part contains details about manuscripts, biographies, bibliographies, and collected works. A separate index of names for each Part is provided.

We may shortly indicate some omissions. In Part I. the histories of Burnet, Fink and Gow, and Brunschvicg's *Étapes de la philosophie mathématique* might

have been mentioned, and a full list given of the historical reports of Meyer, Hilbert, Schoenflies, Burkhardt, and others (cf. p. 65). In the second Part we miss references to the *Portsmouth Catalogue*, Rigaud's *Historical Essay*, and Rouse Ball's *Essay* (on some of Newton's early manuscripts), Mrs. De Morgan's *Memoir* of her husband, Babbage's *Passages from the Life of a Philosopher*, and Campbell and Garnett's *Life* of Maxwell.

In the next edition we hope that the usefulness of Prof. Loria's book will be increased by the addition of a detailed analysis of those works, such as magazines, which deal with many questions of science or history. What most of us want to know is where *exactly* to find reliable information about a particular thing, theory, or person.

(2) The idea of an international catalogue of recent books is a good one; but this catalogue is by no means complete, especially as regards English books, and the prices are always given in Italian money, so that it is of not so much use to, say, an English buyer in England as it might be. Still, it will be found useful, especially to those who are interested in the technical arts.

PHILIP E. B. JOURDAIN.

The Integration of Functions of a Single Variable. By G. H. HARDY, M.A., F.R.S., Fellow and Lecturer of Trinity College, and Cayley Lecturer in Mathematics in the University of Cambridge. No. 2 of the Cambridge Tracts in Mathematics and Mathematical Physics. [Second Edition. Pp. viii + 67.] (Cambridge: University Press, 1916. Price 2*s*. 6*d*. net.)

IN the first edition (1905) of this useful tract the subject of indefinite integration of what the author called and calls "elementary functions" was given in a very thorough and systematic form. An "elementary function" is a member of the class of functions which comprises rational functions, algebraic functions—explicit or implicit, the exponential function, the logarithmic function, and all functions which can be defined by means of any finite combination of the symbols proper to the preceding four classes of functions. The integration of all such functions was treated with particular reference to the work of Abel, Liouville, and Tschebyschef; and an admirable Bibliography of the papers by these and other mathematicians was given.

The new edition differs from the first only in one very important point. Mr. J. E. Littlewood discovered that a proof of Abel's given in the first edition was invalid; a new proof due to Mr. H. T. J. Norton is given in this edition, and a few sections on pp. 36–41 (cf. pp. 66–67) are consequently completely rewritten. There are many other minor alterations in this edition, and the Bibliography has been expanded.

This is probably the most useful Tract in the whole series for one who means to study mathematics systematically.

PHILIP E. B. JOURDAIN.

The Elements of Non-Euclidean Plane Geometry and Trigonometry. By H. S. CARSLAW, Sc.D., D.Sc., Professor of Mathematics in the University of Sydney. [Pp. xii + 179, with 116 figures.] (London: Longmans Green & Co., 1916. Price 5*s*. net.)

THIS little book will be welcomed by all who read it; and those especially who have to spend much time in teaching elementary geometry will be refreshed to

find such a very different branch of their subject brought within easy reach. With charming clearness and brevity, Prof. Carslaw sets forth the history and outlines of non-Euclidean geometry, for readers who possess only an elementary knowledge of mathematics. The book is well printed, but some of the figures should have been repeated overleaf.

The first two chapters are historical: we could, perhaps, have spared even such passing insistence on the human weaknesses of the great inventors. Then the different parallel postulates are explained, and it is shown that on each of them there is built up a different geometry, consistent with itself but contradicting the others. There are three chapters on hyperbolic geometry, trigonometry, and infinitesimal calculus; these are in turn carried just far enough for the newcomer to begin to feel at home among the ideas and notations, and to see that there is a great deal more to be learnt. Much use is made of Saccheri's symmetrical quadrilateral, with two right angles and two acute angles; and Bolayai's classical construction, for a parallel to a given line through a given point, is the most prominent proposition. No use is made of the principle of continuity, nor of solid geometry. Elliptic geometry, with its two varieties, is treated rather more shortly in the next two chapters: the analogy with spherical trigonometry is clearly brought out, so that the ideas are more familiar than in the section before. The last chapter is devoted to philosophy; it establishes the truth of non-Euclidean geometry by interpreting its postulates in terms of a family of circles and spheres in Euclidean space, so showing that any contradiction in either system must be accompanied by a corresponding contradiction in the other. "One geometry cannot be more true than another; it can only be more convenient" is one of the apt quotations with which the volume closes.

H. P. H.

Exercices et Leçons de Mécanique Analytique. Par R. DE MONTESSUS, Professeur à la Faculté libre des Sciences de Lille. [Pp. vi + 334.] (Paris: Gauthier-Villars et Cie., 1915. Price 12 francs.)

THIS book consists of problems, arranged in order of difficulty and solved by analytical methods, and supplements those courses of lectures on mechanics in which, owing to the place which must be given to certain fundamental questions such as the pendulum and the gyroscope, little attention has been paid to educationally valuable problems. There are also numerous exercises. The first part deals with the calculation of centres of gravity of curves and surfaces, and attractions, and then treats of potential and theorems on it. The second part deals with moments of inertia, the principle of virtual work, Lagrange's equations, the motion of a rigid body, stability, small oscillations of a system about a position of stable equilibrium, and impact. A long note, of more than sixty pages, on elliptic integrals in the real domain terminates the book. Elliptic functions have many applications in mechanics, and, as the author says, there is great difficulty in applying the theory as usually given in lectures on the theory of functions.

There does not seem to be anything strikingly novel in this useful collection of problems. I think that a clear discussion of the modifications introduced into Lagrange's equations by cases of rolling with friction (non-holonomy) would impress on the student's mind the nature of the tacit assumptions made on p. 117.

PHILIP E. B. JOURDAIN.

Four Lectures on Mathematics delivered at Columbia University in 1911. By J. HADAMARD, Member of the Institute, Professor in the Collège de France and in the École Polytechnique, Lecturer in Mathematics and Mathematical Physics in Columbia University for 1911. [Pp. vi + 52.] (New York: Columbia University Press, 1915. Price 75 cents.)

THE titles of these four lectures are: (1) "The Definition of Solutions of Linear Partial Differential Equations by Boundary Conditions"; (2) "Contemporary Researches in Differential Equations, Integral Equations, and Integro-Differential Equations"; (3) "Analysis Situs in Connection with Correspondences and Differential Equations"; and (4) "Elementary Solutions of Partial Differential Equations and Green's Functions."

In the first lecture M. Hadamard treats of linear partial differential equations of the second order, and considers only the results which correspond to non-analytic data chiefly because there is a remarkable accordance between them and the results to which physical applications brings us. The second and third lectures are precisely described in their titles. In the fourth lecture the "elementary solutions" referred to are a necessary base of the treatment of every linear partial differential equation, such as those which arise in physical problems; and Green's functions are related to all the chief topics discussed in these lectures. There are very interesting references to method and the mutual relations of geometry, analysis, and physics; and, as we should expect, the subjects are treated in a thoroughly masterly fashion.

PHILIP E. B. JOURDAIN.

STATISTICS

The Elements of Finite Differences. Also Solutions to Questions set for Part I. of the Examinations of the Institute of Actuaries. Second Edition. By J. BURN, F.I.A., and E. H. BROWN, F.I.A. [Pp. iii + 289.] (London: Charles & Edwin Layton, 1915. Price 10*s*. 6*d*. net.)

The Mathematical Theory of Probabilities and its Application to Frequency Curves and Statistical Methods. By ARNE FISHER, F.S.S. Translated and Edited from the Author's Original Danish Notes with the Assistance of WILLIAM PONYNGE, B.A. With an Introductory Note by F. W. FRANKLAND, F.I.A., F.A.S., F.S.S. Volume I. Mathematical Probabilities and Homograde Statistics. [Pp. xx + 171.] (New York: The Macmillan Company, 1915. Price 8*s*. 6*d*. net.)

The Construction of Mortality and Sickness Tables. A Primer by W. PALIN ELDERTON and RICHARD C. FIPPARD. [Pp. vi + 120.] (London: Adam & Charles Black, 1914. Price .)

Vital Statistics Explained. Some Practical Suggestions by JOSEPH BURN, F.I.A., F.S.I. [Pp. x + 140.] (London: Constable & Co. Price 4*s*. net.)

THE study of statistics has become so important for almost every science that t is high time for men of science to be in possession of a single text-book on the subject which will enable any well-educated person to apply statistical methods to his particular branch of knowledge. Unfortunately at present we have to do as best we can with a number of small primers dealing only with special facets, eked out by articles in *Biometrika* and other journals, none of which supply the need.

The second edition of the well-known book by Messrs. Burn and Brown will

continue to be useful to students of actuarial work; but the mere fact that such a book has been called for suggests that there is something wrong in our teaching of mathematics. If the whole subject of series were taken together, as it ought to be, finite differences would naturally precede the study of the infinitesimal calculus. As it is, series are generally dealt with by inadequate and ungeneralised algebraic methods, and the student is then plunged headlong into the infinitesimal calculus—so that the whole of the finite calculus, so useful in every branch of science, is apt to be passed entirely. This is the result of a scheme of teaching which is academical rather than practical. The book itself may be briefly defined as being very largely Boole with numerous examples taken from actuarial examination papers, together with very full solutions of many of the problems—all useful and instructive work for students of many domains of science. The principal additions in the second edition consist of a chapter on functions of two variables and one on Stirling's formula of interpolation. Symbolic methods are introduced early, and the writing is generally clear and easy.

The excellent translation of Mr. Arne Fisher's book lies on the opposite pole of the theoretical and even the metaphysical side of the subject, and has the advantage of giving much continental work which is not well known in England—especially Scandinavian and Russian work. After a lucid philosophical introduction and a short historical chapter, the author proceeds with his theme in a manner which will be particularly helpful to those who have not read in this line. Laplace's work is taken as the basis and the treatment adopted gives new points of view. The examples are well put, and the book continues with Bayes' Rule, Tchebycheff's Theorem, the Theory of Dispersion, and Homograde Statistical Series, etc., a knowledge of both the finite and the infinitesimal calculus being rightly demanded. The second volume will be looked forward to, and should provide something like the required text-book. We can find no mention of Todhunter's admirable history.

The primer of Elderton and Fippard attempts to explain "each step verbally with the help of arithmetical examples, in order to avoid the introduction of algebraical formulas." It appears to us that one might as well try to teach rifle shooting without live cartridges, or swimming without water. Surely every one should be well enough educated nowadays to understand "algebraical formulæ," and the absence of them seems to us merely to add to the difficulties. But as far as it goes the book is very well done.

Mr. Burn's new work is prepared on a similar basis, but contains more details helpful, say, to Medical Officers of Health—who are often notoriously non-mathematical. We doubt the wisdom of writing down to ignorance—a thing which is too frequently done. If a person does not possess enough energy to learn a little mathematics, he will not possess enough to deal profitably with statistics, and had better be left alone, without any attempts at spoon-feeding. The work contains, however, some useful information apart from any special study of vital statistics.

The Declining Birth-Rate: its Causes and Effects. Being the Report of and the chief Evidence taken by the National Birth-Rate Commission, instituted with official recognition by the National Council of Public Morals, for the Promotion of Race Regeneration—Spiritual, Moral, and Physical. [Pp. xiv + 450.] (London: Chapman & Hall, 1916. Price 10s. 6d. net.)

THE seriousness of the population question in this country must have been impressed upon all thinking people for some time past by the almost monotonous

fall in the birth-rate recorded in the returns of the Registrar-General. More than ever is it brought to the fore when one considers the effect of the war on the manhood of the country. The flower of the younger men, physically and in many other ways as citizens, is being destroyed, and even though their noble sacrifice will not be forgotten nor in vain, yet as citizens and potential fathers they are lost to the nation. This report is especially interesting as it is an attempt on a fairly large scale to deal with the problem, and it is worthy of all commendation. It involved a considerable amount of time and labour on the part of all concerned and, in the case of some of the witnesses examined, a very thorough and thoughtful preparation of their evidence.

Out of the welter of facts, theories, and opinions brought before this committee, which in themselves indicate the enormous complexity of the whole question, a few points stand out quite clearly. There is beyond all doubt a large and significant fall in the birth-rate, and again, as Miss Elderton and other workers in Prof. Karl Pearson's laboratories have shown, the decline is to a large extent selective. It is among classes that are likely to contain the best citizens that the fall is most marked, while among classes with a fair proportion of undesirables it is as high as ever. This may or may not be a good thing, but most people will undoubtedly regard it as distinctly bad. There is, indeed, a difference of opinion as to whether a large birth-rate is or is not desirable, but apparently none with regard to its present selective action. The causes of this decline are not so obvious; it may be that there is a certain reduction of the fertility of the population in general, or perhaps in the educated classes, or as seems more likely from the opinions expressed by the witnesses, it is due to the conscious restriction of the number of children in the family. All parties are in unison in condemning the practice of destroying the product of conception by causing an abortion, a practice that unfortunately appears to be prevalent in some of the northern midland districts. Concerning the desirability of limiting the family by preventing conception in some way or other serious differences are found, but the evidence on this point is of a most nebulous character, and to practically every answer of the witnesses concerning it the words of one of the questioners apply with ever-increasing force: "I gather this is your opinion, but are there any methods of getting facts?" This is probably one of the most striking features of the report, the appalling general ignorance concerning many of the points involved, even though of an important character. Save on the statistical side, where of course there is always a difficulty of obtaining the facts or of properly interpreting the results, there is a strange absence of facts, and as a consequence a number of different opinions. If this report does no more than call attention to this lack of knowledge and direct the attention of the right people to filling in the blanks it will have served a useful purpose.

Almost all witnesses agree that it is economic pressure that leads to the restriction of the family, and consequently many of the suggested remedies proposed in the addition are in the direction of lightening the burden to be borne by parents of families. The proposal to tax bachelors is hardly to be endorsed, since if such a tax is to be levied it should fall on all childless people of both sexes, married or unmarried. In other words, precisely the same result would be achieved by a rebatement of taxes for each child under a certain age such as is done at present, but of far larger amounts. Another proposal that would meet with approval is that the expenditure on the education of the children should be freed from tax as is now done with the payment of insurance premiums.

C. H. O'D.

PHYSICS

A Manual of Practical Physics. By H. E. HADLEY, B.Sc. [Pp. viii + 265, with 153 illustrations and diagrams.] (London: Macmillan & Co., 1916. Price 3s.)

THIS is just a collection of some 160 recipes for performing experiments in Physics up to intermediate pass standard. Very excellent recipes they are too, the author's reputation as a writer of elementary text-books is a sufficient guarantee of that, and the book can be strongly recommended to the notice of school teachers who have large classes, so that several different experiments have to be supervised at the same time. Such ample instruction is given both as to the manipulation and mode of entering results in the laboratory book that students of ordinary ability should not require any further assistance in these respects. It will be necessary, however, for the teacher to explain the why and wherefore of the many minute experimental details which the author gives so liberally, but too often without any explanations being even hinted at. That is the disadvantage of the recipe method, for the student at this stage cannot discover reasons for himself in a scientific fashion, and, in the absence of proper guidance, will either guess, which is unscientific, or carry out his instructions mechanically, which subverts the chief purpose of his study. In the hands of a skilful teacher these disadvantages will not arise, and the method is safe enough.

All branches of Physics are dealt with, and a brief treatment of preliminary measurements makes the book suitable for those who are commencing their study of the subject. The author has exercised a wise discretion in his choice of experiments; he has, for example, refrained from including any of the usual and misleading experiments on the magnetic force due to isolated magnetic poles. Experience shows that, once the student is permitted to apply the inverse square law to practical measurements with long magnets, he is very apt to apply it always. On the other hand, simple quantitative measurements can be made so easily in electrostatics that one or two should have been included. The use of cotton wool as a lagging for calorimeters is again advocated in this book. This is much to be regretted, for not only does it introduce quite unknown errors, but it almost always gets wet, and then the last state is more than ever worse than the first.

D. ORSON WOOD.

CHEMISTRY

A System of Physical Chemistry. By PROF. WILLIAM C. MCC. LEWIS, M.A. (R.U.I.), D.Sc. (Liv.). In two volumes. Vol. I. [Pp. xi + 523, with diagrams.] Vol. II. [Pp. vii + 552, with 98 diagrams.] Text Books of Physical Chemistry. Edited by SIR WILLIAM RAMSAY, K.C.B., F.R.S. (London: Longmans, Green & Co., 1916. Price 9s. each net.)

THIS, the most recent addition to the Ramsay series of Physical Text-Books, is at the same time the largest and most comprehensive number yet published in this admirable series. Unlike its well-known predecessors, its scope is not limited to any one definite branch of physical chemistry, but embraces the whole subject so far as advancement has been made in this domain of science. To all acquainted with the enormous strides which physical science and its inter-relations with chemistry have made in latter years, it will be judged no mean feat of the author to have dealt so adequately and tersely with all its branches, and to have incorporated the large mass of new work within the confines of these two new volumes.

The work opens with a tabular tree indicating the author's concept of matter associated with energy from the standpoint of equilibrium, the main divisions being into systems in equilibrium and systems not in equilibrium. The text is conveniently divided into three parts. Part I. deals with considerations based on the kinetic theory, Part II. comprises considerations based on thermodynamics, whilst the last part treats of considerations founded on thermodynamics and statistical mechanics. Most of the newer work, such as Planck's quantum theory, Einstein's extension of this, photo-reactions, low temperature, specific heats, etc., is to be found in the final part, and not the least value of these volumes to English readers lies in the fact that they contain the first simultaneous presentation in their own language of these new theories and their mutual bearing on one another.

The whole work is redolent of the spirit of inquiry, and at every point opposing views are clearly stated and the evidence for each critically sifted. In this respect the volume differs considerably from former text-books on physical chemistry, and the feeling is aroused that the whole subject of physico-chemical science is being passed in review by a mind determined to probe the truth.

Text-books which are merely treasuries of facts and theories have at all times a specific value, but when fact and theory are carefully weighed up together and their bearing on one another evaluated we pass to the designation Classic. Most readers will find it easy to place the present work in the latter category,

C. S. G.

GEOLOGY

The Deposits of the Useful Minerals and Rocks. Their Origin, Form and Content. Vol. II. By F. BEYSCHLAG, J. H. L. VOGT, and P. KRUSCH. Translated by S. J. TRUSCOTT, A.R.S.M. [Pp. xxi + 515–1262, with 176 illustrations.] (London: Macmillan & Co., Ltd., 1916. Price 20s. net.)

THE second volume of this work more than fulfils the promise of the first. The translator has done his work excellently, and British and American students of ore-deposits will be grateful to him for his arduous labours on their behalf. He has evidently taken into account the various criticisms offered in reviews of the first volume, as is indeed acknowledged in the preface. The style, for example, is much improved, at least for the present reviewer, by the freer translation of the original German, with the result that there are very few of the stiff and stodgy sentences so common in the first volume. We have very little criticism to offer as to the manner of the second volume. "Nordengland" (p. 775), "federerz" and "zundererz" (p. 779), and a few other German terms might have been given their English equivalents. "Shale" would be a better term than "slate" in the description of the Cleveland ironstone (p. 1023). "Trachyte-dolerite" (p. 564) is a term strange to petrography; and "Quartenary" (p. 1002) is an obvious misprint.

In his preface the translator proposes several new renderings of original German terms. "Gangtonschiefer" is now translated as "lode-slate," instead of "flucan," as in the first volume; whilst the last-named term is taken to be the equivalent of "Lettenkluft," and is thus used as the name of an occurrence rather than a material. Similarly "Sahlband" is now translated as "lode-wall" instead of "gouge."

The geographical and subject indexes provided at the end of the second volume cover the whole of the work, but the index published in Volume I. is not incorporated in its entirety. Hence some references to subjects and places in the

first volume are not to be found in the full index. There seems also to have been differences in the methods of compiling the indexes to the two volumes. The detail of the index to the second volume is less full than in Volume I., as may be seen by comparing the references to, say, Australia or Canada, in the two volumes. The bibliographies are occasionally incomplete. To take a single example, that of the West Australian "young" gold-silver lodes only includes memoirs up to 1903. In some cases, however, references appear to have been added from 1909, the date of publication of the original German work.

Coming now to the matter of the second volume, one of the most valuable characters of the work, as we noticed in the first volume, is the comprehensive description of the, to English-speaking investigators, little-known but important European ore-occurrences, many of which are illustrated by excellent geological maps. The first volume ended at an early stage in "Lodes and Metasomatic Deposits," having completed the description of the tin, apatite, and quicksilver lodes. The second volume thus begins with the "young" gold-silver lodes, and goes on to describe the "old" gold lodes, and other important groups. These are followed by the ore-beds, which include many famous iron-ore fields, the auriferous conglomerates, and the tin, gold, and platinum gravels. Interspersed in the purely descriptive material are sections devoted to a closer discussion of theoretical points than was attempted in the first part of the work. Instructive comparisons may be drawn between this work and the latest American work on the same subject—Lindgren's *Mineral Deposits*. Beyschlag, Krusch, and Vogt classify ores as magmatic segregations, contact deposits, cavity-fillings and metasomatic deposits, and ore-beds, in an order of decreasing relation to the eruptive rocks from which their materials have been ultimately derived. Lindgren's classification is into two groups only—deposits formed by mechanical and chemical modes of concentration respectively. The latter group is elaborately subdivided with an attempt to delimitate conditions of temperature and pressure for the various classes. Curiously enough Lindgren's classification, made from a totally different standpoint, works out in practically the same order with regard to eruptive origin as that of the work under review.

We have not sufficient space to criticise the work in as much detail as it deserves, but we may notice one or two points. A distinction is made between the "young" gold-silver and the "old" gold lodes, which is reminiscent of a still active principle in German petrological thought. The former group corresponds generally with Lindgren's class of deposits formed in connection with igneous activity, but at slight depth and at low temperature and pressure. The "old" gold lodes are regarded by this author as formed in the same way, but at greater depth and higher temperature and pressure. Beyschlag, Krusch, and Vogt, however, regard the West Australian gold-telluride ores as belonging to their "young" group, whereas Lindgren places them among the high-temperature lode deposits. It is notable, also, that the authors decide for an infiltration theory as against the marine placer theory advocated by J. W. Gregory and others for the origin of the auriferous conglomerates of the Witwatersrand. The recent work of the Transvaal Geological Survey, however, has produced evidence strongly in favour of the latter mode of origin. The present volume brings this book to a premature ending. The third volume indicated by its title has, so far as is known, not yet appeared; but it is not perhaps too much to hope, that when the present cataclysm has passed, the worthy conclusion to at least one great work "made in Germany" will appear.

G. W. T.

BIOLOGY

Modes of Research in Genetics. By RAYMOND PEARL. [Pp. vi + 182.] (New York: The Macmillan Company, 1915. Price 5*s.* 6*d.* net.)

DR. RAYMOND PEARL is well known in this country as the author of a series of papers in which the methods distinctively termed biometric and those associated with the name of Mendel are skilfully employed in combination. The present volume, although it contains a useful summary of the author's work on the measurement of the intensity of in-breeding, is chiefly devoted to a popular exposition of the problem of inheritance and to a criticism of the methods proposed for its solution. The advance of knowledge has been hindered both by popular indifference and by the mutual animosity of experts.

The early work of Prof. Karl Pearson was received by most pure biologists with ignorant contempt, while Prof. Pearson and most of his adherents have, in their turn, adopted an attitude of hostility, rather insecurely founded upon the intellectual superiority thought to be conferred by a knowledge of the differential calculus, towards most phases of Mendelian research.

Dr. Pearl analyses the fundamental problem into its elements, shows that both the mathematical and the experimental lines of attack are of essential value, and disposes of the claims made by rival schools to a monopoly of truth.

There will be differences of opinion as to details of Dr. Pearl's criticism, but his book may safely be recommended to the layman in search of an objective study of the subject.

M. GREENWOOD, Junr.

A Vertebrate Fauna of the Malay Peninsula: Reptilia and Batrachia. By G. A. BOULENGER, D.Sc., Ph.D., F.R.S. [Pp. xiii + 294, with 79 figures and a map.] (London: Taylor & Francis, 1912. Price 15*s.*)

THE Government of the Federated Malay States has decided to issue a Vertebrate Fauna of the Malay Peninsula, of which this present volume on Reptilia and Batrachia is the first. Mr. Boulenger is such a well-known authority on these two classes of animals that his name is a sufficient guarantee of the thoroughness and reliability of the work. Much in this volume is also contained in Blandford's *Fauna of British India*, with which it is uniform. Works of this character are very valuable to zoologists studying the distribution of species, and also to local naturalists, for they form an authoritative source of reference. Little can be said in criticism of the book, since its ultimate value depends on its use in reference, but it is well printed and arranged, and its copious alphabetical index allows of the easy finding of any species that may be required. The systematic index at the beginning enables one to see at a glance the relation any species bears to its allies or to other reptilia or batrachia. It forms a good starting-point for what should be a very useful work.

C. H. O'D.

NATURAL HISTORY

Natural History of Hawaii. By W. A. BRYAN, B.Sc. [Pp. 596, with 117 full-page plates.] (Hawaii: The *Hawaiian Gazette* Co., Ltd., 1915. Price $5.50.)

THE sub-title of this book describes quite well its contents and the ground it covers: "Being an Account of the Hawaiian People, the Geology and Geography of the Islands, and the Native and Introduced Plants and Animals of the Group." The problem of the volcanic oceanic island is always an interesting one to the

biologist, and in the case of the Hawaiian group it has added to it a race of people who came from a considerable distance within what in Europe would be considered historic times. Here these people, of Polynesian stock, settled down and reached a considerable degree of culture and civilisation. Their environment determined to some extent the course of their development, and all this is very well pointed out by the author. Another very useful feature in the book is that the indigenous plants and animals are most carefully marked off from those that have been introduced. Thus there is a permanent record of the introduced forms readily available to any naturalist visiting the islands. No amphibia were found originally, but since they have been introduced from the United States and from Japan and are now commonly distributed. Particularly full accounts of the shells, the birds, and the fish are given, but in the case of the last two the plates would have been improved if a clearer indication of the actual size of the animal had been given in each case. Then, again, most of the plates suffer from overcrowding, due no doubt to a laudable desire on the part of the author to get as much as possible into the book. It is stated in the Preface that the work is not an original contribution to the natural history of Hawaii; be that as it may, it certainly makes available to the biologist and general reader a vast amount of information not otherwise available. Throughout it is clear and readable, and it is subdivided in such a manner that it is very easy to turn up any particular subject that may be desired. Most of the plates are from negatives in the possession of the author, and they give a very good series illustrating the life of the people and the striking features in the scenery of the islands. To residents and visitors it should prove of great utility, but it also appeals to the naturalist and nature lover the world over. The book is very well printed, and the long index, which is also partly glossary, forms a valuable addition. To quote again from the preface: "If the bare facts of nature have been clothed with living interest sufficient to make them acceptable and full of information for the general reader, . . . and above all should the book prove generally useful, the author's ambition will have been attained." We think that there is no doubt that these modest conditions have been fulfilled.

C. H. O'D.

The Rambles of a Canadian Naturalist. By S. T. WOOD. [Pp. vii + 247, with 6 coloured plates by Robert Holmes, and decorative headings by students of the Ontario College of Art.] (London: J. M. Dent, 1916. Price 6*s*. net.)

HERE are a series of short essays on many things—the birds, the beasts, the insects, flowers and trees—that are to be met by the naturalist rambling in Eastern Canada and arranged in the order of the seasons. It is not a book that is to be read as a whole, with intricacies of plot and action to be followed up, but a series of quick, deft, delicate pictures of wild life in our great colony. When the essays are read on one after another, a sense of sameness and more than a hint of repetition will be felt, but this is not manifest if they are read one or two at a time at intervals. Exactly why this feeling comes it is hard to say, for the author has fluent command of a wide and well-chosen vocabulary. One point is well worthy of commendation, and that is the small number of "Americanisms" present. These tricks of construction so fascinating in certain types of novel or useful in the scrambles of a hustling world form such a forceful part of the every-day vocabulary in Canada that it seems hard for them to be kept out of essay writing. Yet many of them appear so incongruous as to be almost offensive to English readers when they occur in what would otherwise be almost

polished writing. No deep anatomical or ecological discoveries will be found in this book, but there is much of the grace of the woodlands, the scent of the flowers, and the freshness of outdoor wild life as seen and recorded by a lover of nature. Some of the illustrations are good, but one or two are rather crude and do not appear quite in harmony with the book. The headings are variable but nowhere aggressive, and some of them are quite charming pieces of work.

C. H. O'D.

A. **The Life Story of an Otter.** By J. C. TREGARTHEN, F.Z.S. [Pp. xiii + 188, with 8 illustrations.] (London: John Murray, 1915. Price 2*s*. 6*d*. net.)

B. **The Story of a Hare.** By J. C. TREGARTHEN, F.Z.S. [Pp. xi + 199, with 8 illustrations.] (London: John Murray, 1915. Price 2*s*. 6*d*. net.)

MR. TREGARTHEN has undoubtedly made a place for himself among the writers on wild life in this country, by the publication of the two volumes under review. The reception accorded them in their first form some years ago was evidently such as to encourage the publisher to reprint them at a lower price, and we can heartily commend this step. Their reprinting should introduce them to a wider circle of readers and give them an opportunity of endorsing the good opinions that have already been won by the volumes. As a fitting background for the well-written stories of the lives of the two animals we have a charming account of the enticing west-country scenery amid which the dramas are played; and a drama the life of a much-hunted animal must always be. There are in it the periods of youthful gambols and play, so skilfully portrayed by the author, but after all, what are they but the preparations for the lifelong struggle to escape being killed which has a bloody termination in the case of the otter and as tragic an end in the hare when, bereft of its cunning, its skill and strength fail?

In the preface of one of them the author submits that certain attempts to interpret the animal's actions and motives are "for the most part of a safe character," and with this we are in entire agreement. Indeed in this respect both accounts seem to have avoided pitfalls into which writers of animal biographies are prone to fall by endowing their characters with an undue amount of moral and introspective activity. To one who has not missed opportunities of making the acquaintance of these animals under natural conditions as well as in the hunting field there is practically nothing in either volume that runs counter to experience, and herein lies a great part of the charm of the works. Both could very probably be true life stories, although one is forced to recognise and admire the patience of the author in getting together his facts, or rather his experience, and the skill with which he has woven them together.

A. The title of this book is quite indicative of its contents, for it treats of the whole life of the otter from its birth till its "kill." The various phases of its life are all treated in turn, including the extraordinary habit it exhibits at certain times of slaying, as far as can be seen, simply for the mere love of killing and not in defence or for food.

B. This volume treats in the same way of the life of the hare, and though of course there is not the same series of exciting hunts for food, it none the less retains the interest of the reader to the end.

Together they form a pair of books that should find a place in the library of all lovers of the countryside, giving as they do an interesting and charming picture of the everyday lives of these often-mentioned but little-known wildlings.

C. H. O'D.

More Minor Horrors. By A. E. SHIPLEY, Sc.D., F.R.S. [Pp. xiv + 163, with 49 illustrations.] (London: Smith, Elder & Co., 1916. Price 2*s.* net, cloth; 1*s.* 6*d.* net, paper cover.)

THIS volume forms a continuation of the author's *Minor Horrors of War*, reviewed last year. It is written in the same easy and pleasant style. There are fourteen chapters devoted to accounts of the anatomy, life-history, and economics of various insects and of rats. The subjects dealt with are cockroaches, the bot or warble fly, the mosquito (at considerable length), the yellow-fever mosquito, the biscuit weevil, the fig-moth, the stable fly, and rats and field mice.

The trouble caused to our sailors by cockroaches on board ship, both by the annoyance they cause and by the consumption of food supplies, is related. The ravages of the bot or warble fly in leather, in meat, and indirectly in the milk supply are set forth. The attacks of this insect may cause cattle to stampede. It is the larva which is harmful, as the mature fly is rarely seen. *Anopheles maculipennis* is described in detail, and its harmfulness as a transmitter of malaria is pointed out. When camping in the tropics, it is always well to keep to the windward of a native village, as the insects may be blown for some distance by the wind. Anti-mosquito measures are clearly stated.

The larva of the so-called biscuit "weevil" burrows into and attacks the dried biscuit—the "hard-tack" of the Navy. Again, it is the larval stage (or "worm") which is so harmful in the case of the fig-moth, *Ephestia cautella.* The stable fly, *Stomoxys calcitrans*, spreads trypanosomiasis or "surra" among horses and cattle in the tropics. The spreading of acute epidemic poliomyelitis or infantile paralysis is also attributed to this fly. The book concludes with an interesting account of rats and field mice. Plague may be conveyed by the former, and the latter are a nuisance in the cornfields.

F.

ANTHROPOLOGY

The Men of the Old Stone Age. Their Environment, Life, and Art. The Hitchcock Lectures of the University of California for 1914. By HENRY FAIRFIELD OSBORN, Sc.D. [Pp. xxvi + 545, with 8 plates and 269 other illustrations.] (London: G. Bell & Sons, Ltd. New Edition, 1916. Price 21*s.* net.)

THE Palæolithic Period is evidently a favourite subject with the educated public, for the number of English books dealing in a popular manner with fossil men is now considerable. Within the last three years, large treatises have been written by Profs. James Geikie, W. J. Sollas, and Arthur Keith, and we have now to welcome this massive monograph by Prof. Osborn, who is, of course, one of the leading American authorities on palæontology. In 1912 the author went for a tour through that region in Southern France, Northern Spain, and Northern Italy which has long been famous for its palæolithic relics, and he had the advantage of being shown round the various prehistoric sites by distinguished French archæologists—Henri Breuil, Émile Cartailhac, and others. The book was first published in June 1915, and before the end of the year it became necessary to print a second edition.

The scheme of the book is strictly chronological, and it deals exclusively with the ancient men of Europe and Java, not with the American finds. The greater part of the work is a compilation of well-known facts, such as has often been published before. The usual descriptions of the Javan, Piltdown, Heidelberg, and Neandertal species are given, but on the anatomical points raised the author has

little that is new to say, and he leaves the deeper problems of human evolution almost untouched. The descriptions of the various bone and stone implements are, however, excellent, and the book deals more fully, if not more accurately, with the fauna and flora than any other book in the English language. The illustrations, which include several good maps, are also admirable.

The story is told, as already stated, in a chronological manner. Now, as is well known, human palæontologists are divisible into two schools, which may be styled the extreme and the conservative. The extreme views of such writers as Profs. Keith and Rutot are not considered by Prof. Osborn worthy of discussion in this popular volume. It is not, however, so generally known that the conservative school is itself subdivided into two subordinate classes of opinion, differing from one another on the question of the geological antiquity of man in Europe. All the scholars of the conservative school agree in accepting the same discoveries, and in discarding an array of finds which are brought forward by the extremists; the point in dispute is the correlation of the accepted discoveries of bones and implements with the glacial and interglacial ages. Prof. Osborn accepts the ultra-conservative interpretation of the facts, which is that believed by most French authorities, and he accordingly regards the whole of the Late Palæolithic Period as later than the Fourth (Würmian) Glacial Period. The other scheme is that adopted by the leading German scholars, and by Prof. James Geikie and probably by most English geologists, though not by Prof. Sollas. The reader should remember when perusing the author's chronology that the two ingenious schemes are different from beginning to end, and that they are notoriously a matter of controversy. We think the author is doing a very useful service in attempting to describe quite definitely the fauna of each glacial and interglacial period. The time for definite statements on these points has arrived; and it is, of course, very possible that the scheme which Prof. Osborn and his eminent French colleagues advocate may ultimately be proved to be the true one; but it is unfortunate that he has not always been careful to represent correctly the views of those from whom he differs. For instance, the table on page 33 quite misrepresents Geikie's scheme. That writer placed the Aurignacian and the Solutréan in the transition time between the Third Interglacial Period and the Fourth Glacial, and regarded the Magdalenian as essentially the culture of the Fourth Glacial Period. Prof. Osborn makes Geikie attribute the Magdalenian entirely to post-Würmian times. Again, the Lower Turbarian Age of Geikie, which was Neolithic, certainly does not correspond with the Bühl stadium in the Alps (see pp. 361–3). It probably corresponds, as Geikie believed, with the later Gschnitz stadium, but was certainly not earlier. The trouble arises from the fact that Geikie makes six glacial epochs, not seven, the reason being that in Scotland he could not disentangle the traces of the Würmian and Bühlian glaciers from one another.

A. G. THACKER.

ENGINEERING

Text-Book of Mechanics. Vol. VI. Thermodynamics. By LOUIS A. MARTIN, Junr., Professor of Mechanics at the Stevens Institute of Technology, Hoboken, N.J. [Pp. xviii + 313, with 78 figures.] (London: Chapman & Hall, 1916. Price 7s. 6d. net.)

THIS little text-book is intended for the use of engineering students who have no previous acquaintance with thermodynamics, but who possess some knowledge of the rudiments of the calculus. In its general plan it follows the usual lines, and it will be much appreciated by the teacher for its numerous examples; it contains

no fewer than 382 with answers. The author has not, however, made a sufficient effort to raise the subject above the rather uninspiring level of a branch of applied mathematics, and the absence of all reference to the experimental and historical sides of the work is very regrettable. A treatment which gives little or no account of the work of Joule and of Meyer, of Kelvin and of Clausius, and which does not as much as mention Rumford, is distinctly incomplete. A clearly reasoned statement of Carnot's work is almost essential if the subject is to become a live one to the engineer; the mere dry bones—a cycle treated along with, and at not much greater length than, Ericsson's and Otto's, and a mangled statement of Carnot's Principle—are not enough.

Perhaps the weakest thing in the whole book is the proof of the identity of Kelvin's thermodynamic scale with the absolute gas scale (which is only defined implicitly). The student is referred to one of Callendar's papers for the numerical value of the efficiency of a Carnot cycle with air as working substance. Since the efficiency has already been worked out for a perfect gas, this is as unnecessary as it is inconclusive to the novice. Indeed, the whole of Kelvin's treatment of this question is best relegated to an appendix.

The chapters on vapours and vapour cycles are excellent, and such questions as entropy and total heat are treated very carefully. The notation, however, is not very suggestive, and the equations here (and throughout the book) are given a clumsy appearance by the continual insertion of the mechanical equivalent J in the denominator of the work terms. A direct proof of the Clapeyron equation for the liquid-vapour change is desirable, for it appeals far more to the engineer than that given by the Maxwell relations. The flow of fluids, with special regard to the turbine, is discussed at length, and the book closes with an account of the differential equations of thermodynamics.

It ought to be pointed out that the author has attempted to explain clearly what is meant by a reversible cycle, and that he does not confuse adiabatics and isentropics—a rare virtue indeed! On the other hand, he has permitted himself to use a system of spelling which cannot but prejudice his book in this country. "Thru," "thoroly," "enuf," "gage," are fortunately not yet tolerated in English orthography, however legitimate they may be in American.

D. ORSON WOOD.

The Thermo-dynamic Properties of Ammonia. By FREDERICK G. KEYES and ROBERT B. BROWNLEE. [Pp. v + 74, with 7 figures, numerous tables and large Mollier diagram.] (New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1916. Price 4*s*. 6*d*. net.)

THE Government and the scientific and technical institutions of the United States of America have done and are doing a vast amount of scientific work of direct and immediate value to trade, industry, and commerce. Refrigeration, in its many sub-divisions, has received special attention and affords a striking example of what pure science can do for commerce and incidentally points the way to the men of science (and Government) of this country.

The work under review was "computed for the use of engineers from experimental data derived from investigations made at the Massachusetts Institute of Technology" with the hope that it "would prove useful in controlling the performance of refrigerating machines."

The whole research has been carried out on the soundest possible lines—both from the scientific and utility points of view. The authors in their Preface state: "After the completion of the preliminary work, in connection with which the

already existing data had been critically examined, it appeared desirable to carry out a more comprehensive experimental investigation. Throughout the whole work we have been indebted to Prof. Miller for his advice and support." Needless to say, the comprehensive tables published at the end of the book as the outcome of such work may be accepted by refrigerating engineers as representing the latest and best results.

Scientists, as a body, will be interested in the thermo-dynamical portion of the work. Mr. Brownlee's results on the specific volume of liquid ammonia are so different in the extreme values as to give quite a different form of curve from that obtained from the plotted results of Dieterici and Lange. In the main Mr. Brownlee's figures are practically a mean, but the whole question is so important that it should attract other investigators.

The authors have had a very great advantage in having the critical investigation of Goodenough and Mosher on the "Properties of Ammonia" (*Bulletin No.* 66, University of Illinois, 1913) beside them and have not failed to compare equations, methods, and results. The methods of attack are, in the main, very different, and without detracting in the very least from the earlier work, we must say the present work carries conviction with it.

We particularly commend the sound judgment of publishing all the records in the C.G.S. units and the tables in British units. The working engineer both in U.S.A. and Great Britain uses the latter set of units—in their own good time they will use the C.G.S. We have seen recent volumes intended for engineers—some wholly in C.G.S., others in centigrade-pound, and others wholly in British units. Which is best? The authors apparently think all three are open to question and supply a fourth, *i.e.*, the scientific work in C.G.S. and the utility side in those units employed by the user. We agree with the authors and think it points a lesson to the whole Empire.

J. WEMYSS ANDERSON.

Elementary Applied Mechanics. By T. ALEXANDER, C.E., M.Inst.C.E.I., M.A.I., and A. W. THOMPSON, D.Sc. [Pp. xx + 512, with numerous diagrams.] (London: Macmillan & Co., 1916. Price 15*s*. net.)

THIS work, although it is called Applied Mechanics, deals only with a fraction of the subjects usually scheduled under this title—*i.e.*, with the "theory and design of structures," together with the necessary introduction, "strength of materials." Further, the book may well serve as a text-book for the undergraduate in engineering science during his college career, and therefore the term "elementary" does not imply that it is a suitable text-book for students in evening classes in their first year.

The subject-matter, from the first chapter on "Lineal Stress and Strain" up to Chapter XXII on the "Scientific Design of Masonry Arches," is dealt with, step by step, in masterly style.

The authors are clearly masters of their subject, and this allows them to develop theories, such as Rankine's method of conjugate load areas when dealing with the equilibrium of arches, and to introduce new methods of treatment and also, further, to freely quote methods of solving particular problems developed by others, and which, the authors feel, merit inclusion in their work. A good example of this is to be found in Chapter XIX on "Long Steel Struts," where Fidler's excellent method of design is quoted and tables given.

The book is altogether refreshing after the very stereotyped methods followed in most text-books.

There are, however, one or two practical points that require the attention of the authors. In such a text-book it is wise, perhaps, to leave out the important if somewhat long and very practical work on rivets, rivet holes, and riveted joints in general. This should not, however, mislead the authors into such examples as given on pp. 280 and 281. Such a construction as shown in fig. 26, p. 280, is not possible in practice, as the rivets must be "staggered" (on the scale shown) to get them into position.

Further, in example No. 2, p. 281, it is given: "If the rivets in fig. 26 be pitched 4 in. apart, find the diameter necessary for each rivet." The problem is worked out, and the answer given as 1·2 in.

Such an example gives the student wrong ideas altogether, as the size of rivet is a most important item in constructional work, and is determined by (with other considerations) the appliances possible for closing them. A 1¼ in. diameter rivet may not be out of place on a Quebec bridge, but it certainly would not be used in an ordinary plate-web girder from which the example is taken. A ¾ in. or ⅞ in. rivet would be used, *and the pitch made to suit the rivet*.

We further certainly think more attention should be given to secondary stresses and strains in structures, and, while admitting the very great value of the theory and practice of the steel-arched girder, it is hardly wise to quote, without some word of warning, examples from station-roofs, which the tendency of modern design clearly indicates as being quite out of date, as the huge span is now replaced by a series of small spans.

A number of small corrections is necessary throughout the book to bring it into line with the practice adopted in printing the proceedings of our learned societies. For instance, in Example 18, p. 15, we read: "A rod of steel, 10 ft. long and ·5 of a square inch in section, is kept at the proof strain by a tension of 25,000 lbs. . . ."

We would much prefer to see this printed: "A rod of steel, 10 ft. long and 0·5 of a square inch in section, is kept at the proof strain in tension by a load of 25,000 lb. . . ." The 0·5 and lb. should be particularly noted.

The suggested corrections are, after all, small, compared with the vast amount of excellent work in the book, and we could wish that all text-books in use in engineering colleges and schools were of the type of *Elementary Applied Mechanics*.

J. Wemyss Anderson.

The Principles of Electrical Engineering and their Applications. Vol. I. Principles. By Prof. Gisbert Kapp, M.I.C.E. [Pp. xii + 356, with 175 figures.] (London: Edward Arnold, 1916. Price 15*s*. net.)

Much water has flowed under the bridge since the day when an engineer could get along without knowing something about electricity. Nowadays it comes within the field of every engineer, from the builder of the Panama Canal, where a large station has been erected to supply electrical power for operating the locks and all the manifold electrical contrivances that are used for expediting traffic, to the water engineer, who, as the preface suggests, "wants to know something about the drop of potential along the rails of a tramway because the resulting earth currents may eat up his pipes."

This volume, to which a sequel is promised, is introductory. There is nothing in it about any kind of electrical machinery. The most striking features of the book are its directness, its "practicalness," and its conciseness. It sums up in 350 pages all the knowledge that any engineer wants of general electrical

engineering distribution problems. As the preface says, it probably contains too much. It is easy to criticise and to suggest that the general engineer, for whom the book is intended, does not want to know all the methods for measuring low resistances, for comparing standards, for testing the magnetic quality of iron, the theory of the fluxmeter and so forth; but "extras," if one may call them by that name, very often prove extremely useful (for there is nothing described that cannot be used by the engineer for practical purposes), and it is better, generally, to have too much rather than too little.

The book lays down with great clearness and with the use of only the simplest mathematics the fundamental principles underlying electrical measurements, the general laws that must be observed in obtaining an economical distribution system for electric power, the laws of electrostatics which are of practical importance in the design of cables and overhead lines working at high pressure, the laws of magnetism, and some of their applications to electro-magnets. There is a very useful chapter on units, and at the end of the book a short introduction to the principles of alternating current working. Any one with experience in the instruction of engineering students knows how difficult it is to decide what things must be left out in a course of this kind. There are so many subjects for which a case can be made out, that they may, and probably will, be of value to the general engineer. So far as it goes, Prof. Kapp's book is one of the best selections that there is, though one may express the belief that it would have been of more general utility if the price had been lower. There are other books of the same kind that cover nearly the same ground, though not possibly with the same thoroughness and breadth of treatment, which may be bought for half what this book costs. It is to be hoped that the publishers may find it possible in later editions to bring it more within the reach of the young engineers who are likely to use it.

PHYSIOLOGY

The Respiratory Exchange in Animals and Man. BY AUGUST KROGH, Ph.D. Reader in Zoophysiology, University of Copenhagen. [Pp. viii + 173, with 35 figures.] (London: Longmans, Green & Co., 1916. Price 6*s*. net.)

DR. KROGH'S Respiratory Exchange is one of the best monographs of the excellent series edited by Prof. Hopkins and Dr. Plimmer.

It deals chiefly with the quantitative aspect of this exchange and takes no count of the physiological mechanisms involved, but (in spite of a prefatory disclaimer) is, within its limits, a really exhaustive review of the subject.

Methods of measuring the exchange are fully described, though from the theoretical rather than from the practical experimental standpoint. "Standard Metabolism" is defined as that corresponding to a minimum functional activity; and the bulk of the volume is devoted to this standard metabolism—to its variations under the influence of internal and external factors, to its alterations during the life-cycle, and to the differences exhibited in it by different species, and by more widely divergent groups of animals.

The book is no mere compilation—Dr. Krogh has, himself, worked over much of the ground, and is, consequently, able to furnish many original data and to deal with the work of others from a really critical standpoint. Its subject-matter is chemical rather than physical, and, though dealing with chemical physiology rather than with physiological chemistry, it will be of particular interest and value to those concerned with the chemical details of animal metabolism. In style it is

judicial rather than terse. It will, therefore, appeal more strongly to the post-graduate student than to the undergraduate.

The text is illustrated with thirty-five figures and upwards of forty tables. A full bibliography is appended. This is classified by chapters, but its subdivisions are unfortunately separated from the chapters to which they belong. Apart from this bibliography there is no index of authors. There is, however, an excellent index of subjects.

We would repeat that the monograph is one of the best of an excellent series.

W. L. SYMES.

MEDICINE

The New Psychiatry; being the Morrison Lectures delivered at the Royal College of Physicians of Edinburgh, March 1915, by Dr. W. H. B. STODDART. [Pp. iv + 66.] (London: Baillière, Tindall & Cox. Price 2*s*. 6*d*. net.)

MENTAL problems are never easy to solve, and there is always something baffling, alluring, and mysterious in the endeavour to explain the working of the normal human mind. Much more difficult must be the effort to investigate the intricacies of the abnormal mind, so that there is a natural tendency to welcome any fresh means of exploration that may appear, provided only that these means are advocated from a reputable source. For some time past the old method of psychological analysis by observation and induction has been supplemented by what has been abbreviated into "psycho-analysis," but why this method is denominated the "new" psychiatry remains untold; and the author of these lectures, which demonstrate the utility of the method, does not attempt to supply the answer.

To describe any method of analysis as "new," or to apply the epithet to a creed or a belief, is to imply some divergence from previously accepted facts; but the only thing that is new in the "new" psychiatry is to trace the origin of all normal and abnormal mental phenomena to the primitive instincts of sex and love. Some years ago the "new" psychology was brought forward with a flourish of trumpets as a discovery, but its only significance was to imply that all experimental methods which could be supplied from the laboratory would now be added to observations and inferences drawn from introspection; in another sphere the "new" theology implied fundamental changes, yet it was based upon common doctrinal dogmas. The only effect of these "new" additions has been to strengthen our belief in the old, and, in regard to the subject of this review, to question the value of what purports to be of extreme help in the diagnosis and the treatment of mental diseases.

The first to initiate the study of mental phenomena in relation to sex was Sigmund Freud, of Vienna, and he has few more devoted disciples in this country than Dr. Stoddart. K. Yung, of Zurich, is another physician of experience and repute who represented the so-called Freudian School, but he has lately broken away completely from the teachings of his master. The essence of Freud's psychiatry is the assertion that erotic and sexual perversions are at the root of all analysis of the unconscious mind, and that these are the motive power of the conscious mind and the *causa causans* of all abnormal mental symptoms; the conscious active mind of thought, feeling, and will, according to Freud, being a commingling of the unconscious and the conscious. This is no new discovery, for it has always been within the knowledge of every physician practising in mental diseases that

the unconscious mind plays a great part in the conscious; and that certain definite sensations or experiences may determine certain attitudes of mind long after the memories of them had faded, and that such memories of past experiences are always potential agencies, at any later period, for determining mental states. Even the lay mind appreciates the story of the maid who, during the delirium of a fever, spoke excellent Greek and Hebrew, because in her girlhood she had waited upon an old professor who read his classics aloud. Further, the records of every alienist physician can testify to the horrible vocabulary and the shocking indecencies indulged in by young girls suffering from adolescent mania, or young mothers suffering from puerperal insanity—utterances which no decent person could have imagined had ever been heard by ears trained and brought up in refined and delicate circles. It is certain that these words—accidentally heard on one occasion—have by some association in the unconscious mind been dug out of the obliterated memories of past experiences; and based upon this surmise, it is truly asserted that submerged in the brain cortex of each of us there exist enough bad words to horrify the most profligate and debased! At any rate it is a well-known fact, as Freud has pointed out and as Dr. Stoddart repeats, that in the education of children many incidents are remembered, although more are forgotten; that many of these incidents, although forgotten, remain engraved upon the tablets of the unconscious mind, which is "wax to receive and marble to retain." The great trend of social and ethical education proceeds also in the direction of repressing natural tendencies, suppressing feelings and passions, inhibiting personal wishes and sentiments, as is pointed out in these lectures, until the pupil after constant effort eventually succeeds in controlling them. These tendencies, thus repressed "like steam in a boiler," as Bergson states, may at any moment, when the conscious control is loosened, become uppermost in the mind, and when this state of diminished inhibition has occurred, then arise the dreams, longings, perverted desires, and delusions which constitute insanity.

These pathological states are, it must be insisted, not alone, as Freud and the author assert, the result of unpleasantly painful or shameful events in the past history of the sufferer. Insanity and other forms of mental perversion are indeed not chiefly even the consequences of uncleanness of mind, of sex perversion, lustful covetousness, and base desires. Those physicians who have devoted their lives to the investigation of mental phenomena assert that the abnormal conditions described as insanity are the result of environmental influences, fatigue, and exhaustion; they occur through inheritance or from various poisons—some generated within the body and others introduced into it from without. Mental diseases are complex factors caused partly by bodily disorders, such as are associated with alcohol, syphilis, tuberculosis, or organic diseases of the brain, partly also through disturbed mental effects, such as worry, grief, anxiety, bereavement, and losses, but not, as the Freudians maintain, chiefly through the effects of libidinous desires, suppressed sexual longings, or unjustifiable affections.

The first lecture of the three under consideration deals with the fundamental psychic instincts, and here the Freudian School is seen at its worst. Dr. Stoddart suggests that "Freud is right that breast-sucking for the purpose of nourishment (in babes) is partly of sexual import"; that the pain of constipation is accompanied in children "by a pleasurable sensation which the child seeks to experience again. It is a variety of masturbation," and he further adds "the desire to retain fæces becomes sublimated into the desire to retain money, and those who have had experience of psycho-analysis know how commonly in the neurotic 'fæces' symbolises 'money'!" This is Freudism to the extreme limit of disgust, and the

"endo-psychic censor" in these lectures should be compelled to feel ashamed of itself; unfortunately, however, it is a purely fictitious "ego," and cannot express contrition nor suggest reformation! The second lecture deals with the technique of psycho-analysis and the last with its application.

It is claimed by the disciples of Freud that they are able to disclose the workings of the unconscious mind by the examination and comparison of spontaneously uttered thoughts—*i.e.* by free association; and that once the clue of sexual meaning is elicited, therein lies the bed-rock and the root-cause of all forms of mental perversion. To be always seeking for these is to reflect the mind of the explorer, and it is an insult to an innocent sufferer to be placed under the care of such an investigator. Dr. Stoddart is, however, an able clinician and an interesting and clear writer, but in these lectures he is the too ardent disciple of an ethically objectionable foreign tutor.

ROBERT ARMSTRONG-JONES, M.D.

Localisation by X-Rays and Stereoscopy. By SIR JAMES MCKENZIE DAVIDSON, M.B., C.M. [Pp. xi + 72, with 35 stereoscopic illustrations on special plates and other figures in the text.] (London: H. K. Lewis & Co., 1916. Price 7*s*. 6*d*. net.]

THAT the size of a book is not always an indication of its value is amply substantiated by the publication of this work by Sir James McKenzie Davidson. Into about seventy pages of text is concentrated the life work of McKenzie Davidson in the field of X-ray research and practice.

Röntgen published his discovery of the effects of X-rays on photographic plates in 1895. McKenzie Davidson visited Röntgen in 1896, saw his work, and quickly grasped the significance of the discovery. As a practising ophthalmologist he foresaw the great value of the new rays in the localisation of foreign bodies in the orbit, then, as now, one of the most difficult matters for the surgeon to decide.

Steady application to the problem led to the publication of a paper in 1898 by McKenzie Davidson, "Remarks on the Value of Stereoscopic Photography and Skiagraphy." On the author's work are based practically all the modern methods of localisations by means of X-rays. Many modifications have been introduced, most of them efficient substitutes when rapid work is required, but none of them likely to displace the original method when accuracy of result is the main object.

The author calls attention to the value of a combination of precise localisation by the use of three co-ordinates, with stereoscopic radiography. In this combination his method excels all others, in that the one may be used as a check observation on the others, and it possesses the inestimable advantage that, to use McKenzie Davidson's own words, "the stereoscopic relief will enable the surgeon to visualise the relative position of the cross wires and the bones of the patient to the foreign body." In these words lie the gist of the whole method of localisation. To ensure accurate results the surgeon must visualise the position of the foreign body and its relation to well-known anatomical landmarks. An estimation of the depth of the foreign body from the skin surface enables him to approach it with confidence, and when these anatomical landmarks are borne in mind, he can quickly cut down and remove it.

This work sets out in simple language the methods of McKenzie Davidson, no elaborate apparatus is described, all of it might readily be constructed by the "handy carpenter," and when completed the operator is fully equipped to carry out with the utmost accuracy the localisation of a foreign body in any portion of the human anatomy.

The illustrations are good, all are of practical value, and each shows striking points in the technique.

The introductory chapter deals with the X-ray tube, the physics of this subject being briefly entered into. The importance of a fine focus point in the tube is emphasised. The chapter ends with a description of the Coolidge tube.

X-Ray Stereoscopy.—The details of the method are well described. An important matter is discussed under the heading "The Misleading Single Picture."

The works of Wheatstone, Brewster, and Le Comte are recommended to those who are interested in the principles underlying stereoscopy. The vertical ray is employed in stereoscopic work, and the methods of selecting and utilising it are well described, as is also the subsequent arrangement of the negatives for the examination of the radiographs in the stereoscope.

Chapter IV. deals with rapid X-ray localisation, a subject of the utmost importance at the present time. A new couch adapted to the requirements of rapid localisation is illustrated and described. It is light, portable, and can be readily constructed by any one who can handle tools.

The measurement of the displacement of the shadow of the foreign body on the screen is next described. This rapid method is valuable in times of stress when many cases have to be done quickly.

The author next deals fully with the cross-thread method of measuring. Elementary facts in geometry are utilised to simplify the explanation, and a number of instructive drawings are included in the text.

This is one of the best chapters of the book, and should be read carefully by all workers who desire to grasp the principles of localisation by this method.

The localisation of foreign bodies in the eyeball and orbit is dealt with in full. This is, as would be expected, the most instructive section of the book. The method is fully described, and a number of instructive stereoscopic photographs employed to render the text as clear as it is possible to make it.

An important piece of auxiliary apparatus is described in Appendix I.—the Telephone Attachment in Surgery. This should be read by all surgeons who have to deal with foreign bodies in the human frame.

A most instructive appendix on the localisation from a single photograph is a feature of the book. This is most clearly written, and illustrates how from a single plate all the data necessary for the guidance of the surgeon may be obtained.

The author concludes the book with a short appendix on the rectification of the current supplied to the X-ray tube.

The publication of a book on localisation by X-rays and stereoscopy at the present time will be welcomed by all who are engaged in X-ray work in connection with the war. Operating surgeons will find in its pages a lucid and instructive description of the original method of localisation which must greatly aid them in the interpretation of the X-ray plates of their cases.

Milk and its Hygienic Relations. Medical Research Committee Series. By JANET E. LANE-CLAYPON, M.D., D.Sc. [Pp. viii + 348.] (London: Longmans, Green & Co. Price 7*s*. 6*d*.)

THE Medical Research Committee under the National Health Insurance Act entrusted to Dr. Janet Lane-Claypon an important investigation into milk and its hygienic relations. No article of food is of greater importance, nor more widely used, at all ages, in health and in sickness, and it is not surprising therefore that, notwithstanding the many treatises written upon the subject, and the great attention given to it, there is still room to reward the further investigations by the author.

Dr. Lane-Claypon has gone into the subject with her well-known care, with the result that in one volume there is comprised not only the results of her own investigations, but those of a very large number of British and foreign workers. The facts are stated clearly and succinctly, and are presented in an interesting and a readable manner.

Variations in general composition of milk, and their causes, whether of breed of cow, influence of diet, period of milking, and so forth, the writer deals with fully. Reasons are assigned for practical and important conclusions arrived at: on the question of lactation in the human subject, there is no necessity to wean an infant should pregnancy supervene, unless the health of the mother requires it; the practice in regard to farming is almost universal in allowing cows to become pregnant within a few weeks or months of parturition. In ordinary lactation, feeding a baby every two hours by day, and every four hours by night, is too frequent—the infant is not hungry, consequently the gland is not emptied and the due allowance of fat is not received.

The so-called "biological properties" of milk form the subject of several important chapters which are deserving of careful study, that relating to the substances concerned in the production of immunity being especially interesting. Our knowledge upon the transference of immunity by suckling demonstrates the immense value to the young of the colostrum of the mother, and emphasises the need for breast feeding; the artificially fed infant, although it does not die, and may even show no outwardly detrimental effects from early artificial feeding, leads a much more precarious existence than its fellow who has received its natural food after birth; the difference in progress made by infants fed at the breast, as compared with those fed in other ways, is obviously to the advantage of the breast-fed infants.

No one doubts the advantages of breast feeding, where it can be carried out, but in instances in which mothers are unable to suckle the infants there is no question that cow's milk, so altered as to most closely resemble human milk, is the best alternative. Dr. Lane-Claypon's previous investigations into the relative nutritive value of "raw" and "boiled" cow's milk adapted for the food of infants are well known. Many authorities are quoted upon the subject.

It is, perhaps, to be regretted that owing to the paucity of published material, the author has not been able to quote more fully the results of the use of humanised sterilised milk in this country, as well as of its use abroad. Many towns have now had a prolonged experience of the use of this food, and its value is beyond all question. Dried milk is also having a wide use, and its value is of considerable importance, provided that sufficient intelligence is applied in preparing it for the infant's use. The electrical sterilisation of milk is fully described.

The text is well illustrated by plates, and the volume contains much valuable information which will pave the way to further advance in this country.

E. W. Hope.

Medical and Veterinary Entomology: a Text-Book for use in Schools and Colleges as well as a Handbook for the use of Physicians, Veterinarians, and Public Health Officials. By William B. Herms. [Pp. xii + 393, with 228 text-figures.] (New York: The Macmillan Company, 1915. Cash price in Great Britain, 17*s*. net.)

Although many works relating to medical entomology have been published during recent years, there exist but few text-books on the subject. Prof. Herms' volume, while dealing more intimately with the American fauna, yet provides a

good account of parasitic forms in general. It is not intended as a comprehensive treatise, but, rather, is written with the object of systematising the subject, and securing for it a place among the applied biological sciences.

The introductory chapters afford the student a clear insight into the economic importance of the subject, the nature and effect of parasitism, and the general characteristics and relationships of insects. The prominence given to the morphology of the mouth parts—several types being discussed in detail—is much to be commended. The information relative to the noxious forms concerned is given concisely and systematically, and includes much original matter. Certain experiments conducted by the author are carefully described, the methods employed demonstrated, and lines of research suggested. Considerable space, in due sequence, is devoted to the *Diptera* and, as is usual in books of this nature, certain Arachnids, such as ticks and lice, receive attention because of their intimate connection with the spread of disease in man and animals.

The value of this admirably produced volume is enhanced and its subject-matter rendered more easily assimilable by the numerous and, in general, excellent photographs and line drawings provided. The work calls for little adverse criticism, yet some stricture must be passed upon the treatment accorded to those important insects the Tsetse-flies. The information given by no means approaches completeness, and in places is much out of date. Notably is this the case in the systematic portion where the author has followed Grünberg, and entirely ignored the recent and valuable works of Austen and Newstead. Consequently we find but few species mentioned, while the easily distinguished and very important species *G. morsitans* is not only "said to be almost identical with *Glossina longipalpus*" (sic), but is stated to coincide with the latter in its distribution.

It is to be regretted that these terrible pests have not received the careful consideration to which their importance entitles them, and the inadequate treatment of this section of the work tends, unhappily, to mar the general excellence of the whole.

H. F. C.

GENERAL

Reminiscences of Fifty Years' Experience of the Application of Scientific Method to Brewing Practice. By Horace T. Brown, LL.D., F.R.S. [Pp. 82.] (May–June number of the *Journal of the Institute of Brewing*.)

It is to be hoped that this reprint will be, in some way or other, made available to a wider circle of readers, both scientific and manufacturing, than it is likely to reach in the journal in which it is published. The connection of Pasteur with brewing is already known, at any rate to biologists, although perhaps not so well to manufacturers. The follower of "pure science," on the one hand, and the entirely "practical man" on the other, will both find much food for thought in the pages of this paper. At the present time, too, it is imperative that all connected with manufacturing should realise the enormous help that a well-trained scientist, with adequate laboratory accommodation, can be to almost any industrial process. The lack of co-operation between the theoretical and pecuniary side of industry in this country is much to be deplored, and perhaps results, as the author suggests, from the fact that most businesses are in the hands of boards of directors. Such a control means only too often that the directors are not interested in the manufacture as a series of skilled operations, but simply as a somewhat complex machine to produce dividends. Any suggested improvement, therefore, must have to support it a plausible possibility of a fairly quick return on the outlay, or

at the very least a stoppage of a present leakage by more adequate testing of the materials employed or utilisation of what are otherwise waste products. It is of course impossible to promise that the employment of a scientific assistant will very quickly increase output or decrease cost of production; hence such an appointment is to be regarded in the nature of a luxury or hobby, but not as a "business proposition." The sooner this short-sighted policy is recognised and abandoned the better for the country. It is, perhaps, obvious that research on brewing would lead to the anatomy and histology of barley, and even, one can see, a remoter connection with the development and physiology of the same plant; but as a matter of fact, the author was led into questions of osmosis, diffusions through multi-perforate diaphragms, semi-permeable membranes, and other problems of an equally physical and mathematical nature. These side lines, branching off from the main subject of brewing, indicate quite clearly how any manufacturing process that is probed thoroughly and scientifically ramifies into a number of sciences often yielding results of scientific importance, or providing a method of attacking problems previously encountered in their theoretical aspects.

The pamphlet is very interesting and one, as was pointed out above, that would do a great deal of good if it were circulated as widely as possible.

C. H. O'D.

Manuring for Higher Crop Production. By E. J. RUSSELL, D.Sc. [Pp. 69, with index and illustrations.] (Cambridge University Press, 1916. Price 3*s.* net.)

ALTHOUGH the immediate object of this book is to assist farmers in so arranging the cropping of their land under the conditions obtaining in war-time as to increase their production for the good of the State, it nevertheless deserves a place—in spite of its small size—on the reference shelves of all who are interested in agricultural matters. It would make illuminating reading for those people, and they are not uncommon, who think poorly of the knowledge and organising ability required by farmers.

The mere fact that the book has of necessity to take into account, over and above its scientific material, certain conditions of price, labour supply and transport, which at present are very variable, whereas in Britain they were formerly (by comparison) immutable, has given it a breadth of treatment which is rare in works on agricultural technology, whilst the most conservative of farmers could not fail to find suggestions on directly practical matters in its pages of concentrated evidence. The limitations imposed on the author have left no room for superfluities, and he has achieved a minimum amount of omission.

The diagrams are simple and clear, while the five chapters of which the book consists deal successively with the improvement of soil, with farmyard and artificial manures, and with the manuring of arable and pasture land.

L. B.

A Voyage in Space. By H. H. TURNER, D.Sc., D.C.S., F.R.S. [Pp. xvi + 299, with over 130 illustrations.] (London: Society for Promoting Christian Knowledge, 1915. Price 6*s.* net.)

THE movement at present on foot to raise science into greater prominence in the school curriculum will no doubt bring with it an awakening desire in our youths to find their amusement as well as their instruction in scientific literature. The

above work is eminently suitable to meet such a demand, for it is a series of six lectures adapted to a juvenile auditory, delivered at the Royal Institution at Christmas 1913, and treating the subject of astronomy in an easy style adequately illustrated. The author has successfully accomplished the difficult task of bringing an abstruse subject down to the comprehension of the juvenile reader, at the same time making it attractive by the introduction of little anecdotes connected with the well-known astronomers. The titles of the lectures, which of themselves describe their contents, are as follows: The Starting Point, Our Earth, The Length of our Voyage and the Start through the Air, Journeying by Telescope, Visits to the Moon and Planets, Visit to the Sun, and Visits to the Stars.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Lectures on Ten British Mathematicians of the Nineteenth Century. By Alexander MacFarlane, late President of the International Association for Promoting the Study of Quaternions. Mathematical Monographs, Edited by Mansfield Merriman and Robert S. Woodward, No. 17. New York: John Wiley & Sons. London: Chapman & Hall, 1916. (Pp. 148.) Price 5*s*. 6*d*. net.

Ruler and Compass. By Hilda P. Hudson, M.A., Sc.D., with Diagrams. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. 143.) Price 6*s*. net.

Eight Lectures on Theoretical Physics Delivered at Columbia University in 1909. By Max Plank, Professor of Theoretical Physics in the University of Berlin, Lecturer in Mathematical Physics in Columbia University for 1909. Translated by A. P. Willis, Professor Mathematical Physics in Columbia University. Publication No. 3 of the Ernest Kempton Adams Fund for Physical Research established December 17, 1904. New York: Columbia University Press, 1915. (Pp. ix + 130.)

Practical Experiments in Heat. By W. St. B. Griffith, B.A., B.Sc., and P. T. Petrie, M.Sc., Assoc. M.Inst. C.E., Assistant Masters of Uppingham School. London: Rivingtons, 34, King Street, Covent Garden, 1916. (Pp. viii + 109.) Price 3*s*. 6*d*. net.

A Text-Book of Quantitative Chemical Analysis. By Alex. Charles Cumming, D.Sc., F.R.S.E., and Sydney Alexander Kay, D.Sc., Lecturers in Chemistry in the University of Edinburgh. Second Edition. London: Gurney & Jackson. Edinburgh: Oliver & Boyd, Tweeddale Court, 1916. (Pp. xv + 402.) Price 9*s*. net.

Tubular Structures in Rocks which are probably due to Osmotic Action. By George Abbott, F.G.S. Reprinted from the "Transactions of the South-Eastern Union of Scientific Societies, 1916." (Pp. 20–23.)

The Hunting Wasps. By J. Henri Fabre. Translated by Alexander Teixeira de Mattos, F.Z.S. London, New York, Toronto: Hodder & Stoughton. (Pp. ix + 393.) Price 6*s*. net.

A Bibliography of British Ornithology, from the Earliest Times to the end of 1912, including Biographical Accounts of the Principal Writers and Biblio-

graphies of their Published Works. By W. H. Mullens, M.A., LL.M., F.L.S, M.B.O.U., and H. Kirke Swann. To be completed in about six bi-monthly Parts. Part III. London: Macmillan & Co., St. Martin's Street, 1916. (Pp. 241 + 384.) Price 6*s*. net.

Records of the Indian Museum. Vol. XII. Part IV., August 1916. With 22 Plates. (Pp. 129–175.) Price 2 rupees. And Vol. XII. Part IX., August 1916. Zoological Results of the Abor Expedition 1911-12. With 48 Plates. (Pp. 543-559.) Price 2 rupees. Published by order of the Trustees of the Indian Museum. Printed at the Baptist Mission Press, 1916.

The Migrations of Fish. By Alexander Meek, M.S., Professor of Zoology, Armstrong College in the University of Durham, and Director of the Dove Marine Laboratory, Cullercoats. With 11 Plates and 128 Figures. London: Edward Arnold, 1916. (Pp. xviii + 142.) Price 16*s*. net.

Arboreal Man. By F. Wood Jones, M.B., D.Sc., Professor of Anatomy in the University of London (London School of Medicine for Women). London: Edward Arnold, 1916. (Pp. x + 230.) Price 8*s*. 6*d*. net.

Form and Function. A Contribution to the History of Animal Morphology. By E. S. Russell, M.A., B.Sc., F.Z.S. With 15 Illustrations. London: John Murray, Albemarle Street, W., 1916. (Pp. ix + 383.) Price 10*s*. 6*d*. net.

A Hausa Botanical Vocabulary. By John M. Dalziel, M.D., B.Sc., D.T.M. West African Medical Staff. London: T. Fisher Unwin, Adelphi Terrace. (Pp. 119.) Price 6*s*. 6*d*. net.

Algæ. Vol. I. Myxophyceæ, Peridinieæ, Bacillaricæ, Chlorophyceæ, together with a Brief Summary of the Occurrence and Distribution of Freshwater Algæ. By G. S. West, M.A., D.Sc., A.R.C.S., F.L.S., Mason Professor of Botany in the University of Birmingham. With 271 Figures in the Text. Cambridge Botanical Handbooks. Edited by A. C. Seward and A. G. Tansley. Cambridge: at the University Press, 1916. (Pp. viii + 475.) Price 25*s*. net.

Proceedings of the Prehistoric Society of East Anglia for 1915-16. Vol. II. Part II. With 59 Figures. London: H. K. Lewis & Co., 136, Gower Street, W.C., 1916. (Pp. 161-325.) Price 3*s*. 6*d*. net.

The Biology of Tumours. By C. Mansell Moullin, M.A., M.D., F.R.C.S., Lt.-Col. R.A.M.C. (T.); Consulting Surgeon to the London Hospital; late Vice-President Royal College of Surgeons; Fellow of Pembroke College and Radcliffe Travelling Fellow, Oxford. London: H. K. Lewis & Co., 136, Gower Street, W.C., 1916. (Pp. 55.) Price 2*s*. 6*d*. net.

The Essentials of Chemical Physiology for the Use of Students. By W. D. Halliburton, M.D., LL.D., F.R.S., Fellow of the Royal College of Physicians, Professor of Physiology in King's College, London. Ninth Edition. With Coloured Plate and 72 Illustrations. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. xi + 324.) Price 6*s*. net.

Diseases of the Throat, Nose and Ear for Practitioners and Students. By W. G. Porter, M.B., B.Sc., F.R.C.S. Second Edition, fully revised for the Author during his absence from England in the Service of his Country by P. McBride, M.D., F.R.C.P., F.R.S.E. With 77 Illustrations. Bristol: John Wright & Sons, Ltd. London: Simpkin, Marshall, Hamilton, Kent & Co. Toronto: The Macmillan Co. of Canada, 1916. (Pp. xvi + 280.) Price 7*s*. 6*d*. net.

Dynamics. Part I. By R. C. Fawdry, M.A., B.Sc., Head of the Military and Engineering Side, Clifton College. London: G. Bell & Sons, 1916. (Pp. viii + 177 + ix.)

Raphael Meldola, Reminiscences by those who knew him, and a Chronological List of his Publications, with a Preface by the Rt. Hon. Lord Moulton. London: Williams & Norgate. (Pp. xv + 225.) Price 5*s*. net.

The South African Journal of Science. Comprising the Report of the South African Association for the Advancement of Science, 1915. Pretoria. Issued Monthly. Vol. XII. No. 1, August 1915; No. 2, September 1915; No. 7, February 1916, and No. 10, May 1916. Cape Town: Published by the Association, 1915 and 1916. Price 2*s*. net.

Science from an Easy Chair. By Sir Ray Lankester, K.C.B., F.R.S. With 66 Illustrations. London: Methuen & Co., 36, Essex Street, W.C. (Pp. xii + 292.) Price 1*s*. net.

The Right Honourable Sir Henry Enfield Roscoe, P.C., D.C.L., F.R.S. A Biographical Sketch by Sir Edward Thorpe, C.B., F.R.S. With Frontispiece. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. viii + 208.) Price 7*s*. 6*d*. net.

Truth and the War. By E. D. Morel. London: at the National Labour Press, Ltd., 1916. (Pp. xxiii + 320.) Price 2*s*. net.

William Oughtred. A Great Seventeenth-Century Teacher of Mathematics. By Florian Cajori, Ph.D., Professor of Mathematics, Colorado College. Chicago and London: The Open Court Publishing Co., 1916. (Pp. vi + 100.) Price 4*s*. net.

ANNOUNCEMENTS

ROYAL ASTRONOMICAL SOCIETY. Meetings, 5 p.m., January 12, February 9.

ROYAL METEOROLOGICAL SOCIETY. Annual General Meeting, January 17.

CHEMICAL SOCIETY. Meetings, 8 p.m., January 11, 18 (Lecture by C. T. Heycock), February 1, 15, March 1, 15 (Lecture by Horace T. Brown); Annual General Meeting, 4 p.m., March 29.

INSTITUTION OF MECHANICAL ENGINEERS. Meetings, 6 p.m., January 19, March 16; Annual General Meeting, February 16.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

POLYMORPHISM

By F. D. CHATTAWAY, F.R.S.

When matter passes from the gaseous or liquid to the solid state it generally assumes a definite geometric form, this being the external mark of the crystalline condition, that is, a condition in which the molecules are assumed not to be distributed in space at random, but to have an ordered arrangement.

Towards the end of the eighteenth century Haüy, as a result of his work upon crystals, came to the conclusion that each individual substance has an invariable crystalline form, peculiar to itself. For many years this conclusion was accepted without question until Mitscherlich proved that certain substances were capable of assuming two entirely distinct crystalline forms. This phenomenon was designated by him " dimorphism," but since it is now known that the number of forms is not limited the more comprehensive term " polymorphism " is generally used.

This discovery was made by Mitscherlich in 1821 when he was investigating the arsenates and phosphates of the alkali metals and of ammonium in Berzelius' laboratory at Stockholm. In the course of his work Mitscherlich noticed that sodium dihydrogen phosphate, $NaH_2PO_4 . H_2O$, did not crystallise in the same form as the similarly constituted sodium dihydrogen arsenate, $NaH_2AsO_4 . H_2O$, as he expected from his recently discovered Law of Isomorphism.

Thinking that this might be the result of some impurity he repeated the crystallisation several times, getting on some occasions crystals of the same form as before, but on others crystals with angles almost identical with those of the arsenate. He found that these latter crystals on solution or recrystallisation would often give him the form which differed from that of the arsenate.

It is curious that no one had previously reached the same conclusion as Mitscherlich, for various similar cases had been known for some considerable time, as, for example, the much-discussed one of calcite and aragonite. These, however, were

minerals which were never quite pure and could not be obtained artificially, while Mitscherlich, starting from the same pure material, was able to obtain either form of the sodium dihydrogen phosphate.

Two years later, in 1823, he found in sulphur another instance of dimorphism and showed that the element separates from a solution in carbon bisulphide in orthorhombic-bi-pyramids, from the state of fusion in monoclinic prisms.

The next advance was the discovery that the temperature at which the crystals are produced frequently determines the appearance of one or other modification of a polymorphic substance. This was first recognised by Mitscherlich in the case of mercuric iodide. He showed that the alteration of the red into the yellow form on heating was accompanied by a change in crystalline form from tetragonal to orthorhombic.

It is worth recording that Brodie in 1854 first observed in the case of sulphur that polymorphic forms may have different melting-points.

During the next few decades many new instances of polymorphism were recorded among inorganic substances, but it was a long time before similar examples were discovered among organic compounds.

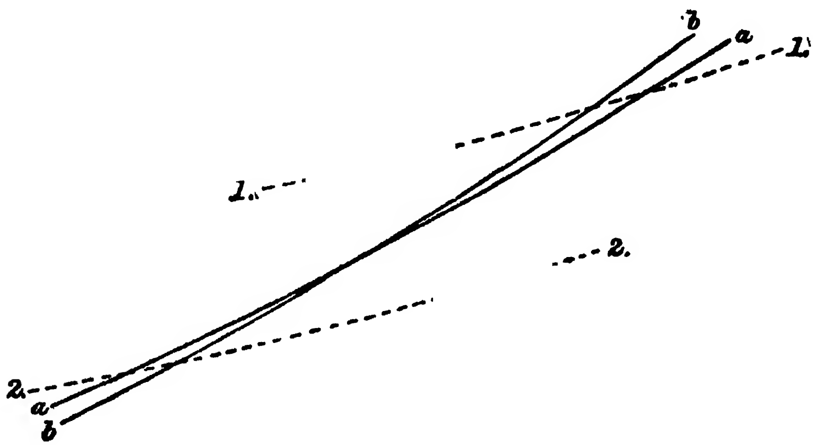
This extension was made by Jungfleisch, who obtained two modifications of 1:2:4-chlorodinitrobenzene, one melting at 50°, the second of a different crystalline habit melting at 43°. The crystals of the latter could be kept unaltered in a closed vessel, but on being touched with a crystal of the form melting at 50° they changed into this, becoming opaque like wax.

Benzophenone was soon afterwards added to the list by Zincke. Chancel, who first prepared this compound in 1849 by heating calcium benzoate, obtained it in orthorhombic crystals melting at 46°. Zincke by oxidising diphenylmethane obtained it in monoclinic crystals melting at 26°. The latter form was unstable with respect to the former at all temperatures and changed into the higher-melting variety when warmed or touched with a crystal of it.

Other examples were soon discovered, and from a study of all then known Lehmann in 1877 was able to group them in two classes. He recognised more clearly than had hitherto been done that whilst in some pairs of polymorphs one form was unstable in contact with the second at all temperatures, in

others the relative stability altered as a certain temperature (the transition temperature) was passed through. The former class he termed monotropic, the latter enantiotropic.

Lehmann further recognised that the transition point of enantiotropic polymorphs could be overstepped in either direction, since the rate of change diminishes as that point is approached. As the temperature falls below this, however, the rate of change soon reaches a maximum value, after which a diminution follows which can end in an apparently complete



Temperature

FIG. 1.

cessation of transformation. This he connected with a diminution in the molecular mobility at the lower temperature.

The explanation of monotropy and enantiotropy generally accepted at the present time was given by Ostwald. He regarded both as due to the same cause, and concluded that in enantiotropic modifications the melting points were above the temperature of transformation and in monotropic modifications below it, one modification being, therefore, always unstable, since its region of stability lies above its melting-point, and so cannot under ordinary conditions be realised. This is expressed graphically in fig. 1.

Of the two crystalline modifications the more stable of the two at any temperature must have the lower vapour pressure, for if this were not so, on placing the stable and labile forms at the two ends of a sealed tube, the stable, since it possessed the higher vapour pressure, would distil over and the unstable variety would grow at its expense, which is contradicted by all experience.

In the figure the continuous lines *a*, *a* and *b*, *b* represent the vapour pressure curves of the polymorphic solid forms and the dotted lines 1, 1 and 2, 2 the vapour pressure curves of the liquid in the two cases.

The statement that in monotropic pairs of compounds the melting-point is below the temperature of transformation is misleading, since such a transformation could only be referred to change within individual molecules or to a breaking down of complex aggregates, and such changes according to current views would be classed as isomeric or polymeric rather than polymorphic. What is really intended is, of course, to express the opinion that in all cases of monotropy the ratio between the vapour pressure of the unstable variety and that of the stable variety progressively diminishes as the temperature approaches the melting-point. Ostwald in this explanation apparently overlooks the possibility of cases where the transition points lie much below the ordinary temperature and thus cannot be realised or experimentally determined except approximately by extrapolation on account of the slow rate of change at the low temperature. It is probable, therefore, that many substances are classified as monotropic though potentially enantiotropic.

In a course of lectures delivered in 1897–8, van't Hoff put forward certain theoretical deductions relating to the reversible transformation of polymorphic forms. He bases his conclusions on the assumption that polymorphic differences are due to variations in molecular grouping, which disappear when the substances pass into the gaseous or liquid state or into solution. He shows that the stable modification must have the smaller vapour pressure, the smaller solubility, and the higher melting-point; that a reversible polymorphic transformation must proceed to completion at one temperature, the direction of the change depending on the direction in which the temperature is altered, and that the modification stable at low

p-Bromoacetanilide

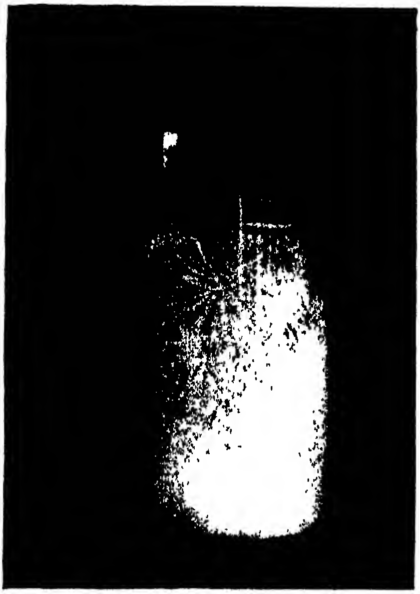


FIG. 1.

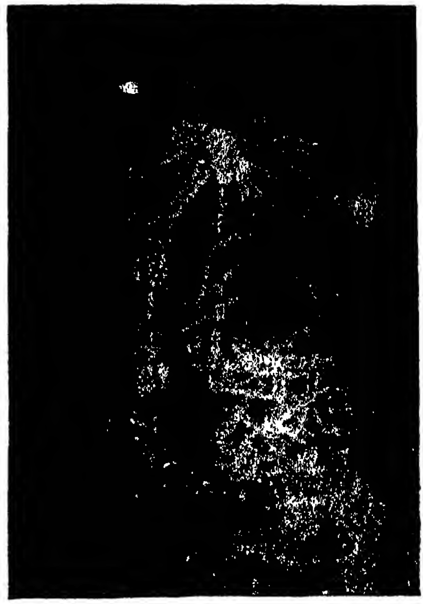


FIG. 2.

FIG. 3.

FIG. 4.

Crystallisation of *p*-bromoacetanilide.

Photographs taken on successive days.

temperature must be formed from the other with evolution of heat; and, finally, that polymorphic modifications must have a constant ratio of solubilities in solvents which take up so little of the substances that the laws of dilute solutions are applicable.

It has only recently been possible to put several of these deductions to the test of experiment on account of the lack of suitable material. Such material has, however, been found in various anilides and hydrazides. Para-bromoacetanilide and 2:4-dibromoacetanilide, both easily obtained by the bromination of acetanilide, are perhaps the most easily accessible examples of monotropic polymorphs and show excellently the different solubilities of polymorphic forms.

When a hot saturated alcoholic solution of *p*-bromoacetanilide or of 2:4-dibromoacetanilide is allowed to cool, needle-shaped crystals of the unstable form, generally of considerable length, separate. These long, slender crystals, which form an interlaced mass, if left in the solvent at the ordinary temperature slowly redissolve, whilst crystals of a second form much more compact in habit make their appearance. Crystals of this compact form generally appear spontaneously, but if none appear after some hours, a few may be added. The appearance (Plates I. and II.) during the transformation is very characteristic. In the mass of interlaced needles, which if a concentrated solution is cooled completely fills the liquid, cavities make their appearance, each containing a crystal or group of crystals of the compact form. These rest on the needles below and gradually sink, producing irregular, worm-like tubes. The whole mass becomes filled with such holes and tubes and crumbles away, whilst the compact crystals subside into a comparatively thin layer at the bottom.

The photographs reproduced in Plate I. illustrate the gradual transformation of the unstable into the stable modification of *p*-bromoacetanilide. Fig. 1 shows a beaker, about half its actual size, filled with crystals of the unstable form immediately after crystallisation has taken place. Fig. 4 shows the same beaker, similarly reduced in size, after complete transformation of the unstable into the stable form. Figs. 2 and 3 show a part of the same beaker, about the actual size, on successive days during the progress of the transformation.

A similar series of reproductions on the same scale of photo-

graphs illustrating the transformation of the unstable into the stable form of 2 : 4-dibromoacetanilide is given in Plate II.

If small quantities of the needle-shaped form of either compound are moistened with alcohol, sealed up in a tube and plunged into a heated oil-bath, transformation in each case is complete in a few minutes at 100° and just below the melting-points of the compounds is so rapid as to appear instantaneous.

The melting-points obtained, therefore, are those of the stable forms, since in consequence of the rapid change the unstable forms completely disappear before fusion occurs.

Although transformation of these unstable forms occurs so easily, it has been found possible to shake them sufficiently long with alcohol at moderate temperatures to obtain saturated solutions and hence to determine their solubilities.

The results show excellently how solubility depends upon the exact nature of the solid present and afford a good illustration of van't Hoff's generalisation that the stable modification of any pair of polymorphs must have the smaller solubility.

The following tables give the solubilities of the two forms of *p*-bromoacetanilide and of 2 : 4-dibromoacetanilide in ethyl alcohol, the numbers recording the weight of anilide in grams in 100 grams of solution.

p-*Bromoacetanilide*

Temperature.	Needle form.	Compact form.	Difference.
5°	4·569	4·244	0·325
10°	5·231	4·847	0·384
15°	5·981	5·561	0·420
20°	6·945	6·390	0·555

2 : 4-*Dibromoacetanilide*

Temperature.	Needle form.	Compact form.	Difference.
5°	3·062	2·480	0·582
10°	3·535	2·876	0·659
15°	4·049	3·338	0·711
20°	4·804	4·002	0·802

The solubilities were determined by shaking in a constant-temperature bath excess of the anilides with the solvent for a number of hours sufficient to ensure saturation. A sufficient quantity of the saturated solution was then taken out, weighed, and the solvent removed by evaporation. The weights of solute and solvent were thus obtained.

Between 0° and the melting-point the compact form both of

p-bromo and of 2 : 4 dibromoacetanilide is the stable form; the compounds therefore do not show a transition point between these limits.

The number of enantiotropic organic compounds is not large and it is only recently that the discovery of two suitable examples in phthalylphenylhydrazide and phthalylmethylphenylhydrazide have enabled several of van't Hoff's conclusions for the first time to be put to the test.

These compounds,

$$C_6H_4\Big\langle{CO \atop CO}\Big\rangle N-N\Big\langle{H \atop C_6H_5} \quad \text{and} \quad C_6H_4\Big\langle{CO \atop CO}\Big\rangle N-N\Big\langle{CH_3 \atop C_6H_5}$$

are easily obtained by heating together at about 100° equivalent amounts of phthalic anhydride and the corresponding hydrazine when water is eliminated. The practical determination of transition points depends in general on—

(*a*) Obvious changes in crystalline form or colour as one modification transforms into the other.

(*b*) The change in volume to be observed when one modification changes into the other.

(*c*) The difference in vapour pressure—or solution pressure—shown by the two modifications.

(*d*) The difference in energy content shown by evolution or absorption of heat when transformation takes place.

The properties available, in the case of the phthalylhydrazides, for the determination of their transition points are the relative solubilities, the colour and crystalline form, which are markedly different in the pairs of enantiomers, and, in one case, the volume change which accompanies transformation.

Phthalylphenylhydrazide crystallises in bright yellow monoclinic crystals of short and compact habit and in very pale yellow orthorhombic plates. The pale yellow form when heated on a water bath for a short time in presence of a small quantity of any solvent transforms completely into the bright yellow form, which is consequently easily obtained pure. The pale yellow form is less readily obtained quite free from its enantiomer. This can be effected, however, by boiling a concentrated solution in alcohol sufficiently long to remove all nuclei and then cooling rapidly. Some time as a rule elapses before crystallisation begins, but on shaking till crystals appear the pale yellow form usually separates free from the other in small almost colourless

plates. A pure crop does not always result at the first attempt, but the crystals of the deep yelllow form, if any are present, show up very clearly against the almost colourless ones, and can be at once detected with a lens.

The solubilities of the two forms in alcohol are given in the subjoined table as the number of grams of the compound contained in 100 grams of the saturated solution.

Solubility of the Enantiotropic Forms of Phthalylphenylhydrazide in Ethyl Alcohol

Temperature.	Pale yellow form.	Difference.	Deep yellow form.	Ratio.
5°	0·428	−0·010	0·438	0·977
10°	0·517	−0·001	0·518	0·998
15°	0·614	0·012	0·602	1·020
20°	0·749	0·019	0·730	1·026
25°	0·913	0·031	0·883	1·034
30°	1·098	0·043	1·055	1·040
35°	1·313	0·055	1·257	1·045

The solubility curves given in fig. 1 show that the transition point lies in the neighbourhood of 10°.

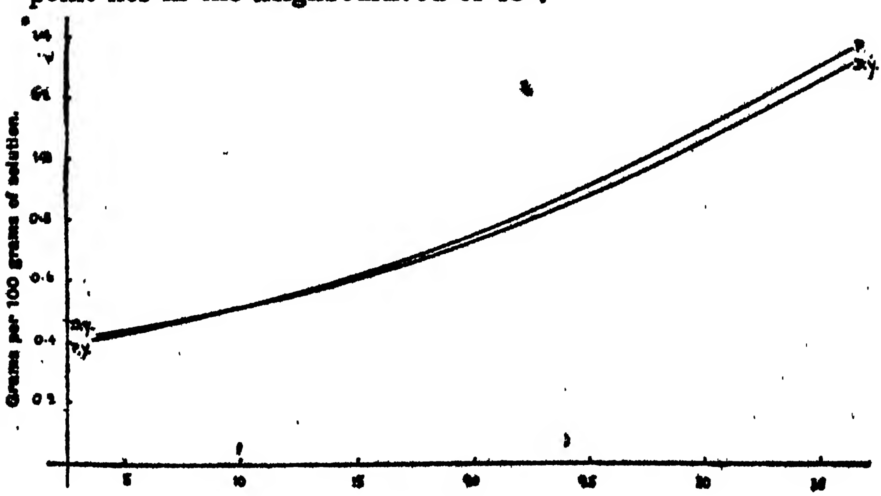


Fig. 2.—Phthalylphenylhydrazide in alcoholic solution.
P.Y. = pale yellow form. D.Y. = deep yellow form.

At this temperature, however, phthalylphenylhydrazide is so sparingly soluble in alcohol that the difference in solubility of the two forms is of the same order of magnitude as the experimental error. The transition point could not, therefore, be determined exactly, using this solvent.

PLATE II

2 : 4-Dibromoacetanilide

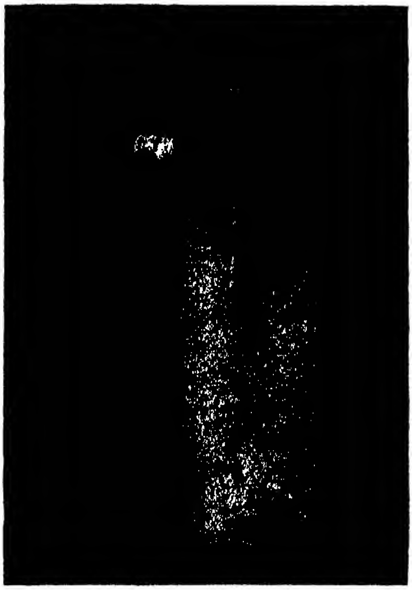


FIG. 5.

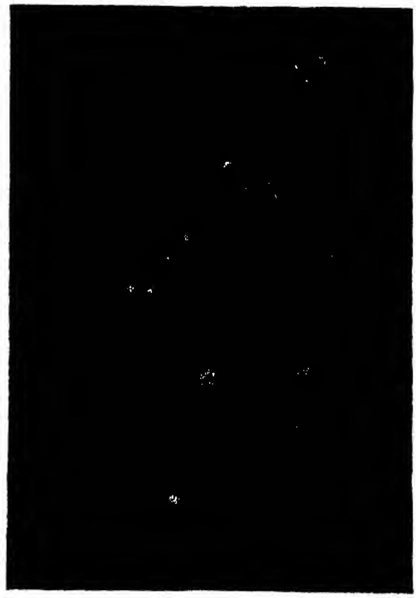


FIG. 6.

FIG. 7.

FIG. 8.

Crystallisation of 2 : 4-dibromoacetanilide.

Photographs taken at intervals of two days.

The solubilities in chloroform, in which both forms dissolve much more freely, were therefore determined at temperatures in the neighbourhood of 10°.

Solubility of the Enantiotropic Forms of Phthalylphenylhydrazide in Chloroform

Temperature.	Pale yellow form.	Difference.	Deep yellow form.	Ratio.
5°	2·864	−0·030	2·894	0·9896
8°	3·034	−0·013	3·047	0·9957
9°	3·097	−0·007	3·104	0·9977
10°	3·158	0·010	3·148	1·0031
11°	3·220	0·013	3·207	1·0040
15°	3·494	0·045	3·449	1·0130
25°	4·484	0·160	4·324	1·0370

The solubility curves are given in fig. 2. By plotting on a large scale and intrapolating, it is seen that the transition point lies a little above 9·4°.

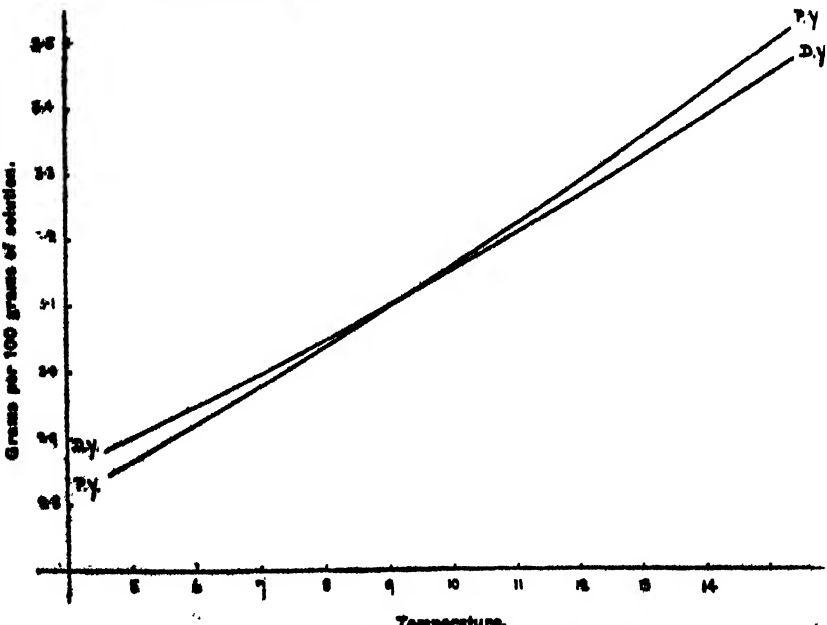


FIG. 3.—Phthalylphenylhydrazide in chloroform solution.
P.Y. = pale yellow form. D.Y. = deep yellow form.

The extremely slow rate of transformation near the transition point and the very slight difference in density of the two forms did not allow the observational or dilatometric methods of determining the transition point to be employed.

Phthalylphenylmethylhydrazide affords the best example of dimorphism yet met with among compounds of this class, as each form can be easily transformed into the other. It exists in two modifications: the one stable at the higher temperature crystallises in bright orange-coloured triclinic prisms, the other stable at the lower temperature in monoclinic crystals of very compact habit. There is no difficulty in obtaining either form. When a boiling saturated alcoholic solution is allowed to cool, the orange-coloured, triclinic modification separates, and when collected and dried at 100° is obtained pure. If this modification is allowed to remain suspended in alcohol for some days, pale yellow crystals of the monoclinic modification make their appearance and grow at the expense of the orange crystals, which become etched, dissolve away, and finally disappear.

The solubilities of the two forms in alcohol were determined by the method previously described, and are recorded, as before, as the number of grams of the compound contained in 100 grams of the saturated solution.

Solubility of the Enantiotropic Forms of Phthalylphenylmethylhydrazide in Ethyl Alcohol

Temperature.	Pale yellow form.	Difference.	Orange-coloured form.	Ratio.
5°	0·705	0·208	0·913	·295
10°	0·819	0·210	1·029	·256
15°	0·999	0·214	1·213	·214
20°	1·302	0·221	1·523	·170
25°	1·697	0·234	1·931	·138
30°	2·239	0·235	2·474	·105
35°	2·922	0·283	3·205	·079

It was not practicable at higher temperatures to shake the orange-coloured form with the alcohol sufficiently long to ensure saturation without some transformation into the pale yellow form occurring. The transition point could not therefore be fixed with exactitude by solubility determinations, but by plotting the ratio of solubilities against the temperature and extrapolating, it was seen that it must lie in the neighbourhood of 55°.

Two other methods, however, which gave results correct within very narrow limits were available in the case of this compound.

The two modifications are very obviously different in colour and general appearance, and transformation in acetone is suffi-

ciently rapid, even in the immediate neighbourhood of the transition point, for this to be determined by observing the growth of one form at the expense of the other.

When a large crystal of either form is growing, its edges are sharp and distinct, but when it is dissolving they become ill-defined, and the whole crystal takes on a cloudy, etched appearance.

Tubes containing a mixture of crystals, of moderate size, of the two forms, just covered with acetone, were sealed up and heated in a bath of liquid paraffin. Starting from about 54°, the temperature was raised by tenths of a degree, the bath being kept constant for many hours at each temperature, and individual selected crystals from time to time examined.

At 55° the pale yellow crystals, and at 55·5° the orange crystals, were unmistakably growing, although very slowly. Between these temperatures no definite alteration of either form could be observed. The transition point must therefore be between these temperatures, and lie in the neighbourhood of 55·25°.

The difference in density of the two modifications allowed a dilatometric method also to be used. This was found to be very sensitive, although the rate of transformation near the transition point is slow.

A dilatometer, packed with a mixture of fairly coarse crystals of the two forms, and filled to a suitable height with a saturated solution of the compound in acetone, was placed in a constant-temperature bath adjusted to about 55°, and the capillary tube sealed off. A thermometer wired on to this tube gave the temperature of the bath and provided a scale for reading the height of the column of liquid.

Experiments extending over a considerable period, during which the limits of temperature where opposite changes in volume occurred were narrowed, showed that the volume diminished at 55° and increased at 55·2°, whilst there was no appreciable alteration at 55·1°, which may therefore be taken as the transition point.

By means of phthalylphenylhydrazide van't Hoff's deduction that the ratio of the solubilities of enantiomers at a given temperature is independent of the solvent has also been put for the first time to the test. It is a substance very well suited for the purpose, as it is easy to obtain in the

two forms and has widely differing solubilities in different solvents.

The following table gives the solubilities of the two forms of this compound in five different solvents at 25°, the temperature furthest removed from the transition point at which a complete series of determinations was made:

Solvent.	Solubility of pale yellow modification.	Solubility of deep yellow modification.	Ratio.
Benzene . . .	0·910	0·882	1·032
Alcohol . . .	0·913	0·883	1·034
Chloroform . .	4·484	4·324	1·037
Ethyl acetate . .	4·654	4·489	1·037
Acetone . . .	10·060	9·693	1·038

The agreement between the ratios is remarkably close.

Phthalylphenylmethylhydrazide is not so suitable for the purpose, on account of its greater solubility. A similar confirmation was found, however, on comparing the ratios of the solubilities of the two forms in ethyl and in methyl alcohol at 30° C.

Solvent.	——— of pale yellow modification.	Solubility of orange modification.	Ratio.
Ethyl alcohol . .	2·239	2·474	1·105
Methyl alcohol . .	4·547	5·033	1·107

It ought, perhaps, to be pointed out that the solubilities given are not concentrations, but the densities of the corresponding pairs of solutions do not differ sufficiently to affect materially the ratios.

From the first recognition of polymorphism attempts have been made to gain an insight into its nature and to relate it with the phenomena of isomerism. Although no strict line can be drawn between isomeric, polymeric, and polymorphic changes, the first two are almost universally regarded as being due to changes within the chemical molecule, the last are regarded as due to change of molecular grouping within the crystal.

That some change in the relationship of the atoms within the molecule occurs in both isomeric change and polymorphic change is certain; the difference appears to be as to whether this is sufficiently great to cause the molecules to react differently or not. In the former case the change is designated isomeric, in the latter polymorphic.

A few years ago Smits put forward a new theory as to the nature of polymorphic modifications. He assumes that all substances which exist in two polymorphic forms contain when in the liquid state two kinds of chemical molecules which we may call x and y in equilibrium. When the liquid crystallises, mixed crystals are formed still containing the two kinds of molecules, but in proportions differing from those previously existing in the liquid. On lowering the temperature the proportions alter both in the crystals and in the liquid until we arrive at a point—the transition point—at which the crystal structure will not allow of any further increase of x at the expense of y; another kind of crystal structure therefore makes its appearance which will allow of a further proportion of x.

Polymorphic modifications are therefore space lattices composed of the two different kinds of molecules, each interpenetrated by the other, the proportion of the one not forming the space lattice varying with the temperature.

The temperature of transition is that at which the proportion of the one not forming the space lattice takes upon itself the formative function and produces the space lattice, the molecules of the old space lattice now interpenetrating the new one.

The theory, however, is not supported by much experimental evidence and has not received any wide acceptance. Attempts have been made from time to time to lay down rules for distinguishing between polymorphic and isomeric substances. They have generally been empiric and far from satisfactory. Sidgwick has recently suggested a method free from objection depending on the solubility.

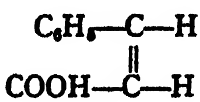
If to a saturated solution of the more soluble of two polymorphic modifications some of the other modification is added, the concentration of the solution will not increase, since the two forms give the same molecule in solution; on the contrary it will diminish to a greater or less extent, depending on the difference in solubility of the two forms, although this difference may be too small to measure. If, on the other hand, the two forms are isomeric or tautomeric, the addition of the less soluble form will cause an increase in the concentration of the solution, for chemically different molecules do not seriously influence each other's solubility. As a measure of the concentration of the solution, Sidgwick determines the cryohydric point, using an ordinary Beckmann apparatus. The cryohydric

points are determined in some suitable solvent for the two forms separately and then for the two forms together. If polymorphic, the mixture of the two forms will give a point lying between those of the separate forms; but if isomeric (or tautomeric) the point given by the mixture will lie below that of the more soluble form, the depression of freezing-point by the two together approximating to the sum of the individual depressions.

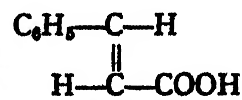
The method has the great advantage that as it takes only a few minutes to carry out at a low temperature, the probability of the two forms (if indeed they are not polymorphic) coming to tautomeric equilibrium in the course of the experiment is greatly diminished.

It seems likely that many supposed cases of isomerism will prove on close examination to be instances of polymorphism. A recent example of this is worth recalling.

According to current views on configuration two cinnamic acids of the formula $C_6H_5CH : CH . COOH$ should exist



Cis.



Trans.

but at least four have been described.

The best known is the acid whose aldehyde occurs in oil of cinnamon and which is found in various balsams. It is generally made by a synthetic method discovered by Sir William Perkin: benzaldehyde is heated with sodium acetate in presence of acetic anhydride. This acid crystallises in needles or prisms, melts at 133°, and boils at 300°.

Three other acids of identical composition and chemical properties melting at 68°, 58°, and 42° respectively have been obtained from coca leaves and by various other methods, including the partial reduction of phenylpropiolic acid. They must therefore have the cis. configuration. It follows that ordinary cinnamic acid has the trans. configuration.

Many fruitless attempts have been made to devise formulæ to explain the existence of these three low melting acids. They have recently been shown to be simply polymorphic forms of the cis. acid.

By melting either of the three acids identical fusions are obtained and the introduction of a crystal fragment of any

particular modification into the super-cooled fusion always leads to a complete solidification into that modification. In this way, then, any one of the three acids can be transformed into either of the remaining two. Transformation of the two lower melting acids into that of higher melting-point may also be effected by inoculation without previous melting, the progress of transformation being visible by the growing opacity of the clear crystals or under the polarising microscope by the change of interference. The acid melting at 42° may be directly transformed into the acid melting at 68° or indirectly by changing it into the acid melting at 58°, and subsequently transforming this so that the 68° acid may be regarded as stable, the 58° as metastable, and the 42° labile.

The three acids are therefore to be regarded as polymorphic forms of the trimorphic cis. cinnamic acid.

When a polymorphic compound, monotropic or enantiotropic, separates from solution, the unstable and not the stable form almost invariably appears and subsequently transforms more or less rapidly into the stable form. The frequency with which such unstable forms appear when substances of the most diverse compositions separate from solution leads to the conclusion that polymorphy is a general property of crystalline matter as universal as condensation and freezing. It may be stated generally, therefore, that matter is capable of existing in the gaseous, the liquid, and in various polymorphic crystalline states, and it is safe to predict that unstable forms of most crystalline solids could be obtained if they were sufficiently closely studied during crystallisation under different conditions.

SALT AND THE AGE OF THE EARTH.

By G. W. BULMAN, M.A., B.Sc.

In the able hands of Prof. Joly the salt of the ocean has been made to give an answer to the question of the age of the earth. The general idea of it is perfectly simple, though the difficulties of working it out in detail are immense. Salt is being carried down year by year into the sea where it accumulates. If then—assuming an original saltless ocean—we divide the amount of salt in the ocean by the amount carried in every year, the quotient will be the age of the earth. Prof. Joly works it out to 99 million years, which he afterwards, making allowances for possible sources of error, reduces to 90 millions.

Two special comments suggest themselves.

One is on a matter of fact regarding the actual occurrence of salt in the geological sequence of strata. The other touches on the underlying and necessary assumption that the primeval ocean was practically saltless.

Salt occurs in the strata of various geological ages. In our own country great deposits occur in the Trias. In Germany immense quantities—including the famous potassium and magnesium salts of Stassfurt—are found in the Permian. The noted salt mines of Wieliczka are of Miocene age. In India in the Salt Range, " It occurs in solid cliffs, which for extent and purity are stated to have no rival elsewhere in the world." The Indian salt is of Devonian age. In the Silurian of North America, again, deposits of salt 14 to 40 feet thick occur.

Now if we compare in a broad and general way these salt deposits from the Silurian to the Miocene it is impossible to suggest that the Tertiary oceans were *salter* than the Primary, as they ought to have been. The seas which could give us the salt beds of the Salina group of North America, and those of the Indian Salt Range must have contained—one suggests—*at least* as much salt as those of to-day, or of the Miocene which gave the Polish deposits. Nor can we think that our Triassic salt deposits, or the German Permian, came from oceans richer

in salt than those Devonian seas which yielded the salt of the Salt Range.

And the general aspect of the marine faunas of the various periods do not indicate any gradual increase in the saltness of the seas.[4] The general facies of the Cretaceous fauna, in its foraminifera, sea urchins, corals, molluscs, etc., is so like that of the present ocean that it seems to require a sea *not less salt*. Had the original ocean been saltless, should we not have expected to find an increasing *fresh-water* aspect in the fauna as we went back in time? But there is even a suggestion that the ocean has been growing *less salt*. Consider the Mollusca. Brachiopods, cephalopods, pteropods—which are exclusively marine—have *diminished* in comparison with the lamellibranchs and gasteropods—which are also fresh-water forms.

And if, as Prof. Joly suggests, the rocks of the earth are having their sodium contents washed out continually, the newer formed deposits should have less than the older.

Thus a river which is cutting its way through *Triassic* rocks may be dealing with matter which has had its sodium subject to a like action, say in Carboniferous times. The rivers of to-day must be bringing down *less* sodium than those of the past. Is this possibly why the salmon requires to go to the sea? The river having become *too fresh* for it, the salmon must go for the necessary saltness to the ocean. The eel, also, may have found it impossible to complete its life history in the river's growing scarcity of salt.

As regards the assumption that the original ocean was saltless, there is a serious objection. When the lowering of the temperature permitted the water to condense, and collect in the ocean hollows, it would *dissolve* the salt over the whole area of its bed. Thus from the very first it would have a not inconsiderable amount of salt.

Perhaps the most interesting point about the salt estimate of the age of the earth is, that it is rather of the order of Lord Kelvin's modest suggestion than of the somewhat sensational figures given by the radium method. But while Lord Kelvin's period dated from the first consolidation of the earth, Prof. Joly's, dating from the first condensation of water in the ocean, would be a little later in beginning.

OSMOTIC PRESSURE IN ANIMALS AND PLANTS

By W. R. G. ATKINS, M.A., Sc.D., F.I.C.,
Assistant to the University Professor of Botany, Trinity College, Dublin

A COMPARATIVE study of the osmotic pressures existing in animal and plant cells reveals at once the great difference in the conditions under which these two great divisions of living matter have developed. And as in their morphology the most primitive of both groups have diverged but little, so also in their relations with the external medium close similarity may be observed. Again the ontogeny of each species is an abbreviated recapitulation of its phylogeny as far as morphology is concerned. The same is no less true of its physiology, of which the osmotic pressure is an expression.

The nature of osmotic pressure need not be considered here, as indeed much controversy still rages concerning its manner of action. It may be remarked, however, that it can be regarded as a hydrostatic pressure exerted on membranes which limit the diffusion of matter in the crystalloid condition when dissolved in a solvent to which the membrane is permeable. Further, its magnitude depends upon the number of molecules of a non-electrolyte or of molecules and ions of an electrolyte in a given volume of solvent, or more accurately in a given weight of solvent. In this and in its temperature coefficient the osmotic pressure of a dilute solution obeys the gas laws with some degree of accuracy. For concentrated solutions, however, a more complicated equation has been devised, and the gas law equation is a simplified form of this.

Since, therefore, the osmotic pressure of a solution depends upon the total number of ions and molecules in a definite weight of solvent, it is evident that the pressure in a living cell is regulated by the activity of the protoplasm in synthesising colloidal matter from crystalloids and in splitting up colloids again to form crystalloids, as well as by the entrance of external ions

or molecules, that entrance itself being regulated by the alterations in permeability of the protoplasmic membrane. Changes in osmotic pressure are therefore an expression of the net result of cell activities in altering the total number of molecules and ions at large in the water it contains.

Measurements of osmotic pressure may be made directly by measuring the hydrostatic pressure developed inside a semi-permeable membrane in contact with the solvent on the other side, or they may be made indirectly by measurements of freezing-point, boiling-point, or vapour pressure of the solution. The connection is evident when one considers that all these constants depend upon the molecular concentration of the solution, and that the same amount of work must be done to change the molecular concentration—and consequently the vapour pressure—of a solution by a definite quantity, quite irrespective of the means by which the alteration is effected. It is therefore immaterial whether the concentration is carried out by forcing a semi-permeable piston through the solution or by separation of the solvent by freezing it out, or by boiling it away.

In physiological investigations two methods have been employed in the main, that of plasmolysis—the balancing of the internal osmotic pressure of the cell against the external osmotic pressure of a non-poisonous solution of electrolytes or non-electrolytes—and that of cryoscopy—the determination of the freezing-point of the expressed contents of the cell.

In the plasmolytic method of determining the osmotic pressure of a plant cell a series of solutions of different concentrations are brought into contact with the cell, until one is found which just forces back the protoplasm from the cellulose wall against which it had been pressed by the osmotic pressure, a hydrostatic pressure, of the cell sap in the vacuole. The osmotic pressure of the sap is then taken to be equal to that of this solution. In reality this gives a slightly high value, as the external solution at first causes a shrinkage of the cellulose walls to their normal dimensions, for they are somewhat distended by the internal pressure.

There are also a number of other sources of error due to the fact that the protoplasmic membrane is but rarely completely impermeable to the solute in the external medium, that at different times this permeability may vary, and that the external

medium itself may bring about changes in the permeability. Many of these are reversible and can be repeated over and over again without causing any injury to the cell. The work of Osterhout upon selective permeability, true and false plasmolysis, and the quantitative measurement of the antagonistic action of different ions is of especial interest in this connection. It is, however, too large a subject to be discussed in any detail here.

For these reasons the cryoscopic method is to be preferred when sufficient quantities of the tissue fluid can be obtained unaltered in composition, and when the latter does not undergo any appreciable change during the interval that elapses between expression and the actual determination of freezing-point.

It has already been mentioned that in plasmolysis the protoplasmic layer is forced back from the cellulose wall. Under normal conditions it is this cellulose wall that takes up the hydrostatic pressure or osmotic pressure of the cell sap and sets a limit to the distention of the protoplasm. Without this support the latter would expand owing to the intake of water through it into the vacuole, and would blow out like a soap bubble until it met with the usual fate of bubbles. The analogy is not at all a superficial one, for in each case the enlargement is resisted by the force required to do work, in increasing the surface, against the surface tension of the soap-film in contact with air or of the protoplasm in contact with the external solution. It is precisely the possession of a cellulose wall setting a limit to expansion that constitutes such a profound distinction between plants and animals. For animal cells constancy of the osmotic pressure of the external medium and of the internal fluids is of paramount importance. For plant cells such constancy is quite unnecessary provided always that the osmotic pressure of the external medium is not raised above that of the cell sap to too great an extent or too rapidly.

Consider, for instance, the fate of a protozoan placed in a hypotonic medium; water is absorbed continually till the cell bursts. The same result might be effected by the accumulation of urea or other katabolic products by increasing the internal osmotic pressure, and so causing a relative change, hence the need for an elementary nephric system, such as a contractile vacuole as has been pointed out by Dixon.

Osmotic Pressure in Primitive Organisms

Since the most primitive forms of life developed in water and were devoid of a firm continuous limiting membrane, it is obvious that they must have possessed a very small osmotic pressure. For either they developed in fresh water—in which case their pressure could not have greatly exceeded 0·25 atmosphere, that of moderately hard water freezing at $-0{\cdot}02°$ C.—or they developed in the sea-water as it was then constituted. The ocean has, however, been growing continually richer in salts owing to denudation of igneous and plutonic rocks. Indeed, the rate of increase of its sodium content has been employed by Joly to estimate the age of the earth reckoned from the time at which water condensed. And although for various reasons all the constituent solutes have not increased at the same rate, and some may even perhaps have decreased, yet the movement of the osmotic pressure has been steadily upwards. The same is true of all water-dwelling animal organisms, so far as the author is aware; their development always involves a rise in osmotic pressure, unless they have a large diminution in the pressure of the external medium superimposed upon their normal condition. Accordingly the naked ova, plant or animal, which develop in fresh water must increase in osmotic pressure from that of their primitive ancestors, namely very approximately the pressure of fresh water, up to that of the mature organism.

Osmotic Pressure in Animals

(*a*) *The Lower Water-Dwelling Phyla.*—In all these groups there is contact either complete or partial between the protoplasm and the external medium. Even though a cœlom has been developed in the higher groups, contact is still maintained through the external surfaces, and the nephric system serves to prevent the too great accumulation of solutes. It has been shown by the extended researches of Fredericq, Bottazzi, Quinton and others that all these organisms possess an osmotic pressure equal to, or very slightly higher than, that of the water in which they live. Thus the same species will have a higher pressure when in the Bay of Naples than when in the less saline waters of the Atlantic. The slight excess of pressure inside the organism serves to maintain its turgidity. Since abrupt changes in salinity are often fatal to such organisms, the dis-

tribution of particular species is frequently limited in a seemingly very arbitrary manner, whereas the salinity of the particular area in which they live is in reality the factor limiting their distribution. The phenomenon may have indirect results also, for the movements of fishes are in some cases considerably influenced by the distribution of the species of crustacea or other organisms upon which they feed principally.

(*b*) *Fishes*.—In this group the division occurs between those organisms which are at the mercy of the external medium and those which maintain their own osmotic pressure more or less irrespective of outside conditions. The cartilaginous fishes, the elasmobranchs, communicate freely through their gill membranes with the water, whereas the bony fishes, the teleosts, possess gill membranes which are quite, or very largely, impermeable.

Hence it results that in the former class there are sea-fishes and fresh-water fishes. The same fish never passes from one to the other, though it may pass from sea-water to less saline estuarine water; in which case its blood and body salts undergo a change, or its total weight changes, till it is again in osmotic equilibrium with its surroundings.[4] Such changes have been studied among others by Sumner, Bottazzi, and by Dakin.

The teleosts, however, have a mean value for the osmotic pressure of each species, even though there are in some of them quite considerable changes with changing salinity. In the anadromous fishes such as the salmon the impermeability of the gill membranes is of a very high order. For between the osmotic pressure of the fish at sea and in the river there is little or no difference. The eggs, however, are deposited in fresh water, and it is extremely probable that these possess a low osmotic pressure, which gradually increases as maturity is reached. Thus the method whereby the salmon has isolated itself from its surroundings enables it to feed on the abundant life of the sea and to pass again without injury into the safety of the upper reaches of the rivers for the purposes of spawning. When one considers that the freezing-point of the blood of most teleosts is about $-0{\cdot}6^\circ$ to $-0{\cdot}9^\circ$, corresponding to about $7{\cdot}2$ to $10{\cdot}8$ atmospheres pressure at 0° C., whereas that of the Atlantic is about $-2{\cdot}0^\circ$ or 24 atmospheres, it is evident that the barrier of an impermeable membrane is very necessary.

(*c*) *Reptiles*.—This class does not appear to have been in-

vestigated very fully, but for each species an approximately constant value is found. Some fresh-water forms have pressures of about 5, whereas marine species may go as high as 7 atmospheres. It may or may not be a coincidence that the blood of the fresh-water reptilia is approximately isotonic with the germ cells in the eggs of birds ; the latter group has of course developed from the reptilia.

(*d*) *Amphibia.*—The frog has afforded material for so many physiological researches that the osmotic pressure of its blood was determined among the earliest of such measurements. The blood freezes somewhere about $-0{\cdot}46°$, so the osmotic pressure is about 5·5 atmospheres. Within the last few years, however, an interesting research has appeared from the Upsala laboratories in which the gradual increase in osmotic pressure of the frog, from spawn through tadpole to the adult condition, has been traced. Thus the stages of the development of the race, so far as osmotic pressure is concerned, are passed through in each individual. In the amphibia, as in all classes from the teleosts upwards, the osmotic pressure is delicately regulated by the nephric system.

(*e*) *Birds.*—Among his numerous researches in this domain of physiology Hamburger determined the freezing-points of the blood of the domestic fowl. Cryoscopic measurements of its, of the duck's, turkey's, and rhea's were also made by the author. It was found that the osmotic pressure of the blood of birds is quite constant for each species, and is higher than that of reptiles and amphibia, being about 7·2 atmospheres. The osmotic pressure of the eggs of several kinds of bird was also determined, and found to be remarkably constant also, provided the eggs were fresh; the magnitude was from 5 to 5·5 atmospheres. During incubation it was found that the pressure of the mixed contents of the egg—and presumably of the embryo also—rose progressively, owing no doubt to the increase in crystalloids arising from the splitting up of complex colloidal molecules. One is tempted to correlate the low pressures found in the adult fresh-water reptilia with the pressures of closely similar magnitude found in the embryo bird. The latter group, abandoning more or less watery surroundings and taking to the air, had to economise in weight in many ways. Perhaps one of these economies was effected by carrying their blood and tissue solutes in more concentrated solution.

(*f*) *Mammals*.—The fluids of physiological importance in mammals have been investigated fairly thoroughly; besides the blood, those most studied have been the milk and the urine. As a whole the freezing-points of the blood of the group lie between $-0{\cdot}55°$ and $-0{\cdot}60°$ C., and when one considers the vast amount of anabolic and katabolic changes ceaselessly in progress in the body, such constancy as is met with in each particular species is most remarkable. In fact it is no less wonderful than is the constancy of temperature, the wonder of which is forced upon any biologist who has to construct a thermostat.

In man, for example, the normal freezing-point of blood is $-0{\cdot}56°$. It may vary $0{\cdot}01°$ perhaps in health. After severe hæmorrhage, when the body salts have been lost in quantity, it has within the writer's experience fallen to $-0{\cdot}53°$, and even in one case to $-0{\cdot}49°$. This is probably the lowest value recorded. In kidney disease, on the other hand, the depression of freezing-point may be greater than the normal. If it is necessary to remove a diseased kidney it is prudent to ascertain first of all that the blood is normal; for it has been found that if the blood freezes at $-0{\cdot}60°$ or lower when both kidneys are in the body, the removal of one of them is always fatal. For if both together fail to preserve a normal molecular concentration in the blood, the removal of one, even if seriously diseased, results in the production of such accumulation of katabolic toxins as to result in death. Among a limited number of samples of blood from kidney patients submitted to the writer it was found that two exceeded the limit $-0{\cdot}60°$ laid down by von Koranyi, and by Caspar and Richter. Removal of the kidney would therefore in these cases have been worse than useless. By examining the freezing-point of the urine collected from each ureter separately it is also possible to get a quantitative measure of the functional activity of the two kidneys. In health the values found lie between $-0{\cdot}9°$ and $-2{\cdot}1°$. With diseased organs, however, it is usual to find lower values, $-0{\cdot}6$ or thereabouts.

The freezing-point of milk is also, like that of blood, very constant. Winter and Nernst were among the first of many to study it. That of the cow lies at $-0{\cdot}55°$, a slightly lower numerical value than that of its blood. Variations exceeding $0{\cdot}02°$ are very rare, and seeing that the milk is a secretion from

cells in osmotic equilibrium with the blood this is rather to be expected. Over a thousand samples examined in Paris gave the mean value just quoted, which was identical with that found by the writer by testing something under a hundred samples in Dublin. The utility of the test lies in its application to the detection of fraudulent watering, which would bring the freezing-point nearer to that of pure water practically in direct proportion to the amount of water added. Usually the question of adulteration of milk is largely decided by an estimation of its fat content. If, however, this is for any reason low, and the sample gives a normal freezing-point, it is genuine milk beyond any question, unless faking has been carried out by means quite beyond the knowledge of the average dairyman. The cryoscopy of milk has been adopted as an official test in at least one municipal laboratory, that of Amsterdam I believe.

General Discussion

On the whole, it may be said that the osmotic pressures met with in animal cells do not exceed 7·5 atmospheres, if one excepts those of organisms living in the sea; in these the pressure only slightly surpasses that of the medium, if greater than it; if lower, it is always because the animal has a more or less impermeable membrane which renders it to a certain extent independent osmotically. Within the body, however, each cell of an animal tissue must be considered a water-dweller, being in osmotic equilibrium with the other cells and the inter-cellular solutions.

Osmotic Pressure in Plants

In primitive naked plant cells, and in free sperms and unfertilised ova, the osmotic relationships with the surrounding medium are much the same as in the animal kingdom.

The advent of the comparatively inextensible cellulose wall introduced an entirely new condition. The cell was free to elaborate solutes to a relatively enormous amount, consequently high osmotic pressures are met with in plants, 15–20 atmospheres being nothing unusual in the foliage leaves of phanerogams. Accordingly as cellulose or very similar substances bound all plant cells except in a few primitive types, differences of osmotic pressure are due to external conditions affecting metabolism rather than to the particular groups of plant in which

they occur. Moreover, instead of the osmotic pressure of all the cells of an organism being very nearly equal, as is the case in animals owing to the circulation of the blood and the action of the nephric system, in plants very great differences are met with in the osmotic pressures existing in the different tissues.

Substances producing Osmotic Pressures

In animal body liquids by far the greater part of the osmotic pressure met with is due to electrolytes. Small quantities of sugar are found in the blood, very minute indeed, but in the milk lactose abounds, being presumably elaborated in the milk glands. Very small amounts of urea and other down-grade products can also be detected. This comparatively insignificant content of non-electrolytes in animal blood is partly due to the fact that a small supply to each cell, maintained at a constant level by the movement of the blood, is equivalent to a larger supply. Soluble waste products, too, are continually being removed, so do not accumulate. Moreover animal cells do not take carbon dioxide in to form sugars as do those of green plant tissues, and much of their nourishment is supplied in the form of complex nitrogenous products of high molecular weight conveyed by the blood-stream only or by the cœlomic liquid in the lower groups. The writer is not, however, aware of any exact measurements having been made as to the percentage, undoubtedly a very small one, of the total osmotic pressure of animal blood which is due to non-electrolytes.

In plants, however, a large, in many cases a quite preponderating, proportion of the pressure is due to non-electrolytes, of which sugars are by far the most important. This has been shown by Dixon and Atkins by determining both the freezing-points and electrical conductivities of sap pressed from plant organs or obtained from the water channels of the wood with the help of a powerful centrifuge. The freezing-point of solution of potassium chloride, having the same conductivity as the sap under investigation, was then found. Since the presence of the non-electrolyte has only a small indirect effect upon the conductivity, unless present in very large quantities, it is permissible to consider the freezing-point of the equivalent potassium chloride solution as being that of the electrolytes of the sap. By subtracting this value from that found directly for

the sap, the depression of freezing-point due to non-electrolytes is obtained approximately. The latter value could also be arrived at by estimating the sugars—usually sugars are the main non-electrolyte constituents—and calculating the depressions caused by them severally. Some rough estimates in cases in which only hexose sugars were present gave fair agreement with the values afforded by the other method.

By this method a systematic examination has been made of the tissues of a number of plants, and in some cases the seasonal changes have been traced.

A word must be added as to the method by which the sap is obtained. At first it was expressed from leaves, etc., wrapped in linen and pressed between silver discs in a vice. It was, however, found that the sap which came out was to a considerable extent diluted with more or less pure water pressed out through the protoplasm of cells which had not ruptured. This introduced an error often very great, and varying from tissue to tissue and from species to species. To avoid this error Dixon and Atkins adopted the plan of rendering the protoplasm permeable by immersion in liquid air. The sap given after this treatment is genuinely that of the vacuole and protoplasm. It is, moreover, richer in enzymes than that obtained by direct pressure of unfrozen tissues, and is consequently more liable to undergo change after expression. For instance, sap thus obtained from yeast contains zymase, and will ferment a sucrose solution since it also contains invertase.

It has already been mentioned that sugars are the chief non-electrolytes found in plant cells. The most abundant are sucrose, or cane sugar, and its products of hydrolysis the two hexose sugars, dextrose (*d*-glucose) and lævulose (*d*-fructose). It is as yet a disputed point whether sucrose is the first sugar to be formed in photosynthesis or whether dextrose precedes it. The excessive accumulation of these sugars is prevented by their transformation into the colloid, starch, which exerts no osmotic pressure.

A fourth sugar, maltose, is frequently met with. It is entirely a down-grade product of starch, under the action, in the cell, of diastase. It is further hydrolysed by the enzyme maltase giving two molecules of dextrose for each molecule. In addition certain pentose sugars are occasionally met with in small

quantity. In most analytical work they would be returned as hexoses.

Factors influencing Osmotic Pressures in Plants

The effect of light upon leaves and other chlorophyll-containing plant organs is by far the most powerful of the many causes which alter the pressure. In light, sugars accumulate very rapidly, accordingly rapid increases take place in the pressure. Sucrose is the sugar chiefly responsible for this. Its formation causes about half the increase in pressure, weight for weight, that a hexose would, since its molecular weight is almost twice as great. The same remark applies to maltose, which has the same molecular weight as sucrose. But starch also appears in leaves exposed to light, and this substance exerts no pressure. Thus far larger supplies of carbohydrates can be stored in the leaf without unduly high pressures than could be if sugars only were formed, as in the leaf-blade of the iris. There is, however, another limit to the accumulation of sugar in a leaf, with its consequent rise in pressure. This is the translocation of the sugar downwards through the bast to be stored as starch in the white leaf bases and rhizomes of the iris, for example, as sucrose in the beetroot, as inulin in the dahlia tuber. In trees storage takes place in the medullary rays and wood parenchyma of the woody tissues. The mechanism of this translocation may perhaps be that of simple diffusion from a region of high sugar content, the photosynthetic cells to one of low content, the storage cells, in which as it were an upper limit is set to the pressure by the formation of some colloidal substance. In many woody tissues hemi-cellulose layers are deposited on the walls as reserve products. The normal permeability of the protoplasm appears to be increased in such cases to allow of the passage of the sugars. By these means a pressure gradient from leaf to root is maintained.

Within the leaf itself many other factors may cause a rise in pressure. For example, in conditions favouring rapid evaporation a certain amount of contraction in volume may take place if the water supply is insufficient; this will lead to loss of turgidity, as the cellulose walls contract, and if it proceeds far enough will result in the wilting of the leaf.

When photosynthesis is in abeyance the losses in sugars due to respiration and continued translocation are made good

by hydrolysis of the leaf starch. Thus one does not find a large and rapid fall in pressure beyond a certain amount. In addition to the above-mentioned method of regulating the sugar supplies, there remains the pressure exerted by the electrolytes. Furthermore, in certain leaves which were allowed to remain on the branch, but shut off from light, it was found that quite a considerable osmotic pressure was maintained while the rest of the leaves were actively assimilating. In dull wet weather, however, the pressure of these leaves fell. This furnishes additional evidence that translocation takes place from regions of high osmotic pressure to regions of lower pressure irrespective of whether this motion is up or down the bast.

Moreover, the supply of electrolytes to leaves and cells of other tissues influences the osmotic pressure. Especially is this the case with leaves, from which large quantities of water are lost by evaporation; the electrolytes in the water supply consequently accumulate. Thus old leaves have always a higher electrolyte content than younger ones on the same plant. The conductivity is nearly entirely due to salts of inorganic and organic acids. As a rule very little is to be attributed to organic acids. For example, the conductivity of the juice of an orange or lemon is less than that of many leaves and roots which are not noticeably acid, and very far below that pressed from celery stalks.

In salt marshes considerable quantities of electrolytes accumulate in the cells of the flora and consequently the cells are not plasmolysed even when evaporation causes a very large rise in the concentration of the solutes in the surrounding water. The Neapolitan physiologists Cavara, Trenchieri, and Nicoloso-Roncati have investigated this class of plant-association by cryoscopic methods with special reference to the marshes of Cagliari. Sand-dune vegetation had been studied by the plasmolytic method by Drabble and Lake previously, and the alfvar vegetation of Oeland has since been examined by Falck. Desert flora too gives examples of the existence of very high pressures, up to about 100 atmospheres in some cases.

The strand-flora of East Indian islands and that of mangrove swamps have also been studied by the plasmolytic method. Conductivity measurements on sap from these classes of plant would be of interest, as they would show what proportion of the osmotic pressure is due to electrolytes. It seems highly

probable that a very large percentage is to be accounted for by them.

A few measurements were made by Dixon and Atkins of the freezing-point of sap pressed from brown algæ. It was found to be slightly below that of the surrounding sea-water. It seems probable that the slight excess pressure of the algal cells was due to organic crystalloids, though this has not been tested. It was shown that dilution of the sea-water led to a considerable reduction in the depression of freezing-point of the algal sap within a few hours. The difference existing between the internal and external osmotic pressures was, however, then much greater than at the start, but it seems that sufficient time was not allowed for equilibrium to be reached under the new conditions.

In the foregoing pages a few of the principal causes influencing osmotic pressure in plants have been mentioned. But there are many problems in plant physiology which can be studied by the cryoscopic method, or better by the combined use of it and conductivity measurements.

Osmotic Pressure Studies in Special Physiological Problems

Allusion has already been made to the use of these measurements in studying the physiology of foliage leaves. It must, however, be remembered that the osmotic pressure only gives a measure of the total number of molecules of solute present, irrespective of the kind of molecule. An interesting case illustrating how large changes can take place in the relative proportions of the sugars of a sap, while the total osmotic pressure remains constant, is afforded by some analyses recorded by Brown and Morris in their classical work on the *Chemistry and Physiology of Foliage Leaves*. Dixon and Atkins found but little change in the osmotic pressure of detached leaves stored overnight. Yet Brown and Morris had shown that under such conditions quite considerable changes had taken place, sucrose having been hydrolysed, the quantities of dextrose and lævulose altered, and maltose having made its appearance from the leaf-starch. On calculating out the number of gram molecules of each sugar it was, however, found that this quantity had also remained practically constant. One is therefore tempted to consider that

the osmotic pressure in some way sets a limit to the amount of hydrolysis of reserve colloids.

Cavara and the Neapolitan physiologists examined the changes in pressure taking place during the ripening of fruits. In some cases, that of the grape for instance, there is a steady rise towards maturation, so that the ripe sugar-laden fruit has a pressure of about 30 atmospheres. In others a preliminary rise is followed by a large fall in pressure. This corresponds to a great increase in the size of the fruit. In such a case the additional volume of the fruit appears to result from the growth of each cell; the vacuole solutes are therefore largely diluted.

An isolated instance of the secretion of practically pure water by a leaf has been investigated by Dixon and Atkins. The leaves of *Colocasia antiquorum*, when in a warm moisture-laden atmosphere, give a stream of fine drops of water from the tips. The drops may follow each other so fast that it is impossible to count them. Twenty cubic centimetres or more of the liquid have been collected from one leaf during the night. The anatomy of the tip of the leaf has not been completely worked out as yet, but the fact remains that water of immeasurably small depression of freezing-point and of conductivity less than that of Dublin tap water—a very soft upland water collected from granite and similar rocks—is secreted from it. The solid residue it leaves on evaporation is also extremely small. The interest of the observation lies in accounting for the rapid passage of water of such purity through the protoplasmic membrane of the secreting cells, and more work remains to be done before a satisfactory explanation can be given.

Another problem studied by these methods was that of the nature of the transpiration stream in woody stems. This had generally been regarded as approximating to good drinking water, though Fischer and others had shown that in the spring reducing sugars were to be found in it. Dixon and Atkins attempted to obtain the sap of the transpiration stream by pressing the wood. It was, however, recognised that the sap was loaded with the débris and consequently the solutes of the living cells of the medullary rays and wood parenchyma. Short lengths of the wood were then placed in a powerful centrifuge, and a quantity of almost colourless clear sap was obtained. On examination this was found to have a small depression of

freezing-point, about 0·06°, and a conductivity corresponding to a freezing point of $-0·02°$ or thereabouts. These figures, however, were observed to undergo large seasonal variations, especially the portion of the total depression due to non-electrolytes. As in the leaves, these were shown to be almost entirely sugars. In the wood of some deciduous trees osmotic pressures of three or four atmospheres may be met with in the wood-sap in early spring before the opening of the leaves. It is then that the mobilisation of the colloidal reserve products stored in the medullary rays and wood parenchyma occurs, resulting in the stream, as it rises from the roots, being enriched on the way by the diffusion or secretion into it of these sugars, sucrose, hexoses, and maltose. Several per cent. of sugar is shown to be present by calculation from the freezing-point depression. Direct analysis showed that in some cases this was a mixture of all four of the above sugars—there are two hexose sugars—whereas more often maltose was absent; at other times cane sugar was the only one present, or, again, hexoses alone might be found. Since the analysis of sugar mixtures is both complex and tedious, it is easily seen what service cryoscopic osmotic pressure determinations are in studying a problem of this type. The chief factors influencing the pressure in wood sap were found to be the nature of the tree, the season of the year, and the height above ground from which the sample was obtained. Examinations were made from base to summit of similar trees of the same species at different seasons of the year, and both deciduous and evergreen trees were examined. As might be expected, the latter do not show such well-marked seasonal changes. The transpiration stream then supplies all parts of the tree with sugars and electrolytes in addition to water.

General Summary

From the various examples given above it may be perceived how different is the physiology of the animal and vegetable kingdoms with respect to osmotic pressure. Were animal cells to possess the power of assimilating carbon dioxide to form sugars, these would have to be rapidly transformed into colloids to prevent the development of too great a pressure, in water-dwelling forms at least. Then since in the animal body all neighbouring cells have to be in osmotic equilibrium, the local

accumulation of large quantities of sugar would induce disturbances. It is therefore hardly accidental that it is in the group of organisms possessing the power of assimilating carbon dioxide that a more or less inextensible bounding membrane was developed.

Finally, since water is a necessity for all protoplasmic life, and since between protoplasm and the external medium, the internal vacuole boundaries, and the numerous compartments and plastids in the cytoplasm one meets with all sorts and degrees of semi-permeable membranes, it becomes apparent that questions in which osmotic pressure effects have to be considered abound in most branches of physiology.

In conclusion the writer wishes to state that the present paper was written under circumstances which cut him off from all reference to journals, reprints, or notes of any kind. While tolerably sure of the accuracy of the figures and names of authors given in the text, he is forced to acknowledge with regret his inability to recall the names of some of the workers whose researches he has mentioned.

EGYPT, *September* 30, 1916.

THE HISTORY OF COMPARATIVE ANATOMY

PART I.—A STATISTICAL ANALYSIS OF THE LITERATURE

BY F. J. COLE, D.Sc. OXON., *Professor of Zoology*, AND NELLIE B. EALES, B.Sc., *Museum Curator, University College, Reading*

RECENTLY one of us made a preliminary attempt to apply graphic methods to an historical study of anatomical museums.[1] The result was more interesting than conclusive, principally because the number of such museums which could be traced (537) was too small to admit of satisfactory treatment by statistical methods. Further, a museum is a complex and shifting quantity somewhat beyond the power of ordinary figures to express. It appeared probable, however, that a similar attempt on the literature of comparative anatomy generally would be more successful. A publication is an isolated and definite piece of work, it is permanent, accessible, and may be judged, and in most cases it is not difficult to ascertain when, where, and by whom it was done, and to plot the results on squared paper. Our object was as follows: (1) to represent by a curve the activities of comparative anatomists as a whole from the sixteenth century to 1860; (2) to detach from this general scheme and plot separately the performances of each European country; (3) to determine in a similar way which groups of animals and what aspects of the subject engaged the attention of workers from time to time; (4) to trace the influence of contemporary events, public bodies and individuals on the history of anatomical thought. In other words it seemed possible to reduce to geometrical form the activities of the corporate body of anatomical research, and the relative importance from time to time of each country and division of the subject.

The scheme is not without its difficulties. A chart repre-

[1] Cf. Cole, *Mackay Miscellany*, 1914, pp. 302–17, 4 Plates.

sents numerical values only, and may by itself be seriously misleading. The author of fifty small ephemeral papers is, judged by figures, of greater importance than William Harvey, represented only by two entries, both of great significance. It is hence necessary that any conclusions drawn from the charts should be checked by an examination of the scientific value of the literature dealt with. But it must still be claimed that the figures alone have their value. The fact that much research is published at a particular period, however indifferent most of it may be, is a sure indication of contemporary interests and activities. Most of us have lived long enough to have been advised, as successive means to salvation, to abandon the pleasures of congenial research and follow the cytologist, the protozoologist and the Mendelian. Each of these enthusiasts has in turn presented his " Quicunque vult " with its unpleasant alternative. The history of our scientific *Modes* is worth studying, and it can best be studied by statistical methods. By these means we can trace the branches of the subject that were attracting attention at a particular time, and what influence, if any, was exercised by the more important workers.

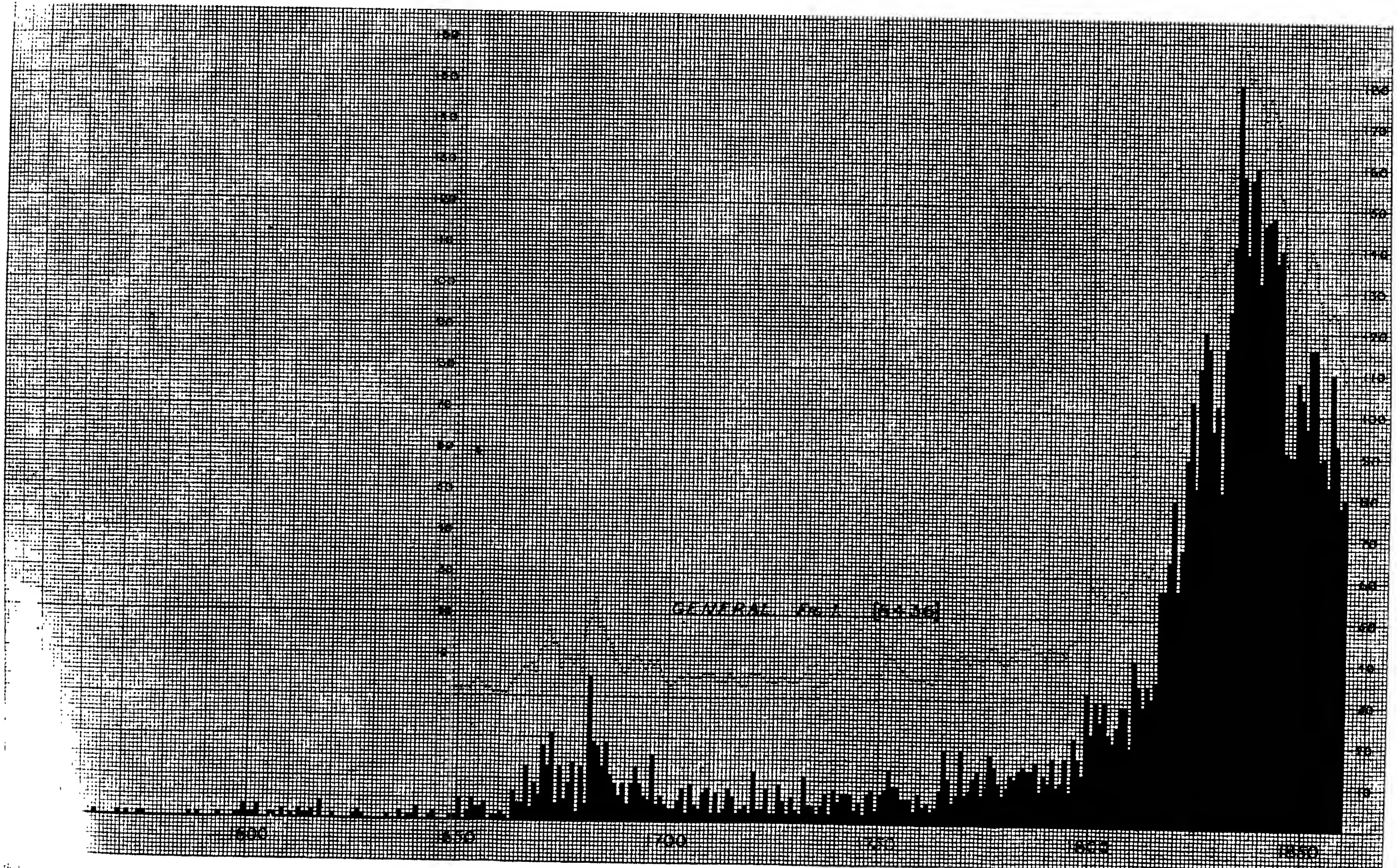
Another difficulty is as regards nationality. How are we to compose the claims of parentage, birthplace, and domicile? Is Cuvier, for example, to be credited to Germany or to France? We have decided each of these cases on its merits, and have been bound by no fixed rule. In the matter of dates it is often important to record the year or years when the work was actually accomplished, rather than the date of publication, which may be years subsequent to the death of the author. Yet here again publication stands for something—at least it represents a current interest in the subject of the research, an interest which may be productive of further efforts of a similar nature. The same may be said of new editions, even if they are only literal reprints of the first. We have, however, disregarded these, but have recorded all re-issues which include new matter. Unedited translations also have been ignored. Place of publication requires careful watching, especially in the case of the early literature, when the publication of scientific work was largely in the hands of a few continental booksellers. Harvey, for example, published his treatise on the Circulation at Frankfort, because he considered its prospects

of becoming known were greater than London publication could secure. This is an obvious case, for no one would think of giving any country other than England the credit of this work, but it is manifest that, with the lesser known men, a mistake could easily arise. As regards work extending over more than one year, and published in more than one volume, the date of each volume has been graphed.

For the period between the years 1543 and 1860 we have made records of 6,436 publications which deal wholly or partly with the anatomy of animals, omitting those which treat exclusively of human anatomy and systematic zoology. It is inevitable that we have missed many, but the number can hardly bear any serious proportion to those we have recorded, nor do we think that the omissions would affect the general conclusions to be drawn from the study of the charts. In fig. 1, in which all our records have been plotted, and which thus embraces the history of comparative anatomy in Europe generally for the period in question, each division horizontally represents a year, and each vertical division one publication. Thus in the year 1800, thirty-two papers dealing with the anatomy of animals were published. The other charts read in the same way.

An examination of fig. 1 shows that only intermittent research was carried on before the year 1650, which is the more surprising as this period was very prolific in men of note. For example, in the few years between 1504 and 1523 we record the birth of Estienne, Rondelet, Servetus, Caius, Mizaldus, Ingrassia, Vesalius, Salviani, Columbus, Gesner, Belon, Paré, Cæsalpinus, Eustachius, Aldrovandus and Fallopius. From the latter year, beginning with Coiter, the birth-rate sharply declines, and does not recover until the beginning of the seventeenth century. It follows from this rise and fall that we have an outcrop of publications between 1540 and 1575, then a comparative blank for over fifteen years, after which the next generation begins to be active in the last decade of the century. For the whole period 1543–1650 we have published works, many of them of striking originality and importance, by Vesalius, Caius, Estienne, Paré, Belon, Gesner, Wotton, Servetus,[1] Salviani, Rondelet, Sylvius,[2] Columbus, Fallopius, Eustachius, Cæsalpinus, Coiter, Varolius, Bauhin, Dulaurens,

[1] Printed but not published. [2] Posthumous.


GENERAL
1600
1650
1700
1750
1800
1850

Ruini, Aldrovandus, Casserius, Fabricius, Ingrassia,[1] Riolan, Butler, C. Bartholinus, Crooke, Paauw, Spigelius, Aselli, Harvey, Worm, Descartes, T. Bartholinus, Vesling, Tulp, Wirsung and Severini.[2] We doubt whether any subsequent period could be made to furnish a list of such formidable names. And yet in spite of it no flourishing school of comparative anatomy was founded, and the research of the period was clearly the result of independent and sporadic activity. Great works were plentiful, but a slow and cautious generation hesitated to endorse them. Vesalius waited nine years before he brought out a second edition of his celebrated *Fabrica*, although the work passed through ten editions in thirty years. It then ceased to be printed, but was revived and enjoyed a considerable vogue in the first half of the seventeenth century. The second edition of Harvey's *De Motu Cordis* only appeared eleven years after the first, but nine editions were issued within about the first thirty years. The beautiful anatomical works of Casserius enjoyed a more brisk but fleeting popularity, but Coiter, important as we now know him to be, was hardly reprinted at all. Fabricius' papers on comparative anatomy, produced within the first twenty years of the seventeenth century, passed through a few editions, but Severini's striking *Zootomia* was never reprinted, and is now one of the rarest of the older books on comparative anatomy. Ruini's magnificent treatise on the anatomy of the Horse enjoyed a better fortune, and was re-issued six times in the first twenty years, but even here the second part of the work on the diseases and care of the horse doubtless accounts for its popularity. What continuity there is at this period is provided by the school of anatomy at Padua, where with such successive professors as Vesalius, Columbus, Fallopius, Fabricius, Casserius, Spigelius, and Veslingius an anatomical tradition could hardly fail to arise and expand. What in fact is most striking in the period before 1650 is the failure to follow up, confirm, or even accurately copy a new departure in anatomy. When Coiter demonstrated, as he did completely, the interest and importance of comparative anatomy, why did the next essay in this promising field take twenty-six years to mature?

[1] Posthumous.

[2] This list is in chronological order as regards the first important work of each author.

Harvey in 1628 exploited the new weapon, and achieved a startling and brilliant result, but it was thirty years before the significance of this lead was grasped and applied. Yet publishers were not wanting, and books enjoyed for the time a wide if sluggish circulation. What was deficient was the critical and expansive faculty of a younger generation. We are thus at the outset compelled to doubt whether the early development of anatomy is not affected by influences other than the writings of its most distinguished exponents. Or perhaps it may be, as Prof. Miall believes, that "nothing less than the harmonious development of every side of biology could really suffice, but biologists were too few and too ill-instructed for so great a task." We must not hastily assume, however, that a great piece of scientific research must inevitably produce an immediate effect. In our own time the Mendelian doctrine lay forgotten for thirty-five years, and was only revived when a vague and pervading interest in genetics, not due to Mendel, resulted in a scrutiny of the literature and the discovery of his work.

The next fifty years, 1650–1700, are characterised by marked interest in anatomical research. It reached its maximum at about 1683, but by the end of the century a considerable decline had set in which was to continue for the next fifty years. Not until a hundred and twenty years later was so much activity again displayed. The discontinuity of the period before 1650 no longer obtains, and we have to record instead an unbroken succession of well-known workers. According to the chart (fig. 1) the revival had set in before 1655, but the real dividing line is obviously at 1650. In fact, both as regards the bulk and regularity of the output, anatomical publication may be said to begin at the latter date. An examination of the birth-rate of outstanding anatomists supplies the explanation. We have already stated that following the high birth-rate at the beginning of the sixteenth century, there was a decline which lasted until the opening of the seventeenth century. The increased rate then initiated held until 1650, when it dropped again. Between 1600 and 1650, fifty-five prominent anatomists were born, whilst the next fifty years produced but thirty-one. We expect, therefore, that between 1650 and 1700 the effect of the increased rate will be most apparent, and similarly that the next fifty

years, 1700–1750, will reflect the decline. And when we find that the period 1650–1700 embraces 2,100 of the working years [1] of the more prominent anatomists, whereas in the succeeding fifty years the number shrinks to 1,700, the rise and fall of the output becomes intelligible. It should be noted that these figures leave out of account the lesser-known writers, but their inclusion would only emphasise the above result.

There is no doubt that the seventeenth-century revival was due to, or perhaps we should say was responsible for, the foundation of the Academia Naturæ Curiosorum in 1652, the Royal Society of London in 1660, the French Academy of Science in 1666, and the lesser-known but still important "Collegium Anatomicum" of Amsterdam in *c.* 1665. In this revival our own Royal Society may justly claim to have played a considerable, if not the leading, part. Most of the prominent anatomists, both at home and abroad, were Fellows of the Society, and others, though not Fellows, were encouraged to contribute to the *Transactions*.[2] Thus amongst the Fellows, many of whom were publishing in the *Transactions*, we have Mayow, Havers, Malpighi, Willis, Ruysch, Moulin, Needham, Vallisnieri, Charleton, Glisson, Willughby, Lower, Douglas, Lister, Kerckring, Leeuwenhoek, Bidloo, Tyson, Vieussens, Grew, Ray, and Valentini, and the authors include C. Bartholinus, Highmore, Pecquet, Steno, Redi, Swammerdam, de Graaf, and Nuck. A noteworthy exception to both categories is Samuel Collins, jun., whose important and encyclopædic treatise on comparative anatomy, published in 1685, first received justice in modern times at the hands of David Craigie, Lecturer on Anatomy at Edinburgh, the author of an able, scarce, and plagiarised history of anatomy. The Academia Naturæ Curiosorum, though the oldest of the societies in question, was not the first to publish or to interest itself in comparative anatomy, for its entry into the field bears the date 1670. In the next thirty years, however, the Academy published a large number of anatomical papers by some forty

[1] By this expression we mean years subsequent to the age of twenty-five. Only rarely is important work published before that age is reached.

[2] For example, Malpighi was invited by the Secretary to correspond regularly with the Society, and lines of research were even suggested to him. Leeuwenhoek's first paper was published by the Society in 1673, and his last in 1724.

contributors, amongst whom we find Seger, Muralt, Major, Lorenzini, Peyer, Schelhammer and M. B. Valentini. This list does not compare with that of either the London or the Paris Society. Nevertheless, if the Academy followed, rather than gave the lead, it took no inconsiderable part in the sequel, as will be seen if their publications are plotted separately and compared with the general chart. But this also proves, however, that having got up late, they went to bed early, for they were among the first to retire when the general decline began. On the other hand, the French Academy was occupied with comparative anatomy from its foundation in 1666,[1] and the "Company" included the great figures of Perrault and Duverney, others only less important being Pecquet, Méry, Charas, Philippe de la Hire, Gayant, and Gouye, with Sébastien Le Clerc as their engraver. It was a tenacious and virile group, the average life of its members working out at seventy-five years, and their publications extending into the early years of the eighteenth century. Of the Amsterdam "College" little is known beyond the two important volumes on comparative anatomy published by its members in 1667 and 1673. The leading contributors were G. Blasius, the author of a well-known and detailed compilation on the anatomy of animals published in 1681, and Swammerdam.

The second half of the seventeenth century confirms the conclusion we drew from the earlier literature. The revival began to spread between 1662 and 1664, and it is difficult to produce any striking publications in the period immediately before that time which would explain it. The Royal Society might, and probably did, have a share in it, but the Amsterdam "College" and the French Academy must be described as the results of the movement rather than its cause. T. Bartholinus, Malpighi, Steno, Redi, Charleton, Velschius, Severini, Boyle, Bellini, Willis, and Ruysch were publishing between 1657 and 1665, but with the exception of Severini these workers were not then the famous names they subsequently became. The decline of the revival is still more inexplicable. It commenced between 1685 and 1687, and continued in spite of the activity of a number of eminent anatomists. Malpighi, Ray, Muralt, Redi, Peyer, Collins, Ruysch, Charas, Grew, Vieussens,

[1] One of us has dealt in some detail with the anatomical work of the French Academy. Cf. Cole, *Trans. Liverpool Biol. Soc.* 27, 1913.

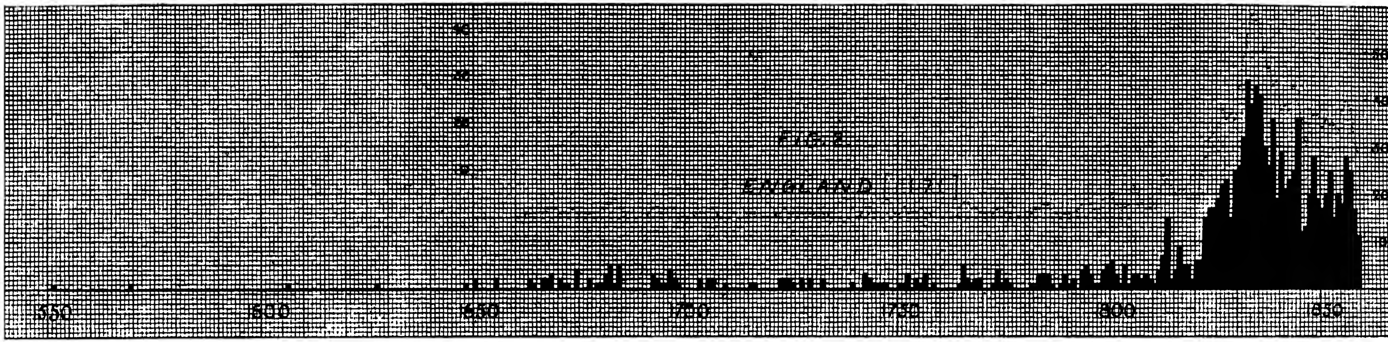


FIG. 2
ENGLAND
1550
1600
1650
1700
1750
1800
1850

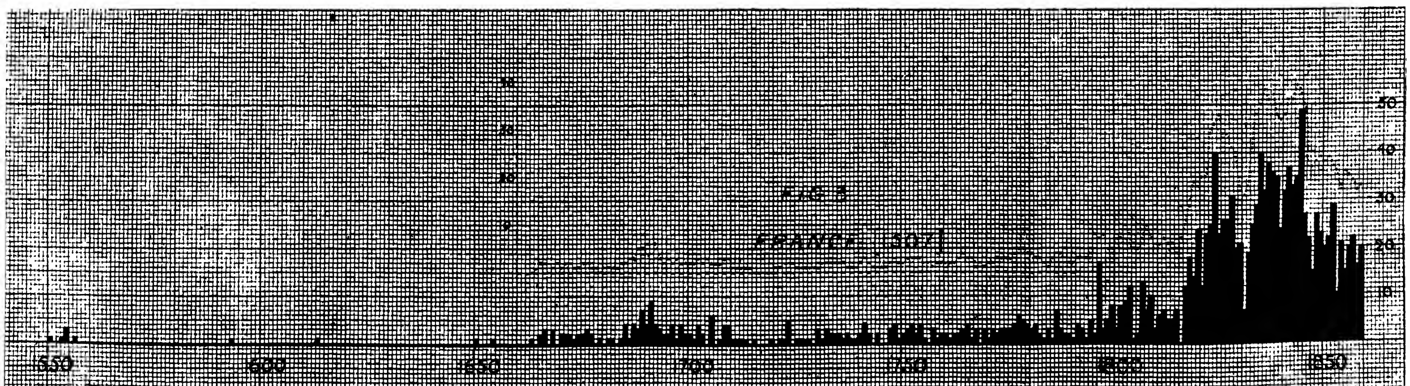


FIG. 3
FRANCE [307]
10
20
30
40
50
1550
1600
1650
1700
1750
1800
1850

Charleton, Lister, Leeuwenhoek, Tyson, Duverney, de la Hire, Valentini, and Méry were still writing, and Swammerdam, Perrault, de Graaf, Steno, Blasius, Willis, and Borelli were by no means forgotten. Again we see that the rise and fall of research is not wholly dependent on the lead of contemporary ability. That lead must be tested and developed by the rank and file of the research army, which in this case, whatever the reason, did not rise to the occasion. It must not here be forgotten, however, that the dissemination of scientific knowledge, if general, was at times very slow. For example, Malpighi's treatise on the structure of the Viscera was published at Bologna in 1666. It was reviewed in the *Philosophical Transactions* for February 15, 1669, where it is stated that no copies of the work had as yet reached England in the ordinary course of business, and that the review was based on a special copy sent by Malpighi to the publisher of the Society.

The first half of the eighteenth century was a quiet and uneventful interval. Leeuwenhoek continues and concludes his industrious career. The last of the old French Academists gradually drop out, their final papers, except posthumous works, appearing before 1720. Poupart died in 1709, Le Clerc in 1714, de la Hire in 1718, Méry in 1722, Gouye in 1725, and Duverney, the last and greatest, in 1730. Their mantle descended on Réaumur, Peyssonel, and Bernard de Jussieu, but although these workers took some count of anatomy, their interests were not primarily morphological. The most noteworthy anatomical event of the period was the posthumous and belated issue of Swammerdam's *Biblia Naturæ* in 1737–8, the English version of which, edited by the satirical "Sir" John Hill, "M.D.," did not appear until twenty years later, and fifteen years had elapsed before the first German edition was printed. The immediate effect of this great work is not apparent. The genius of the time favoured the less rigorous aspects of Biology, and culminated in the launching of Buffon's *Histoire Naturelle* in 1749, a work which even Daubenton's anatomical contributions only slightly dilute. The first extended memoir to bear the general title of *An Essay on Comparative Anatomy* appeared in 1744. It is a small anonymous pamphlet of 138 pages 8vo, based on a student's notes of the lectures of Alexander Monro, primus, of Edinburgh. The notes were published without the knowledge of the Professor,

nor could this "exceedingly defective and erroneous" transcript be expected to gain his approval. About forty years later his son produced a corrected and extended edition, but in neither case is the work more than a series of disconnected accounts of the anatomy of selected animals, and as such is inferior to the treatise of the Parisian anatomists produced eighty years before. An earlier but brief essay, *De Anatome Comparata*, was published by Albinus in 1719, on the occasion of his taking over, in his twenty-third year, the duties of Professor of Anatomy and Surgery in the University of Leyden.

What merit the period 1700–1750 possesses is due largely in the earlier years to overlapping from the preceding century of the works of Leeuwenhoek, Duverney, Ruysch, Méry, Tyson, Peyer, Swammerdam, Charleton, Ray, Lister, Grew, Hooke, and Schelhammer. Muralt, C. Bartholinus, M. B. Valentini, and Vallisnieri, however, all survived beyond 1725, and therefore link up the two half-centuries the more effectually. The publications belonging strictly to the present period include those of Douglas, Stukeley, Sellius, Cheseldon, Artedi, Morgagni, Baker, Trembley, Hales, Réaumur, J. G. Duvernoy, Monro primus, and Lieberkuhn. With the last named we have inaugurated the study of refined microscopic anatomy. The list is not a distinguished one, and is characteristic of the comparative inertia of the period. On the other hand the close of the half-century is more promising, and witnesses the active arrival of Roesel, Buffon, Lyonet, Von Haller, Ellis, Needham, F. D. Hérissant, Daubenton, W. Hunter, Bonnet, and Camper. It is evident the stagnation is not to last.

The next period of fifty years, 1750–1800, contrasts favourably with the monotony of the preceding half-century, and the generation of the impulse which was to develop into the great revival of the nineteenth century is no longer in doubt. The birth-rate, which dropped at 1650 and has since been steadily maintained at the lower level, rises again at about 1770, but the effects of the rise naturally concern the following century. The present period opens with a sudden access of activity which slackens off at the end of the first decade. Nevertheless the movement rapidly gathers momentum, and suddenly swings up to a very high maximum in the first half of the nineteenth century. Some notable personages now make their earlier appearances in public. First comes Dau-

benton, the rejected anatomical colleague of Buffon, followed by von Haller, the greatest of anatomical scholars, and John Ellis. Some ten years afterwards the rivals Camper and John Hunter enter almost together. Monro secundus, Wolff, Vicq D'Azyr, Scarpa, Spallanzani, Home, and Goethe form a group a little later, and at the close the exclusive figure of Cuvier dominates the field. It must not be supposed, however, that this formidable list exhausts the more important work of the period. We have here also the beautiful monograph of Lyonet on the larva of the Goat Moth, Stubbs on the Horse, Roesel on Insects and Frogs, Poli on Mollusca, and comparative anatomical works by F. D. Hérissant, Albinus, Bonnet, Pallas, Hewson, O. F. Müller, C. F. Ludwig, Cruikshank, Blumenbach, Bonsdorff, and A. M. C. Duméril. Gottwaldt died in 1700, but his anatomical treatises on the Tortoise and the Beaver were not published until 1781–2, and important posthumous work by G. J. Duverney appeared in 1761. Towards the close of the century, the graph jumps sharply upwards. This is due particularly to the activity of Cuvier, seconded by Duméril, Blumenbach, Home, and Spallanzani, as well as the last efforts of Vicq D'Azyr. Rudolphi, the teacher of Johannes Müller, Fischer von Waldheim, Wiedemann, E. Geoffroy St. Hilaire, and Poli begin to publish at the end of the century. There is thus no lack of energy and ability at the opening of the new century.

The end of the Napoleonic wars in 1815 coincides so closely with a steep rise in the chart as to suggest the sequence of cause and effect. We must not forget, however, that the rise in the birth-rate at 1770 adds not inconsiderably to the number of prominent naturalists at work early in the nineteenth century, but they would, of course, work under more favourable conditions in times of peace. In this connection it is interesting to remember that Cuvier was born in 1769, and therefore by 1815 the force of his example was in full operation. If we scrutinise the literature which immediately preceded and made possible the nineteenth-century revival, we find that between 1800 and 1815 the most able and active comparative anatomists were Cuvier, his pupil J. F. Meckel, Sir Everard Home, and E. Geoffroy St. Hilaire, but the energy and skill of Cuvier dwarf the efforts of his contemporaries. Less prolific, but not necessarily the less important, are C. R. W. Wiedemann, Rudolphi, G. R. Treviranus, A. M. C. Duméril, Tiede-

mann, Pallas, Poli, Blumenbach, F. Cuvier, and Fischer von Waldheim. Charles Bell, though a human anatomist, is too important to be omitted. Other anatomists active early in the century are Froriep, G. Duvernoy, G. Vrolik, Oken, Spix, Blainville, Bojanus, Dufour, and C. G. Carus. Several of the above workers, however, are but at the outset of their career, and figure more prominently at a later date. The Scots anatomist Fyfe published his *Outlines of Comparative Anatomy* in 1813, and Bojanus an *Introductio in Anatomiam comparatum* in 1815.

It is here necessary to digress in order to consider a new factor in the situation. So far the old method of publication by book and pamphlet had survived in spite of vital and manifest drawbacks. It meant that unless an author had much to say, he had little opportunity of saying it. It suppressed the short and important paper, but offered no bar to verbose incapacity. It worked slowly, and imposed a financial burden on author and public. In the matter of publicity it left too much to the bookseller, and there was no organised attempt to exchange and circulate scientific literature. The remedy for all this was the periodical publication, in which short communications were encouraged, which abbreviated the delays and expense incidental to books, and by the co-operation and fellowship of interested opinion ensured a wide and speedy circulation. It may, in fact, be claimed that science could not have made the advance that it has but for the recognition of the periodical as the most convenient and efficient method of encouraging research. As it happens this was the method first hit upon, but the workers of the time were few and scattered, and spent an opulent leisure in the compilation of elaborate folios. The *Philosophical Transactions* of the Royal Society and the *Journal des Sçavans* both began publication in 1665 in magazine form, and thus both England and France may claim to be the founders of the modern method of publication, the general adoption of which in earlier times would have stimulated inquiry and hastened the coming of liberal and exact knowledge. We have therefore found it necessary to prepare a graph to represent the years in which the various societies and journals concerned more or less with zoology published their first numbers (cf. fig. 10). The diagram is based on the dates of foundation of 577 publications, but

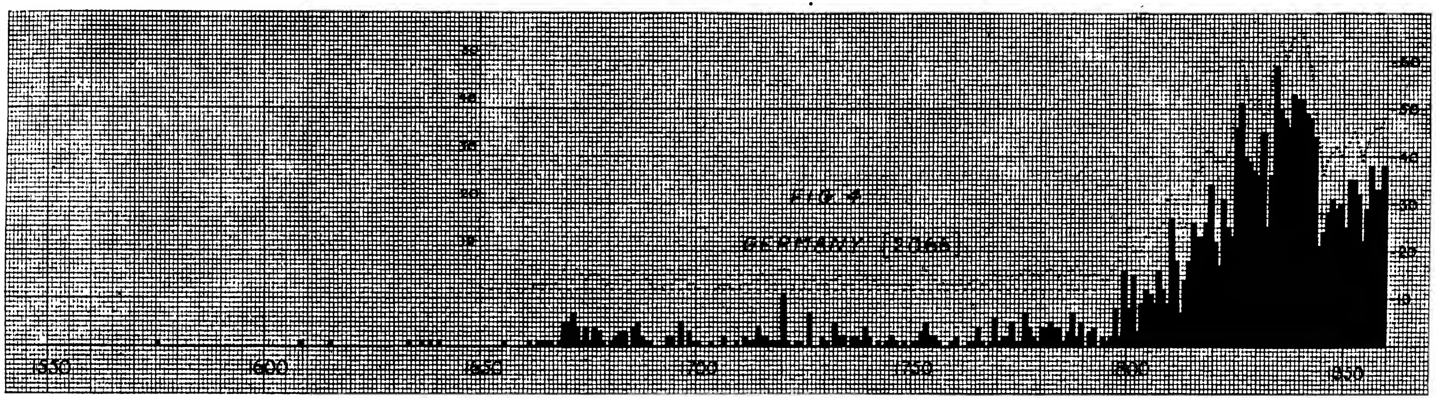


FIG. 4 GERMANY [2066]

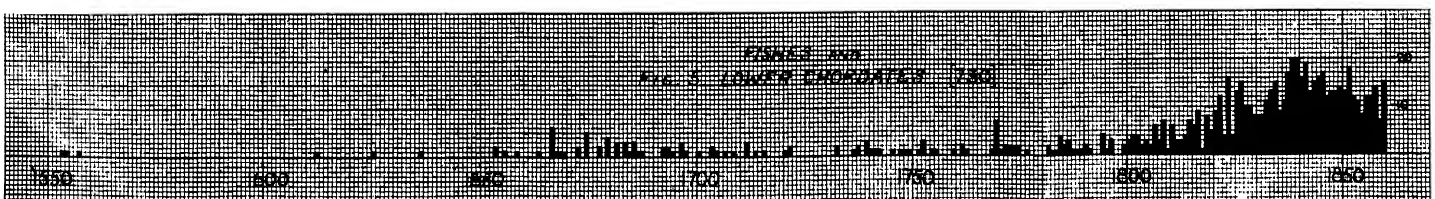


FIG. 5 FISHES AND LOWER CHORDATES [230]

does not show how many periodicals were in existence at a particular time. Many were short-lived, or changed their character, or were absorbed by others of similar aims and wider circulation. Before 1750 the monographic publication was almost the only method, between 1750 and 1800 the corporate serial was taking root, and by 1810 it had assumed control of the situation. If figs. 1 and 10 be compared it will be seen how impossible the great numerical output between 1800 and 1850 would have been but for this radical change in the methods of publication. And if further evidence of the effect of the periodical were necessary, we have only to draw up a list of the authors who have more than ten anatomical works to their credit. Before 1800 only fourteen such can be found,[1] but between 1800 and 1860 the number jumps up to 100. Nothing could be more striking or significant than so abrupt a change, and we cannot avoid the conclusion that a comparison of figs. 1 and 10 suggests the method by which it was brought about. The change, however, relates as well to the character as to the number of works issued. The new method is social, it is the consequence of combinations of different interests, and aims at presenting the results, however meagre, of as many workers as possible. Verbosity would have killed it, and, therefore, whilst the number of papers issued is increased, their length is deliberately abbreviated. Consequently the charts, which assign the same value to a short as to a long communication, emphasise unduly the *bulk* of the output of the nineteenth century, for the work of the preceding period, though powerful in length, is weak in numbers.

The part played by European countries in founding societies and periodicals dealing with zoological topics may now be briefly referred to. In England the representative character and influence of the Royal Society excludes all other effort until near the end of the eighteenth century—a gap which is unique. Nor does England become really effective before 1830. On the other hand Germany is the most active of all in the last half of the eighteenth century, but she fails to add to this advantage until about 1835, when her modern development may be said to begin. France is quiet until the nineteenth century, but at 1820 there is a sudden expansion—not,

[1] These are the exceptions that prove the rule, for the majority of the papers of the fourteen authors appeared in the only periodicals then available.

it is true, maintained—which shows how important a part France played in the final solution of the problem. Of the remaining countries Italy, Holland, Russia, and Scandinavia show steady progress, but Switzerland and Belgium a decline. Austria enters late, but is active after 1845. The drop shown in the graph after 1860 (fig. 10) is due largely to a slackening on the part of Germany and England.

We may now resume consideration of fig. 1. Between 1815 and 1835 anatomical research is in full flood. Then follows the ebb, less rapid but still extraordinarily abrupt. The rise in the birth-rate at 1770 brings the earlier years of a large number of workers within this period. Posthumous work by Lyonet, Camper, Stubbs, Pallas, and Jurine appears. In the early years of the period we have striking works by Poli, G. Cuvier, E. Geoffroy St. Hilaire, Home's Miscellaneous Lectures (1814–28), and Bojanus' beautiful monograph on the Tortoise (1819–21). Other important names occurring here are C. G. Carus, Lamarck, Scarpa, Goethe, Blumenbach, Latreille, Wiedemann, Alex. Brongniart, Rusconi, Rudolphi, Fischer von Waldheim, F. Cuvier, Chas. Bell, A. M. C. Duméril, G. Vrolik, Treviranus, Dutrochet, Blainville, Savigny, G. L. Duvernoy, Froriep, Oken, Spix, Meckel, Chamisso, Tiedemann, Nitzsch, Magendie, Alessandrini, Knox, Dufour, Rathke, Audouin, A. F. Mayer, Serres, Jacobson, Laurillard, Breschet, Eschscholtz, Desmoulins, Dugès, and Zenker. Others at work before 1835, but whose activities extended more or less considerably beyond that date, are: Panizza, O. G. Costa, Straus-Durckheim (whose work on the Cockchafer was published in 1828), Swan, von Baer, F. S. Leuckart, Delle Chiaje, Pander, Valenciennes, Flourens, Grant, Rapp, Ehrenberg, E. H. Weber, Macgillivray, A. A. Retzius, Dumortier, Eschricht, Savi, Diesing, Milne Edwards, J. Müller (whose first paper was published in 1822), Dujardin, W. Vrolik, Volkmann, Wiegmann, Barry, van der Hoeven, J. F. Brandt, Newport, Berthold, Moquin-Tandon, Schlegel, Siebold, Krohn, Owen, I. Geoffroy St. Hilaire, R. Wagner, M. Sars, Westwood, Morren, L. Agassiz, Bischoff, Burmeister, and Stannius. P. J. van Beneden and Hyrtl published their first papers in 1835. Some of these workers were very prolific. The stupendous energy of Owen resulted in over 600 publications, L. Agassiz and Westwood produced over 400, G. Cuvier, E. Geoffroy St. Hilaire,

Dufour, Ehrenberg, J. Müller, J. F. Brandt, and the precocious Forbes and Gervais over 200, and Fischer von Waldheim, G. L. Duvernoy, Blainville, Dutrochet, A. F. Mayer, Flourens, von Baer, O. G. Costa, van der Hoeven, Milne Edwards, I. Geoffroy St. Hilaire, Morren, R. Wagner, Schlegel, Siebold, Burmeister, G. G. Valentin, and Quatrefages over 100. Most of these papers, however, are zoological rather than anatomical, and have little concern with our present purpose. It is worthy of note that the workers of the period, with very few exceptions, and in spite of the fact that many of them attained a ripe age, were publishing almost up to the time of their death. The record of seventy prominent anatomists who began to publish between 1815 and 1835 reaches the astonishing average of over ninety publications. Another point of interest is the increasing precocity which begins to show itself at this time. Up to about 1815 it is unusual that work is published before the age of twenty-five, but after this date the reverse is the case. Here again we see the effect of increased facilities for the publication of short papers which became available at this period. A glance at the above lists, however, shows that it is not so much a time of great leadership as of high average effort and ability. Cuvier, E. Geoffroy St. Hilaire, Dufour, von Baer, Rathke, Milne Edwards, J. Müller, L. Agassiz, and Owen could not fail to stimulate and direct their contemporaries, but before the advent of evolution disconnected and independent research was bound to prevail.

The steep decline after 1835 can only be explained as an admirable example of that rhythm which underlies all the activities of the living world. This primitive abhorrence of the fixed level, which finds its expression in advance or retreat, but never in stability, is just as characteristic of the work of a community as of the internal economy of the individual. It is true that at about 1825 there are signs of another drop in the birth-rate, but this is too late to affect the period in question. Our records do not extend beyond the year 1860, but we believe that the bottom was reached soon after that time, and was followed by another rise. That in its turn gave place to the current depression of which all students of the literature of comparative anatomy have been conscious for several years. To return to the 1835–60 decline, it is evident that it was not due to any disturbance of the facilities for publication, for at

no former period were so many societies and journals founded as between 1840 and 1870. Nor was it due to any lack of distinguished example. We have already referred to the large number of well-known naturalists who were at work before and after 1835. In addition to these an equally distinguished list of others whose working career opened after that year may be given. It will be noticed on reference to fig. 1 that there is a steep drop between 1835 and 1847, then a partial recovery up to 1852, after which the decline is continued to 1860. It is interesting to note that the *Origin of Species* was published on November 24, 1859, and must have played a considerable part in the recovery which followed. Between 1835 and 1848 the following began to publish: Charles Darwin (whose first paper was privately printed in 1835), Doyère, Lereboullet, C. E. Blanchard, Hyrtl, Allman, P. J. van Beneden, Loven, Stein, Calori, Garner, C. Vogt, Steenstrup, Troschel, Erdl, Grube, the brothers Goodsir, Wyman, Bidder, Joly, Desor, Kölliker, W. Peters, Gratiolet, C. B. Reichert, Loew, Brucke, J. T. Reinhardt, A. H. Duméril, E. J. Bonsdorff, Ecker, Will, Hagen, H. Müller, Robin, Huxley, Frey, Giebel, M. J. Schultze, Sappey, E. O. Schmidt, R. Leuckart, J. N. Czermák, and F. Leydig. Straus-Durckheim published a general treatise on comparative anatomy in 1842, and his important anatomical monograph on the Cat in 1845. With such active and responsible leadership in the field, the failure of anatomy in the nineteenth century is difficult to believe, much more to explain. Bound up with it, and perhaps in a large measure explaining it, is the rise of histology and embryology subsequent to the enunciation of the Cell Theory in 1838–9. It is by no means improbable that the energies of many anatomists were thus diverted into histological channels. After 1848 we enter the times of Lacaze-Duthiers, Victor Carus, Virchow, Mendel, Pasteur, W. K. Parker, Gegenbaur, Kupffer, Claparède, W. His, Weismann, and Claus, a consideration of whose labours must be left for another occasion.

Our next task is to dissect fig. 1 and to detach from it and plot separately the records of individual countries (cf. figs. 2, 3, and 4). Before 1650 Italy is the country that figures most prominently, but as against this she is the only nation that does not participate in the seventeenth-century revival, her course being undeflected by that upheaval. France and

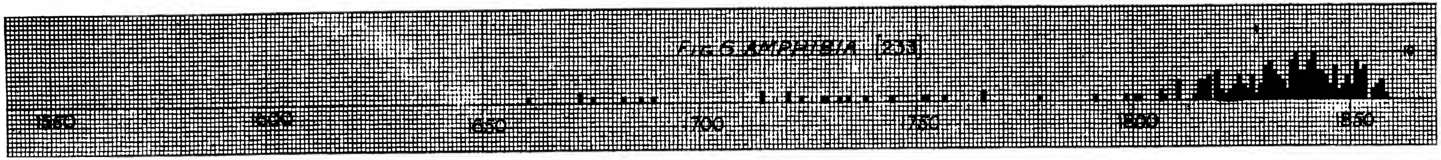


FIG. 6 AMPHIBIA [233]

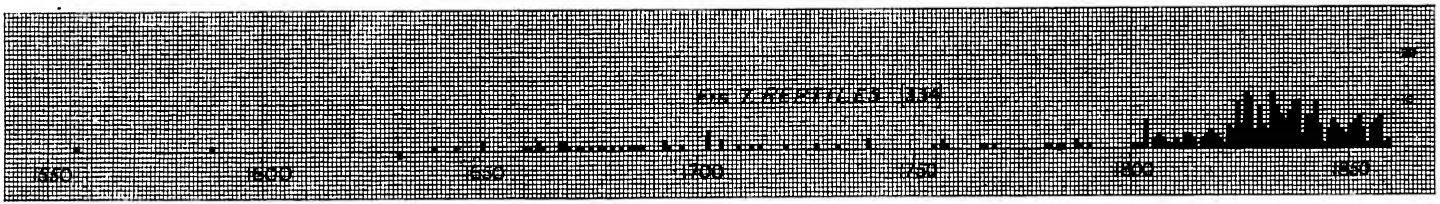


FIG. 7 REPTILES [334]

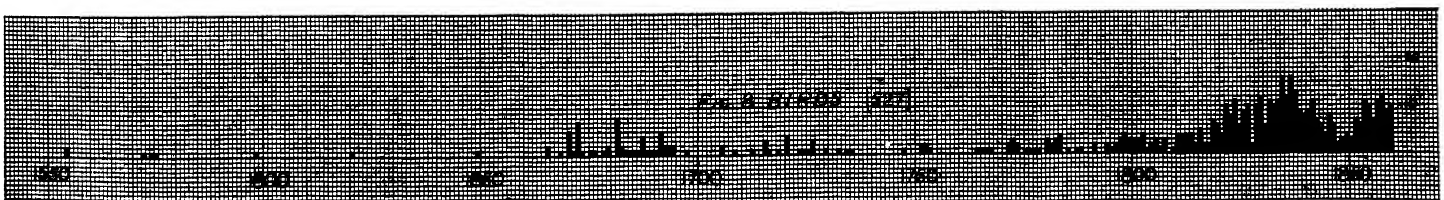


FIG. 8 BIRDS [227]

Germany take the largest part in the revival, Germany rather in the earlier and France in the later phases. To a lesser extent England, Holland, and Denmark are equally concerned, and the activity of a single anatomist brings Switzerland into the contest. Between 1650 and 1700 the first societies and journals are founded in England, France, and Germany. During the next fifty years, Holland, Russia, Italy, and Scandinavia follow this example. The slackening between 1700 and 1750 is least evident in the German graph, but France, Italy, and Holland perceptibly fall away. England still displays an interrupted activity, and Denmark is almost submerged. Russia now makes her first appearance. From 1750 France resumes and retains for some time the leading position, Germany makes slow and almost continuous progress, England grows spasmodically, Italy makes a remarkable recovery, and Russia consolidates her position. Switzerland and Holland, after renewed activity in the early years of the period, die out at its close, but Denmark shows signs of advance. Between 1750 and 1800 there is remarkable activity in Germany in the inauguration of societies and journals—in fact, Germany is the first country to put this policy into effect on an extensive scale. The nineteenth-century revival finds France still in the van, but followed closely by Germany. Towards 1815, and in spite of Cuvier, she drops behind, and gives place to Germany. This pocket in the neighbourhood of 1815, the obvious result of political stress, is to be found in the graphs for England, France, Germany, and Italy. The year 1816 is remarkable as being a blank in the French record—the first to occur since 1769, and the last. In all cases, however, the rebound is immediate and abrupt, as if external events, even of the gravest importance, produce but a transitory effect on learning. The depression at about 1830 is very marked in the French and German graphs, but is much less evident in the English record. England lags behind both France and Germany in the early years of the nineteenth-century revival, but she reaches her maximum first, being followed in this respect first by Germany and then by France. Similarly, in the great decline which followed, a corresponding order is maintained. Curiously enough all three begin to recover at 1847, but again the German record is the best, for the recovery is continued, whilst in the case of England and France,

but particularly in the case of France, the decline is only temporarily arrested (cf. figs. 2–4). Italy may claim an important share in initiating the nineteenth century revival, she reaches her maximum at about 1842, but is in a weak position by 1860. Austria, entering late, reaches her maximum after 1850, and does not seriously go back on it, and Russia also exhibits almost progressive development from 1810. Scandinavia, Denmark, Holland, Switzerland, and Belgium all react to the revival from about 1820, reach a low maximum shortly before or after 1840, and then undergo an almost total eclipse within the next twenty years.

As regards the subjects of study (figs. 5, 6, 7, 8, 9), mammals at all times take precedence, and have received almost twice as much attention as the next most popular group (arthropods). The seventeenth-century revival was due chiefly to an interest in the anatomy of mammals, but other groups studied then, although to a much less extent, were birds, fishes, and arthropods. Reptiles and molluscs also attracted some notice. The slackening between 1700 and 1750 is observable all through, except in the case of those groups, such as amphibia, echinoderms, worms, and cœlenterates, which were only slightly or not at all studied in the earlier days. Interest in mammals, however, if at a lower level, is still strongly maintained. During the second half of the eighteenth century, the time of preparation for the great revival in the following century, mammals continue to attract the greater number of workers, but the study of reptiles is still casual, and amphibia are less investigated than ever. Birds, fishes, molluscs, and arthropods, like the mammals, make a distinct advance, and worms and cœlenterates now begin to be explored effectively. Protozoa, porifera, vermidea, and echinoderms have not yet succeeded in attracting attention. The nineteenth-century revival expresses principally an interest in mammals, arthropods, fishes, and birds in the order mentioned. Worms and amphibia, after the first fifteen years, *i.e.* from 1815, play a considerable part, and so also do reptiles, in which, however, interest has been displayed from the beginning of the century. Of the remaining groups, molluscs and cœlenterates have been most studied, but during the first thirty years of the century very little work is done on protozoa, vermidea, and echinoderms. Sponges are still almost completely ignored. The rapid

decline towards the end of the half-century is evident in every group, although it does not always begin at the same time. In molluscs, for example, the rise continues up to 1845, from which point there is a sudden drop to the neighbourhood of zero. In arthropods the decline dates from about 1845, but continues more slowly than in any other group except fishes, so that by 1860 arthropods are receiving more attention even than mammals. In birds the bottom is reached by 1848, but from this date there is a marked and continuous advance —the only group in which this occurs. Reptiles rapidly lose ground shortly after 1830, but fishes drop from 1840, and then but slowly. The loss of interest in mammalian anatomy is relatively considerable after the strong position occupied by them in 1835, when no group except arthropods was half as attractive. Echinoderms decline from 1840, and by 1860 have completely lost their interest for the anatomist, but the smaller animals such as protozoa and vermidea now begin to be attacked from the morphological standpoint. The investigation of individual organs and sets of organs has been pursued spasmodically since 1600, and even before that time, but it was the nineteenth-century revival which produced the greatest, and a considerable, outcrop of publications of this type, when the influence of Cuvier and his followers was at its height. Works of an encyclopædic or textbook character, the harvest of assiduous compilation, were initiated in the earliest days of comparative anatomy, and they begin to appear more or less regularly from 1670. It is, however, important to note that the nineteenth-century revival, though adding inevitably to the number of such works, is a time of original research rather than of industrious book-making. Hence the general graph would not be materially affected if these publications were withdrawn.

If fig. 1 be compared with the graph illustrating the history of zoological museums already published, it will be noted that in the latter the seventeenth-century revival is not represented. As the anatomical museum owes its development to Ruysch, whose influence can hardly have been considerable before 1700, this is not surprising. The museum chart again differs from the anatomical graph in the bulge it exhibits between 1737 and 1772. This is not due to an interest in anatomical museums, of which very few were founded even at

that time, but to the example of Linnæus and Buffon, who diverted the labours of their contemporaries into natural history channels. On the other hand the nineteenth-century revival is conspicuous both in the anatomical and museum graphs. In the latter the period lies between 1809 and 1847, and, curiously enough, not only does the maximum height occur at exactly the same year in both, *i.e.* 1835, but from this point each records a sharp decline.

Explanation of the Figures

In all the diagrams except fig. 10 each of the small black squares represents one publication. The horizontal scale gives the period, and the vertical scale the number of works published in any year. In figs. 1, 2, 3, 4, and 10 the irregularities of the graphs have been smoothed down by taking three-yearly averages, *e.g.* the total for 1800 is the average of 1779, 1800, and 1801. These curves, for the sake of comparison, have been placed over their appropriate graphs. Fig. 1 includes all the records for all countries. In figs. 2, 3, and 4 the publications of England, France, and Germany have been isolated and plotted separately, and similarly in figs. 5–9 the works relating to fishes, amphibia, reptiles, birds, and mammals have been detached and charted. The totals in brackets represent the number of publications included in each graph. Fig. 10 records the dates on which the various scientific societies concerned more or less with zoology and anatomy first began to publish their Proceedings and Transactions, and also the dates of foundation of those journals devoted mainly or otherwise to the same sciences.

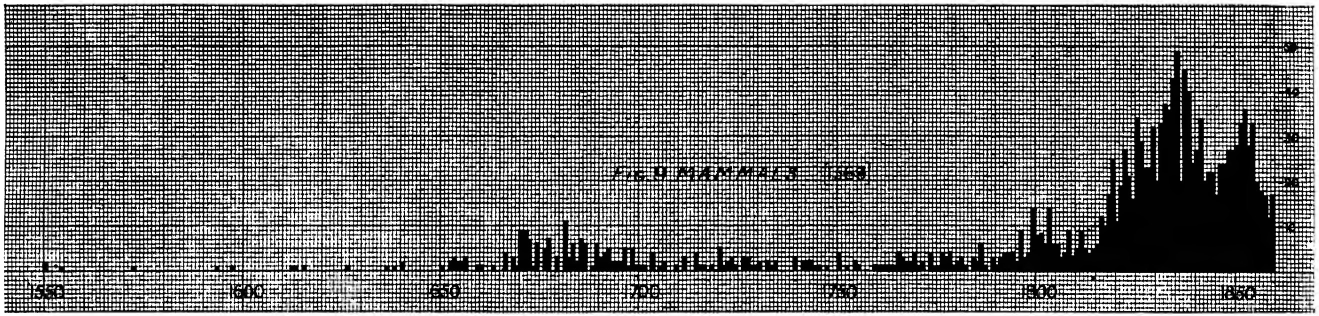


FIG. 9 MAMMALS [368]

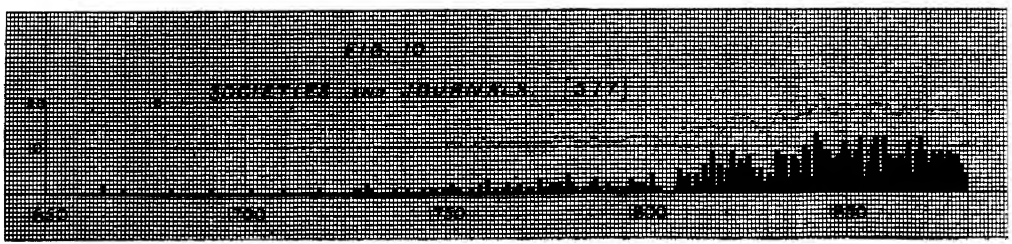


FIG. 10 SOCIETIES AND JOURNALS [377]

POPULAR SCIENCE

SCRATCHES ON FLINTS. By J. Reid Moir, F.R.A.I.

Rocks exhibiting scratches upon one or other of their surfaces occur in many gravels and other deposits in this country. The Boulder Clays of glacial origin contain numerous examples of such specimens, and the rocky floors and sides of valleys in mountainous districts, which were at one time glaciated, very frequently exhibit well-marked groovings or striæ due to the pressure of moving ice with stones embedded in it. In some districts, too, numerous examples of striated flints occur upon the surface of the ground. The author proposes, in this article, to confine his remarks solely to the scratches developed upon flints, though it is possible that the facts to be described may be more or less applicable also to the striæ to be observed upon rocks of a different character.

So far as can be ascertained, no attempt has up to the present been made by scientific men to examine critically the details and characteristics of the striations which are developed upon the surfaces of many fractured flints.

In 1910, however, Dr. Allen Sturge published in the *Proceedings of the Prehistoric Society of East Anglia* a very exhaustive and able paper ("The Chronology of the Stone Age," vol. i. part 1) in which he described and illustrated a number of striated flints found upon the surface of the ground in N.W. Suffolk. This paper, which contained the somewhat startling suggestion that several minor glaciations occurred during the neolithic or latest phase of the stone age, stimulated the present author to investigate a large number of striated flints from different deposits, and to conduct a series of experiments in which flints were scratched by artificial means.[1] The results of these experiments form the ground-work of the present article, but before describing them it appears desirable to consider the various ways in which a fractured flint may be scratched by

[1] For a brief account of this investigation see *Man*, vol. xiv. No. 11, November 1914.

natural forces. The author would like to make it clear that in this article he is referring solely to the striations developed upon the hard interior of the flint, and not to those observable sometimes upon the much softer " crust " or " cortex."

It seems certain that to imprint a scratch upon the surface of a broken flint, the more or less sharp edge of some other harder material must pass over that surface under pressure.[1] Now there are several conditions in nature in which this simple combination of circumstance might occur, and these may be enumerated as follows :

(1) In which a flint is partially embedded in the lowermost stratum of a mass of ice, and dragged over portions of other harder rocks forming the floor over which the ice moves (somewhat similar conditions would be present in the case of an ice-raft with stones partially embedded in it, grounding upon a shore); (2) in which a mass of flint gravel is resting upon some resistant bed and is subjected to the pressure of moving ice, which would cause some of the stones comprising the gravel to be ground against each other and striations imposed upon the softer specimens ; (3) in which a mass of gravel " slips " owing to earth movements of various kinds or to the undermining action of running water, and some of the stones in the gravel are striated in the same manner and from the same immediate cause as in the preceding case. It is possible also that when a mass of gravel slips it may pass over some harder or softer rocks (as in the case of the mass of ice mentioned) and some of the flints partially embedded in its base may imprint scratches upon the underlying rock or be themselves scratched as the case may be.

It has been the custom with some observers to describe striations upon flints as due to what they vaguely term " ice-action " and to regard parallelism of the striæ as indicative of such action. But as has been shown above, pressure may be brought into play in geological deposits without the intervention of ice, and it seems that in each case enumerated the striæ produced might be parallel, or the reverse.

Realising these facts the author has long ago abandoned the attempt to determine whether any given flint in a geological

[1] The weathering out of spongoid and other fossils from flint surfaces sometimes simulates striations, but their real nature is generally easily discernible with a lens.

FIG. 1

FIG. 2.

FIG. 3.

FIG. 4.

FIG. 5.

FIG. 1.—Specimen of slightly patinated flint scratched with a steel point.
Notice the clusters of thin, minute plates of flint produced by the pressure of the moving point.

FIG. 2.—Specimen of striated flint found upon the surface of the ground.
The weathering-out process has not in this case reached completion, and thin, minute plates of flint may be seen at the sides of some of the striæ.

FIG. 3.—Specimen of striated flint found upon the surface of the ground.
The weathering-out process has in this case reached completion.

FIG. 4.—Specimen of flint with glazed surface scratched with a steel point.
Notice the thin, minute plates of flint produced by the pressure of the moving point.

FIG. 5.—Specimen of flint patinated white and scratched with a steel point.
Notice how this comparatively soft surface has been cut into by the pressure of the moving point.

deposit has been scratched by another rock by means of pressure brought into play by moving ice, or by earth movements of one kind or another.

The scratches themselves, however, provide a very profitable field of study. If an extensive series of striated flints is put out for examination it will almost certainly be noticed that some of them, though thin, nevertheless exhibit upon one or other of their surfaces wide and deep striæ. To those who have not made a close study of this subject it would appear that these striæ were imposed as we now see them, by the initial impingement of the rock which passed over the flint surface, and it will also probably be assumed that a great amount of pressure was exerted to produce such a result as is observable. Most people are aware that flint is a very hard substance,[1] and these conclusions therefore appear reasonable and sound; but in reality they are quite unsound. When the author commenced his investigation of striated flints he was at once struck with the remarkable fact that comparatively thin flakes of flint should have stood, without breaking, the great amount of pressure to which they must have been subjected if the deep striæ present upon them were imprinted by the initial impingement of the agent of striation, and this feeling of astonishment was increased by the knowledge, gained in experiments carried out in another investigation, that the soundest flint nodules will disintegrate under a quite moderate amount of pressure. He concluded, therefore, that the deep striæ developed upon the flakes of flint are not now in their original condition, but have become accentuated and altered since they were first imprinted. The difficulty was to find out how and why this accentuation and alteration had come about, and an examination was commenced therefore of several hundreds of scratched flints from various deposits, and from the present land surface.

This examination demonstrated clearly that while some of the specimens showed wide and deep striæ or grooves on their surfaces, others exhibited lines of shattering, composed of very thin and minute plates of flint, instead of the well-marked grooves (fig. 1). Further specimens again exhibited a com-

[1] Reinhard Brauns in his book, *The Mineral Kingdom*, p. 51, gives Moh's scale of hardness of minerals, in which quartz is quoted as being the fourth hardest mineral. Bristow, in his *Glossary of Mineralogy*, p. 140, states that flint is "slightly harder than common quartz, which it scratches."

bination of both these characteristics (fig. 2), and the author concluded therefore that when these flints were first attacked by the agent of striation a line of shattering was in nearly every case produced and that the shattered portion of flint was gradually removed by some natural process (fig. 3). It seems clear that continued exposure to the effects of damp and changes of temperature would be quite competent to remove these thin minute plates of flint, and that it might be held therefore that the well-marked grooves represented simply "weathered-out" lines of shattering. The next point to clear up was the amount of pressure needed to produce lines of shattering upon a flint surface, and a series of specimens was collected composed of newly broken sound flint fresh from the chalk, and of others from various sites which exhibited different degrees of "patination" or surface change. The author then procured a piece of sound flint from the chalk, and having broken it in such a manner that a sharp edge was produced, commenced to attempt to impose lines of shattering with it, upon the various specimens mentioned, by utilising the strength available in his right arm. These experiments demonstrated that while it was impossible to scratch the newly broken, sound flint, the other specimens could be scratched, and that the visibility of the markings produced increased with the degree of patination of the specimens. It was noticed also that nearly all the flints so scratched exhibited similar lines of shattering (composed of thin minute plates of flint) to those observable upon the specimens striated under natural conditions.

The exact cause of the patination of flint is at present obscure (it is possible that several causes may be able to produce it), but it generally shows itself in a distinct colour change of the surface. There appear to be three distinct stages of patination. The first is when the flint is attacked slightly and exhibits a light blue coloration due to the black interior showing through the thin white film spread over it. The second is represented by a denser blue, and the third by pure white which indicates that the patina is thick enough to obliterate the black background on which it has formed.[1] There is another condition sometimes present in flint which may be related to patination, in which the specimen does not exhibit any colour change, but has a

[1] The phenomenon of the staining of flint by various substances is quite a separate phenomenon to patination, and has no bearing on the present inquiry.

FIG. 6.

FIG. 7.

FIG. 8.

FIG. 9.

FIG. 10.

Fig. 6.—Portion of striation upon the Westleton flint, showing the thin, minute plates of flint unremoved.

FIG. 7.—Portion of striation upon the Westleton flint from which the majority of thin, minute plates of flint have been removed by the author.

FIG. 8.—Specimen of flint showing hard, resistant area of oval outline which has remained unaffected by the patinating process.

FIG. 9.—Specimen of bottle-glass exhibiting various kinds of striæ.
This specimen was found upon the surface of a ploughed field. Notice the parallelism of some of the striations.

FIG. 10.—Specimen of bottle-glass exhibiting various kinds of striæ.
This specimen was found upon the surface of the ground. Notice the parallelism of many of the striations.

well-marked glaze. This results in the formation of a somewhat vitreous surface which the author found was susceptible to shattering under the pressure of a moving point (fig. 4).

His experiments showed that this glazed surface is perhaps a little harder than that of the specimens showing the least amount of patination, that those more patinated are still softer, and that the softest of all are those in which the patination had progressed to the white stage. In the case of these latter specimens it was found to be possible to cut into their surfaces and produce well-marked grooves with comparative ease (fig. 5). These experiments demonstrated clearly that newly broken, sound, unpatinated flint is very hard,[1] that other patinated examples are in a much softer condition, and that with no great pressure it was possible to impose lines of shattering upon these latter specimens.

As also the flints scratched under natural conditions and selected for examination had evidently been patinated before they were scratched, and as most of the striæ showed evidences of "weathering out," it became apparent that it was not necessary to suppose that they had been subjected to very great pressure, and that the wide and deep striæ developed upon the thin flakes of flint were in all probability simply "weathered out" scratches, the initial stages of which did not require very great pressure to produce. To put this question of the weathering out of scratches to some sort of test, the author experimented with a flint found upon the surface of the ground at Westleton, Suffolk.

This specimen showed upon its surface a line of shattering about an inch in length, and by the use of a small steel probe the shattered material was easily removed for the distance of half an inch and a well-marked groove produced. Thus this particular striation is now composed of a line of shattering over half its length (fig. 6), the other half being represented by a most obvious cleft or groove (fig. 7). The author has examined a very large number of striated flints, and with the exception of one massive specimen found beneath the Norwich Crag,[2] all

[1] Experiments were conducted in which emery powder mixed with oil was forcibly rubbed upon the surface of such flint, and after two hours' work only a few very slight scratches were produced.

[2] This specimen showed a V-shaped cut in its surface, as though the agent of striation had been able to cut right into the flint; the striation showed little or no signs of weathering. The ordinary weathered-out striæ are more or less U-shaped in section.

show evident signs of the usual lines of shattering in different stages of weathering, and it seems that the idea that the presence of deep and wide striæ necessarily indicates that the flint exhibiting them has been subjected to great pressure must be abandoned.

In the course of his investigations the author has noticed that flints vary considerably in hardness over small portions of their surface. Sometimes the specially resistant areas are readily observable as their texture is quite different from the surrounding material, and these may be likened to the hard " knots " which occur in wood (fig. 8). But there are other and much more subtle differences in hardness in the surfaces of broken flints. An examination of a large series of patinated flints will show that some exhibit an unequal and patchy patina, due, in the author's opinion, to the varying hardness of the flint. The portions of the surface which are patinated may be regarded as the softer, while the unchanged areas are harder and have been able to resist the patinating process. There is, however, no discernible difference in the actual texture of the patinated and unpatinated surfaces. Frequently a specimen is seen which has been flaked at one period and patinated a certain colour. Then at a later period the flint has been reflaked, and this new flaking shows a different and less intense patina. Such specimens are often striated and the striæ on the older flaked areas are of a different order to those observable on the later fracture surfaces. This difference might lead some observers to think that such a stone had been subjected to striation at two different periods. But this may not have been the case, and the author would expect the striæ on the two surfaces to be different owing to the difference in hardness of these surfaces, and unless very definite evidence to the contrary was forthcoming would prefer to favour the view that such a flint had been subjected but once to the scratching process.

The author has been able to confirm his opinion of the variable hardness of flint, by subjecting examples, which to the naked eye presented a perfectly homogeneous texture of surface, to the action of various solvents such as dilute hydrofluoric acid and carbonate of soda (heat was applied in this latter case) which had the effect of simulating patination. In some cases these solvents revealed resistant areas in the flint which remained unaffected while the surrounding and softer portions were altered.

This variable hardness of flint has a distinct bearing upon the characteristics of the striæ imposed upon it. If a flint surface, unequally patinated, be subjected to the pressure of a moving point it will be seen that when this point passes over a patinated area it will cut more deeply into it than when passing over one of the unaltered harder portions. Thus in the passage of the point over the surface of flint-scratches of varying depth and appearance will be formed according to the nature of the surface upon which the moving point impinges.

It has generally been accepted as an axiom that steel will not scratch flint, and if by " flint " is meant the newly broken, sound variety as it is quarried from the chalk, this axiom is true. But the author has found that it is possible to scratch patinated flints with a steel point, and that these scratches vary in depth and appearance according to the amount of patination. He also found that the usual lines of shattering were produced such as have already been described.

The susceptibility of patinated flint to striation by the pressure of a steel point may perhaps explain the large number of scratched flints found upon the surface of the ground in certain localities. It seems reasonable to suppose that some of these striations may have been imposed by the metal edges of ploughs in their passage through the ground, the iron teeth of harrows, etc., especially as it is known that such agencies can produce striations of various kinds upon pieces of glass lying upon the surface of the ground (figs. 9, 10). The author in this article does not wish to convey the impression that he disbelieves " ice-action " to have been responsible for any striations on broken flints, or that these striated flint specimens have not been subjected to an appreciable amount of pressure. He has tried simply to show that striations may be imposed upon a flint without the intervention of ice, and that it is not necessary or in fact possible to invoke " enormous pressure " to account for their imposition.

The photo-micrographs accompanying this article were taken by Mr. Gerald Davey of Ipswich—they are magnified about nine diameters.

ESSAY-REVIEWS

DR. MERZ'S PHENOMENALISM, by JOSHUA C. GREGORY, B.Sc., F.I.C.: on *Religion and Science*, by JOHN THEODORE MERZ. [Pp. 192.] (Blackwood & Sons, 1915. Price 5*s.* net.)

MR. BALFOUR describes the conviction that the universe includes each individual as an item surrounded by things and other personalities as an "inevitable" belief. Inevitableness, he adds, is not in itself a ground of philosophic certitude. If it were, we should not find Dr. Merz among the metaphysicians who, as Mr. Balfour remarks in the same connection, wish to rethink the universe that quite satisfies the plain man.

As the universe expands before the eyes of science this "inevitable" belief becomes more and more a source of humiliation. Man not merely seems to himself to be one portion of reality, but also perceives that he is dwindling into a more and more insignificant item. He has, however, one great resource—his egotism. He can and does think that he, when the universe is rightly interpreted, is the real centre of all being. The egotism of this conclusion is largely unconscious, and is, perhaps, on this account all the more effective. Man perceives a vast reality around him, but he *feels* that he is, after all, a very superior sort of item. Emotion and desire are the real sources of egotistical belief; intelligence inclines to humility. Sigurd Ibsen has summarised the present situation. The Copernican decentralisation and Darwin's withdrawal of man's divine patent of nobility, he writes in *Human Quintessence*, have been received by human intelligence, but have not penetrated the emotions. Dr. Merz foresees that this penetration will come. He notes a steady minimising of the value of human effort and originality. The movement began from Copernicus to Laplace. Darwin strengthened it. It gathered momentum as scientific method rapidly mechanised the aspect of the world and social organisation converted human beings into instruments of production. Science will, in the end, impose its humble view on feeling as well as on

intelligence. The view that the outer world has overwhelming influence on the inner leads to fatalism. There must, therefore, be a revision of opinion concerning this autocratic outer world.

Dr. Merz's revision is drastic. He reverses the plain man's view, disregards its "inevitability," and maintains that the outer world is included in, and subsequent to, the inner world of consciousness. We are not placed in a world external to ourselves, but this world, apparently outside ourselves and independent of us, is really part of us and dependent on our consciousness for its existence. We are not items of a whole—it is the world that is the item. The external world, in short, is an excerpt from our consciousness. Dr. Merz traces the excerpting process in the developing infant mind. At first no outer world exists; there is simply a passage of feeling and impression through consciousness—a primordial continuity of experience. The perceptual and the emotional are fused or unseparated; there is no aggregating or atomising into separated and space-filling objects. There is no inner and outer—no subject and object. Then "clusters" form in the conscious flow and become objects. This mental habit of "clustering" grows and results in the "Firmament of the Soul." In this firmament the primary, unseparated conscious flow, originally continuous and unexternalised, is knotted into groups. These groups include persons in the first place and material bodies in the second. This separation of the external world in the conscious process is initiated by "intersubjective communion." The moment of the birth of the distinction between subject and object arises when the figure of a person flashes on the background of the mind's consciousness. It is then that "clusters" form in the conscious flow and become objects. The remaining background constitutes the subjective side. Dr. Merz evidently does not regard the objectified and externalised conception as an *answer* to intimations from an actual external world—he is no realist. Consciousness is prior; no outer world comes into existence till we are in being, and not even then does it appear at once. We first perceive that other personalities surround us and then, by an illegitimate extension of this perceiving process, imagine ourselves in the midst of a universe of things that is really only a separated part of our own consciousness.

Whether this method of supporting human egotism will compensate the plain man for the disconcerting inversion of the most "inevitable" of all his beliefs is at least open to question. It would be satisfactory to believe that he is really bigger than the universe which *seems* to loom so large beside his own tiny self, but it requires a very settled metaphysical habit to rest comfortably in such a manner of thinking. It is pretty certain that Dr. Merz will not convince the plain man till he has both made him a metaphysician and settled him comfortably in the rôle. But is Dr. Merz's view really metaphysically sound? We may or may not sympathise with his endeavour to maintain the dignity of man; but we are not bound, in either case, to admit that a metaphysician must necessarily believe that the universe is merely an item excerpted in a special manner from human consciousness.

Dr. Merz's theory is, of course, found in the general metaphysical repertoire. It is simply phenomenalism—the belief that the physical world exists *only* as a modification of consciousness. Phenomenalism has obviously one "inevitable" implication. Consciousness is certainly wider than the impressions and ideas that denote for us our surroundings of material objects. There are, for example, emotions as well as sensations. The physical universe, then, is an excerpt—a portion of our total consciousness. Phenomenalism is thus bound to reverse the common-sense view that we are in a material universe and represent the physical world as part of us. Now, it is this particular aspect of the phenomenalistic contention (the aspect that brings it most sharply into contrast with common sense and enlightened realism) that attracts Dr. Merz. He is bent on vindicating the dignity of man. Unfortunately, his method of vindication is not so satisfactory as might appear at first sight. The world is the mind's own product or invention! Allow, for the moment, that it is. We are still cold when it snows, swallowed up by the earth when it quakes, dependent for warmth on the sun's rays. Does Dr. Merz remember the story of Frankenstein? There may be much honour in our ability to understand, however imperfectly, a world into which we have been involuntarily introduced and much credit in making the little nook where we have been inserted as convenient and beautiful as possible. But to be so subject to a world that we have ourselves created, so

far from securing that elevation of individual value that Dr. Merz desires, seems to involve us in a still more abject slavery. We are, in our own little way, conquerors—conquerors who slowly wrest many secrets from nature and gain some advantage of comfort and wider possibilities of living in our struggle with the world that encircles us. Phenomenalism cannot alter these experiences, it cannot alter our dependence on physical factors, however it may choose to interpret the matter; and Dr. Merz can scarcely be said to be happily inspired in suggesting that such small conquests as we can make have to be effected over a world that we have ourselves created.

Phenomenalism, then, is of no use to Dr. Merz. It makes him no less the creature of circumstances, if anything it makes him more so—for it assigns him servitude to his own mind. This, however, is not the whole conclusion of the matter. Metaphysical principles do not stand or fall with their success or failure in promoting human dignity. At the same time phenomenalism actually does fail both to avert man's dethronement from the centre of the universe and to satisfy the demands of reason. Curiously enough, Dr. Merz lays special stress on the very point that most effectively disposes of phenomenalism. The persons that surround us induce us, so runs his argument, to believe in another world surrounding them and ourselves. Now, by arresting its anti-realistic argument at persons phenomenalism lands itself in contradiction. It is quite vain to combine a phenomenalistic view of the physical universe with a realistic view of persons.

If the physical world is an excerpt from consciousness, its flora and fauna are an excerpt too. The sauce for the gander is the sauce for the goose. The argument that proves the shepherd's staff to be purely a modification of, or an operation in, consciousness proves exactly the same thing of the shepherd and his flock. Once Dr. Merz denies that external physical realities exist, he becomes, in logic if not in fact, a solipsist—believing that everything is an occurrence, and only an occurrence, in his own mind. All other men, including brother phenomenalists, have bodies, and these, on his own showing, are simply notions of his own. Their minds, as far as he is concerned, are attached to these bodies, for he only knows them through the latter. If his friends had no bodies he

would never know of their existence—if it be supposed that they could have an incorporeal life. There is no escape for the phenomenalist; he must conclude that, like the material universe itself, the minds and bodies of his friends exist merely in his own consciousness. Phenomenalism must pass over into solipsism, for it can make no provision for the actual existence of other minds and animals. With solipsism comes chaos. Smith is part of Jones, and Jones is part of Smith. The world exists as many times as it has inhabitants, since it is an excerpt from the consciousness of each. The simple fact that he is one of a crowd makes shipwreck of Dr. Merz's phenomenalism, by logically constraining him into solipsism, which, as Prof. C. A. Strong remarks,[1] is "a psychological truism, but an ontological absurdity."

ROSCOE AND MELDOLA, by FREDERICK A. MASON, B.A. (Oxon), Ph.D.: on

The Right Honourable Sir Henry Enfield Roscoe, P.C., D.C.L., F.R.S. A biographical sketch, by SIR EDWARD THORPE, C.B., F.R.S. [Pp. viii + 208, with photogravure frontispiece.] (London: Longmans, Green & Co., 1916. Price 7s. 6d. net.)

Raphael Meldola, D.Sc., F.R.S. Reminiscences of his worth and work by those who knew him, together with a chronological list of his publications, MDCCCLXIX . . . MDCCCCXV. Edited by JAMES MARCHANT; preface by the Right Hon. Lord Moulton, K.C.B., F.R.S. [Pp. xv + 225, with photogravure frontispiece.] (London: Williams & Norgate, 1916. Price 5s. net.)

To judge from the effusions of certain modern novelists, one would suppose that Bloomsbury must be populated entirely—or almost entirely—by fortune-stricken heroes and heroines interred there in lodgings or back attics during such times as the authors of their existences are thickening the plot in the intervening chapters of their stories. To such a pass have things indeed come, that one playwright makes his character remark :—"Bloomsbury, my dear—the place you pass through on the way to St. Pancras." "Heavens!" cries her companion, "I would rather die than live in Bloomsbury!"

It comes therefore as something of a relief to find that other and more famous persons than the ill-starred victims of a novelist's imagination have had their abode in this corner of the metropolis. Chemists in particular have a special interest

[1] *Why the Mind has a Body*, Macmillan Co., New York, 1903.

in this part of London, for many of the illustrious founders of our modern science have lived and flourished in the district.

To give only a few examples, Cavendish, the discoverer of the composition of water, had his house, which still stands, at the corner of Bedford Square, and a succession of famous chemists, Graham, Williamson, and Ramsay, have done the greater part of their life's work at University College, which stands within the charmed boundaries of Bloomsbury.

It is, therefore, without great surprise that one learns that Sir Henry Enfield Roscoe was born in Bloomsbury, at 10, Powis Place, Great Ormond Street, and that the late Prof. Raphael Meldola spent the latter portion of his days, and finally died at 6, Brunswick Square, London, W.C.

The two volumes on Sir Henry Roscoe and Prof. Meldola respectively are a small tribute to the memory of these two great scientists—great both as chemists and as public men—to commemorate their varied activities and to remind posterity of the debt it owes to them. The books are of slightly different character, inasmuch as the work by Sir Edward Thorpe is of the nature of a short biographical sketch of Sir Henry Roscoe's life, whilst the other, written by various hands, is rather more of a series of short appreciations by the various writers of special phases of Prof. Meldola's life, together with a bibliography of the published articles written by him.

It is not proposed to deal here with the accounts given of the purely scientific labours of Roscoe and Meldola, which are too well known to need detailed description; one has only to recall on the one hand the researches of Roscoe on spectrum analysis, on vanadium, or on the composition of constant-boiling mixtures, or his text-book of chemistry, and on the other hand Meldola's work on coal-tar derivatives, and more particularly on dyes, to realise how deeply laid are the results of their labours in the structure of modern chemistry. Both men, though in some respects of very different type, had this in common, a keen delight in the acquisition of new knowledge and experience from whatever direction it came; both, for instance were greatly interested in astronomy, and in each case, curiously enough, made a voyage abroad to assist in observing an eclipse of the sun, Meldola in 1875 and Roscoe in 1870, the latter suffering shipwreck on the voyage, though fortunately without disastrous results.

In other directions the character of the two diverged somewhat. Roscoe was in one way or another somewhat of a public man, both in his capacity as an educational reformer and as a member of Parliament; Meldola, on the other hand, was of a rather more retiring disposition, and after he had relinquished his technical posts in order to accept the Professorship of Chemistry at the City and Guilds College, his tendency was rather to keep to his scientific studies, though he also was not averse to making himself heard by the larger audience outside his lecture-room if he thought the occasion demanded it. In any case it is much to be regretted that owing to the short-sightedness and ignorance of chemical manufacturers in this country, Meldola decided to relinquish his technical career, and confine himself to the more academic branches of his subject; on this point Lord Moulton in his preface expresses himself clearly:

"To my mind it was little less than a calamity that Prof. Meldola in the prime of his life left industrial chemistry to become Professor of Chemistry at the Finsbury Technical College. But in heart he remained an industrial chemist to the end of his life. And in truth he had no choice. Industrial chemists of his type were a drug on the market. No career was open to them, and perforce they had to take to teaching."

Both Sir Henry Roscoe and Prof. Meldola, though not strictly speaking technical chemists, continued, however, during their lives to take the greatest interest in the application of chemistry in the arts and, so far as lay in their powers, endeavoured to assist the retarded development of chemical industry in this country: indeed the slightest study of the lives and activities of these two eminent representatives of chemical science is indissolubly bound up with the history of chemical industry; both endeavoured unceasingly to call attention to the grave public dangers attendant on our national neglect of science, particularly chemical science, and both lived to see their worst fears realised when the outbreak of the present war found this mighty empire almost without dyes or drugs, without high explosives or sufficient supplies of acids or coal-tar products for their manufacture, minus tungsten for high-speed steel (with which to make the tools for preparing the much-needed shells and munitions) or thorium for our gas-mantles, without

optical glass or chemical glass-ware, short of artificial manures, with no plant to work the complex zinc ores from Australia, and with only a mere nucleus of trained chemists to undertake the production of these and a hundred other chemical substances essential for the national welfare both in peace and war.

Fortunately Roscoe and Meldola lived also to see the first faint rays that seemed to betoken the dawning of a new day for chemistry, but both passed away before it could be decided whether it was a real or a false dawn.

The chief ground on which both chemists met was that of technical education, and in many addresses and articles in newspapers and journals they warned the country of that decadence of chemical industry which they saw going on around them.

Sir Henry Roscoe had particular opportunity of observing how industries were faring in this and other countries as a member of the Royal Commission on Technical Instruction in 1881.

The results arrived at, and the Report issued by the Commission, were of such importance, and of such interest to-day, that it is worth noting one or two of the more salient features of the Report.

Sir Henry Roscoe was invited in 1881 by Mr. (later Sir) A. J. Mundella (then Vice-President of the Education Council) to become a member of Sir Bernhard Samuelson's Commission on Technical Instruction.

As Sir E. Thorpe reminds us at the beginning of Chap. X., this was " one of the most important Commissions ever issued by reason of its influence on the industrial history of this country. Roscoe threw himself heart and soul into its work. The task was thoroughly congenial to him, for he was profoundly convinced of its importance. It required long and frequent visits abroad in order to inquire into the methods of the continental trade-schools and polytechnics, and to judge by direct observation of their results. The preparation of the Report was a tedious and complicated business, but with the help of his colleagues, whom he invited to his holiday home in the Lakes, it was gradually, as he says, 'licked into shape,' the last touches to its recommendations being made at the chairman's country house in Devonshire."

The Commission was in many ways a singular one, not the

least curious point being the fact that the members of the Commission had to pay their own expenses, the Government being responsible only for the printing and publication of the Report and the secretarial expenditure. Prof. Roscoe's colleagues on this Commission were Mr. (the late Rt. Hon. Sir) Bernhard Samuelson, the late Mr. John Slagg, M.P., Mr. (now Sir) Swire Smith, Mr. (now Sir Philip) Magnus, and the late Mr. William Woodall, M.P., the Secretary being Mr. Gilbert Redgrave of South Kensington.

It is of course impossible to do more than sketch some of the conclusions published in the 557 pages of the Report (Vol. I. Second Report), but it can safely be said that if all the recommendations had been adopted, this country would not have found itself so hard pressed in many ways at the outbreak of the present war as was actually the case.

The chief result arrived at was that certain foreign countries, particularly Germany and Switzerland, were getting ahead of us in many of the more technical branches of industry largely owing to the great attention paid abroad to technical education and the relatively poor provision made in this country.

Although various hands were concerned in the issue of the Report, there is no difficulty in observing the impress of Roscoe's vigorous personality upon it, more especially as regards the position of chemistry at home and abroad. Two sections in particular, probably that on "The Influence upon Industry of Institutions for Scientific Research" on page 219, and certainly that on "The Influence of Technical Instruction on certain branches of Chemical Industry," were specially written by Sir Henry Roscoe for the Report, and the contrast between the British and German way of running a chemical factory in those days is clearly expressed :

"The first principle which guides the commercial heads of the continental colour-works is the absolute necessity of having highly trained scientific chemists, not only at the head of the works, but at the head of every department of the works where a special manufacture is being carried on. In this respect this method of working stands in absolute contrast to that too often adopted in chemical works in this country, where the control of the processes is left in the hands of men whose only rule is that of the thumb, and whose only knowledge is that bequeathed to them by their fathers."

The Commission made many important recommendations as regards technical education and, to quote again from Sir Edward Thorpe:

"During the ten years that followed the publication of the Report, Roscoe, in common with several of his colleagues, addressed innumerable public meetings throughout the country in order to make its lessons as widely known as possible. The work of the Commission bore fruit in the Technical Instruction Act of 1889, and still later and to a greater extent in the Education Act of 1902. . . . Although more than thirty years have passed since the Report of the Technical Instruction Commission was issued, it may still be read with profit. Indeed the lessons it teaches are singularly applicable to the present juncture. In spite of what has been accomplished, Roscoe was far from being satisfied with our national position. In 1906 he wrote:

"Much remains for us in England to accomplish in the organisation of our secondary and scientific training, in which our competitors are before us, and of which the importance and the effects are well summed up in the following opinion of an eminent German manufacturer:

'We in Germany do not care whether you in England are Free Traders or Protectionists, but what we are afraid of is that some day your people will wake up to the necessity of having a complete system of technical and scientific education, and then with your energetic population, with your insular position, and with your stores of raw material it will be difficult, or it may be impossible, for us to compete.'"

"In 1884 a knighthood was conferred on him, as stated in Mr. Gladstone's letter when intimating the Queen's pleasure, 'in acknowledgment of his distinguished service on the Technical Education Commission.'"

Regarding the other activities of Sir Henry Roscoe as a member of Parliament, his share in the founding of Manchester University, and of the Society of Chemical Industry, we must refer the inquirer to the biographical sketch which is the subject of this review.

Unfortunately, owing to the limitations of a review, one cannot write in detail as to the educational work of Prof. Meldola beyond referring to his numerous warnings in lectures delivered at the Royal Society of Arts, or in his Presidential addresses to the Chemical Society, the Society of Chemical Industry, and the Society of Dyers and Colourists, in which he referred in particular to the decadence of the British coal-tar

industry, but it will suffice to call attention to the most interesting bibliography of his works contained at the end of the volume of reminiscences. It is to be hoped that before long we shall see a collected addition of Prof. Meldola's publications, so that posterity may the better form an accurate opinion of the varied activities of this remarkable man.

Both Roscoe and Meldola belonged to the highest type of manhood and afford brilliant refutations of the alleged incompatibility of high scientific attainments and public service.

Of Roscoe no less than of Meldola we may echo the words of Lord Moulton on the latter:

"It may be said of him that he represented fully the type of intellect and personality which must be developed amongst us if this war is not to be a mere incident in the long-drawn-out decline of our national greatness. We must have leaders and followers who, like him, are inspired with an equal devotion to knowledge and to the utilisation of knowledge."

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By Philip E. B. Jourdain, M.A. Cambridge.

The two chief alterations that strike one in the new (third) edition of the *Index du Répertoire Bibliographique des Sciences Mathématiques* (Amsterdam and Paris, 1916), which has just been published by the Mathematical Society of Amsterdam, are in the classes devoted to the general theory of functions (D) and the theory of differential and functional equations (H). Instead of the old subdivision of the class D into "theory of functions from the points of view of Cauchy, Weierstrass, and Riemann" (D3—D5), the new edition substitutes sections: "General properties of analytic functions of one variable"; "Conformal representation and analogous subjects"; and "Functions of many complex variables." In the class H, the section e on "Particular functional equations" is amplified with subdivisions on: "Linear integral equations (resolving kernel; fundamental functions)"; "Non-linear integral equations"; and "Integro-differential equations." It may be remarked that D1 e (in the class devoted to functions of real variables) is a new section on "Functions of lines and their applications in the theory of integral and integro-differential equations," with a reference to the section (H11 e) just spoken of; but there is no cross-reference in the section H11 e to D1 e, which would probably be convenient to readers. The *Index* is adopted in that exceedingly useful publication the *Revue semestrielle des publications mathématiques*, as well as in some other publications in Great Britain and France, and this new edition is still more useful than the preceding one in the classification of modern mathematical work.

The first of the three parts of the *Jahrbuch über die Fortschritte der Mathematik* for 1913 was published at the end of 1916. It may be remarked that efforts are now being made to wake up the nations of the Entente to the advisability of their not being dependent, owing to their inertia, on Germany for the good work of organising the results of scientific research (see E. Rignano, *Nature*, 1917, 98, 408–9, 413–14).

Among the mathematicians who have died in 1916, a place must be given to Ernst Mach, late Professor at the University of Vienna, who died on February 22, 1916, at the age of seventy-eight years. The most profound influence which Mach exerted on his contemporaries was through his historical and critical studies of various branches of physics. His best-known work is his famous *Mechanics*, which well shows his mathematical skill and the great enlightenment that his singularly unprejudiced view of science brought about. On July 10 Emory McClintock, who is fairly well known from his contributions to the *American Journal of Mathematics*, died at the age of seventy-six years. A notice of the sudden death of C. S. Jackson is given in *Nature* (1916, **98**, 173); Pierre Duhem of Bordeaux died in September 1916, at the age of sixty-seven; and S. B. MacLaren died of wounds on August 14, 1916.

At the Congress of Mathematicians which met at Cambridge in 1912, it was decided to hold the next (the sixth) Congress at Stockholm in 1916. Of course it was impossible to hold this Congress on account of the war, and so a meeting of mathematicians from Sweden, Denmark, Finland, and Norway was held at Stockholm from August 30 to September 2, 1916.

Dr. Caroline E. Seely (*Bull. Amer. Math. Soc.* 1916, **22**, 31–4) gives a translation of part of the Mittag-Leffler testament relating to the proposed Scandinavian Institute of pure mathematics which was referred to in SCIENCE PROGRESS (1916, **11**, 266).

History.—The first volume of the collected works of Paolo Ruffini has been published under the auspices of the Circolo Matematico of Palermo (*Opere matematiche*, Palermo, 1915), with notes by Prof. Ettore Bortolotti. Historically, Ruffini occupies an important place in the development of the theory of groups between Lagrange and Abel, as was pointed out by Burkhardt; and his most important work, the *Teoria generale delle equazioni, in cui si dimostra impossibile la soluzione algebraica delle equazioni generali di grado superiore al quarto*, is included in the present volume.

Felix Klein (*Math. Ann.* 1916, **77**, 303–6) gives an eleventh report on the position of the publication of Gauss's *Werke*. An eleventh volume is planned, which is to contain

a general biography, a list of all the manuscripts that Gauss left behind him, a history of the publication of the *Werke*, and an index.

The latest set of letters from Casorati to Schläfli which J. H. Graf has published (*Boll. di bibl. e st. delle sci. mat.* 1916, **18**, 113–21) is of greater mathematical interest than most of the preceding ones. In one letter, Gauss's measure of curvature of surfaces is discussed.

Emil Lampe communicates (*Archiv der Math.* (3), 1916, **24**, 193–220, 289–310) some letters written by Charles Hermite to Paul du Bois-Reymond during the years 1875–88.

The fifth volume of Weierstrass's *Mathematische Werke*, containing his lectures on the theory of elliptic functions, was published at Berlin in 1915 under the editorship of Johannes Knoblauch. In that year, which was the centenary of Weierstrass's birth (see an address by Lampe in *Jahresber. der Deutschen Math.-Ver.* **24**, 416–38) occurred the death of Johannes Knoblauch (see R. Rothe, *ibid.* 443–57). Rothe also gives a report (*ibid.* 439–42) on the present state of the editing of Weierstrass's *Werke*.

Logic and Principles of Mathematics.—Henri Dufumier (*Rev. de Métaphys. et de Morale*, 1916, **23**, 623) examines how the calculus of classes in logic has been built up, and attempts to show that it only takes a systematic form if we consider it as a generalisation of the theory of aggregates in mathematics. Dufumier's object is to show how excellent a thing it is for logic and mathematics, which have developed quite separately, to be inseparable.

In 1915 Hugo Dingler, whose name was brought prominently before many people by the very laudatory reference to him in the preface of the last edition of Mach's *Mechanik*, published at Munich a book entitled *Das Prinzip der logischen Unabhängigkeit in der Mathematik*. References to some reviews of it are given in the *Rev. semest.* (1916, **24** [2], 79).

It is now possible to give a more accurate description of the paper of K. G. Hagström which was mentioned in the last number of Science Progress (1917, **11**, 453), for the author has kindly sent a copy of it to the present writer. Hagström points out that Burali-Forti's antinomy was originally stated for "perfectly ordered classes" which at first he thought were always well-ordered but afterwards recognised were not

(*Rend. circ. mat. di Palermo*, 1897, 11, 260; the famous paper being on pp. 154–64); Burali-Forti then assumed that all perfectly ordered classes were ordinally comparable, and thus that *any* perfectly ordered class is ordinally similar to a segment of an aggregate Ω. But the perfectly ordered aggregate formed by adding on an element to the end of Ω cannot be ordinally similar to a segment of Ω. Hagström now shows by an example that, though a well-ordered aggregate is also perfectly ordered, a perfectly ordered one is not necessarily well-ordered, but may contain a part of inverse type. Such an aggregate is not *ordinally* comparable with a well-ordered aggregate, and so certain perfectly ordered aggregates are not ordinally comparable. This, now, is what Burali-Forti assumed. But even granting that Burali-Forti made a slip in thinking that his aggregates were well-ordered, the interesting fact remains that the antinomy subsists when we consider well-ordered aggregates instead of perfectly ordered ones, and Hagström, at the end of his paper, indicates this as a problem which must be investigated. Since the author, unlike all mathematical logicians, has missed the interesting part of Burali-Forti's discovery, it is unfortunate that he should condemn mathematical logic for obscurity. We may add that Burali-Forti (*ibid.* 156–7) originally misunderstood what Cantor meant by "well-ordered" in quite another way and thought that a "perfectly ordered class" was well-ordered but not necessarily *vice versa*, that Ω is the type (*ibid.* 163), defined by abstraction, of the class of ordinal numbers (types of perfectly ordered classes) and that this class is perfectly ordered. Since, then, $\Omega + 1$ could be proved to be both greater and less than Ω, it seemed to follow that there are at least two types which are not ordinally comparable. Later (*ibid.* 260) Burali-Forti corrected his mistake as to Cantor's meaning, pointed out that his reasoning was concerned with perfectly ordered aggregates and not with what he thought that Cantor called "well-ordered aggregates," and concluded: "It easily follows that every well-ordered class is perfectly ordered, but not *vice versa*. The reader can verify that the propositions of my note are true for well-ordered classes."

In the twelfth volume (1915) of the *Periodico di Matematica* there are papers on the concept of real number by C. Mineo (1–15) and A. Maccaferri (87–9), articles by A. Palomby on

the arithmetical-philosophical concept of equivalence and of repetition and on the concepts of number and order (228–34, 235–8; cf. A. Nattucci, 282–3), and by F. Palatini on infinitesimals and the actual infinite (127–33).

Arithmetic and Algebra.—W. J. MacMillan (*Amer. Journ. Math.* 1916, **38**, 387–96) gives a theorem connected with irrational numbers consisting in the fact that the limiting value of the geometric mean of certain positive real numbers is zero or 1/2, according as a certain number in the function is rational or irrational.

A. J. Kempner (*Trans. Amer. Math. Soc.* 1916, **17**, 476–82) gives a new form of infinite series which defines transcendental numbers, and which is of great interest by the side of the series of Liouville, E. Maillet, and G. Faber. It is interesting that his theorem shows that the important function which Fredholm introduced in 1891 has transcendental values for an infinite set of real rational values of the argument having the origin as a limiting point.

E. Noether (*Math. Ann.* 1915, **77**, 103–28), in a paper on the most general regions of whole transcendental numbers, constructs a region of these numbers, which is to have certain properties, by means of Zermelo's theorem on well-ordering aggregates.

A. Perna (*Giorn. di Mat.* 1915, **53**, 46–93) continues his paper on the construction of transcendental numbers, and gives an exposition of the investigations of others on the arithmetical properties of transcendental functions.

H. B. Fine (*Proc. Nat. Acad. Sci., Washington, D.C.*, 1916, **2**, No. 9) gives a condition under which Newton's method of approximation for calculating a real root of an equation will with certainty lead to such a root, and gives an analogous investigation for the extension of this method to a system of equations.

H. Taber (*Amer. Journ. Math.* 1916, **38**, 337–72) gives conditions for the complete reducibility of groups of linear substitutions.

O. E. Glenn (*Trans. Amer. Math. Soc.* 1916, **17**, 405–17) treats of a transvectant operation between general connexes in which the variables are ternary and contragredient which gives all invariant formations of the simultaneous systems of the connexes. His method bears a closer resemblance to

that of Clebsch and Gordan (1869) than to subsequently developed methods. Glenn also (*ibid.* 545–56) studies the functions of the a's and x's in a binary quantic of order m whose coefficients are variables which are invariantive under the operation of transforming this quantic by the group represented by the general binary linear transformation in which the coefficients are parameters representing residues of a prime number.

Analysis.—P. Nalli (*Giorn. di Mat.* 1915, **53**, 169–77), in connection with some researches of Denjoy, proves a theorem on the generalised second derivatives of a function. The theorem of course throws light on the question of the development of a function in a trigonometric series.

A. Denjoy (*Bull. Soc. Math. de France,* 1915, **43**, 161–248) gives a paper on summable derived functions in which the conception of " approximate continuity " of a function at a point is introduced and plays an important part.

In 1911, C. N. Moore gave a method for establishing the convergence or uniform convergence in series of Bessel's functions, and determined the values to which they converge by long and complicated considerations. Moore now gives (*Bull. Amer. Math. Soc.* 1916, **23**, 18–27) a fairly simple method of determining the value of a development when we know it converges, " and consequently this paper combined with the previous one gives the first *complete* discussion of the convergence and value of the developments in Bessel's functions under conditions that are usually satisfied in the applications."

G. A. Bliss (*ibid.* 35–44) gives a very careful analysis of W. F. Osgood's Madison Colloquium Lectures entitled *Topics in the Theory of Functions of Several Complex Variables* (New York, 1914).

The general definition of " adjoint systems " of boundary conditions associated with ordinary linear differential equations was given by Birkhoff in 1908, and the idea was further developed by Bôcher in 1913. Bôcher obtained a condition that a system of the second order be self-adjoint. Dunham Jackson (*Trans. Amer. Math. Soc.* 1916, **17**, 418–24) extends the discussion of this problem to the case of systems of any order. He expresses a condition for self-adjointness of the boundary conditions in matrix form without any requirement that a corresponding property be possessed by the differential equations.

Very many mathematicians from the time of Gauss onwards have dealt with the problem of setting up criteria by means of which the irreducibility of certain expressions in certain domains may be seen at a glance from the character of the expressions. H. Blumberg (*ibid.* 517–44) gives a general theorem and certain immediate consequences of it which include as special cases, with minor exceptions, all the results hitherto obtained. A former note of 1915 had given a summary of part of this paper, but since then the author has found how to unify and generalise his previous results.

W. E. Milne (*Proc. Nat. Acad. Sci., Washington, D.C.*, 1916, **2**, No. 9) gives more precise formulæ than those of Birkhoff when discussing asymptotic expressions in the theory of linear differential equations.

In the case of n functions of a single variable, the vanishing of the Wronskian is the most familiar criterion for their linear dependence. The Wronskian also plays an important part in connection with the theory of a single ordinary homogeneous linear differential equation of the n^{th} order. G. M. Green (*Trans. Amer. Math. Soc.* 1916, **17**, 483–516) generalises the fundamental facts connected with these subjects to the case of functions of several variables.

E. Pascal (*Giorn. di Mat.* 1915, **53**, 318–48) gives a systematic exposition of the analogy between the functional calculus and the ordinary calculus.

A. A. Bennett (*Annals of Math.* 1915, **17**, 23–60) discusses the iteration of power series and of a real function of one variable, and then gives some properties of certain rational functions under iteration. Bennett also (*ibid.* 74–5) defines an operation of the " third and higher grades "—addition being an operation of the " first grade " and multiplication one of the " second grade "—by means of the iteration of the function e^x.

Edward B. Van Vleck (*Bull. Amer. Math. Soc.* 1916, **23**, 1–13) gives an exceedingly interesting address on current tendencies of mathematical research. His view is that " the problem of the infinite set " is the most conspicuous in modern pure mathematics, and his address is devoted to showing some of the forms in which this problem appears. Generalisation to an enumerably infinite number of variables is illustrated by the passage from a set of ordinary linear algebraic equations to a

linear integral equation, which gave to Volterra and Fredholm the basis of their theory of integral equations; and the theorem discovered independently by Fischer and Riesz is discussed at some length as illustrating most of the modern tendencies of which Van Vleck speaks. Instead of representing a known function by a Fourier series, Fischer and Riesz sought to represent a given Fourier series, or rather its generalisation into a series of orthogonal functions, by a function, under certain precise definitions as to what is meant by the word "represent." Lebesgue integrals were used throughout, and the necessary and sufficient conditions, which were found by Fischer and Riesz, can be expressed very simply. Modern mathematical research is characterised by its tendencies to invert (thus following what is said to have been Jacobi's advice to his pupils) and generalise problems by taking into consideration an infinite number of variables, by basing a theory (as Riemann did) on a property rather than on an algorithm, by the presence of existence-theorems, and by the examination of the postulates of pure and applied mathematics. It is very much to the point that Van Vleck emphasises that functions of infinitely many variables appear in important questions of mathematical physics; thus, the potential due to an electric current in a wire is a function of the shape of the wire, and thus depends upon the non-enumerably infinite number of its points.

We may here add that even such minute examination of the principles of physics as that of Whitehead (see SCIENCE PROGRESS, 1917, **11**, 454) is necessary, for otherwise we cannot tell if our hypotheses are consistent. Thus, if we hold to the hypothesis that no point can belong to more than one body, we have the necessary result that there is no such thing as contact of bodies with surfaces and that, if two bodies are to be in contact, one of them (and only one) must be deprived of a part of its surface.

G. Andreoli (*Giorn. di Mat.* 1915, **53**, 97–135) writes on integral and integro-differential equations of a more general type than those considered by Volterra and Fredholm. G. Vivanti (*ibid.* 209–11) gives an elementary demonstration of a fundamental formula in the theory of integral equations.

M. Fréchet (*Bull. Soc. Math. de France*, 1915, **43**, 248–65), in a paper on the integral of a functional extended to an

abstract aggregate, gives a generalisation of de la Vallée-Poussin and Lebesgue's definitions of an integral.

Geometry.—G. Ascoli (*Giorn. di Mat.* 1915, **53**, 203–8) gives a non-Archimedean construction of plane homography.

L. Bieberbach (*Jahresber. der Deutschen Math.-Ver.* 1915, **24**, 247–50) proves rigorously the famous old theorem that among all plane finite domains of a given breadth the circle has the greatest content. Cf. the same author's paper in the *Math. Ann.* (1915, **77**, 153–72).

H. R. Kingston (*Amer. Journ. Math.* 1916, **38**, 407–30) lays the foundation for the study of the metric differential properties of nets of plane curves, Wilczynski having discussed their projective differential properties in 1911.

H. C. Gossard (*ibid.* 431–45) discusses whether there are lines connected with a tetrahedron such that, if the vertices are reflected in these lines, the reflections will fall on the opposite faces; and, if so, what the locus of these lines is.

In 1915 C. H. Sisam discussed sextic surfaces whose plane sections are of genus 1. In *Amer. Journ. Math.* (1916, **38**, 373–86) he considers non-ruled sextics whose plane sections are of genus 2.

L. P. Eisenhart (*Trans. Amer. Math. Soc.* 1916, **17**, 437–58) determines certain deformable transformations of Ribaucour. The subject is in close connection with some of Bianchi's work in his *Differenziale geometria* and also in papers published by him in 1906 and 1915, and with some previous (1915) results of the author.

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

Einstein's Theory of Gravitation.—In the Physics section of these notes in the last number of Science Progress (p. 461), Einstein's new theory of gravitation was referred to. Inasmuch as the only evidence which can be adduced either in favour of or against the theory must be derived from astronomical observations, the astronomical aspect of the question is of some considerable importance. The evidence at present available and the directions in which further evidence may be obtained are accordingly summarised below. The metaphysical aspect of the theory and the nature of the new law of gravitation are not considered here, as limitations of space would not permit.

It is one of the consequences of Einstein's theory that since electromagnetic energy possesses inertia, it must be subject to and must exert gravitational attraction in a similar manner to the inertia possessed by matter. Since a ray of light possesses electromagnetic inertia, it follows that when a ray of light passes through a gravitational field, its path will in general be curved owing to the gravitational action of the field. The curvature produced is extremely small; in fact, if we consider a ray of light coming from an infinite distance and passing at a distance r from a body of mass M, the ultimate deflection produced in the direction of the path of the ray is $4\gamma M/c^2r$ where γ is the constant of gravitation, $6{\cdot}7 \times 10^{-8}$ in C.G.S. units and c is the velocity of light, 3×10^{10} cms. Thus M must be enormously great in order that a measurable deflection may be obtained. The only means of testing this result of the theory is in the case of a star seen close to the limb of the Sun, when the apparent displacement due to the sun's gravitational action should be 1·75 seconds of arc. This is a very small quantity when the difficulty of observing a star close to the limb of the sun is considered, and the deflection produced decreases proportionately to the increase in the distance of the star from the sun. A. F. and F. A. Lindemann (*M.N., R.A.S.*, vol. lxxvii. p. 140) have discussed the possibility of testing the hypothesis by photography of stars in daytime. They find that by using red colour filters and plates specially sensitive to the red it is possible to photograph bright stars at some distance from the sun in broad daylight; further experiments carried out on similar lines at an observatory enjoying a fine climate and a clear atmosphere might result in stars being photographed sufficiently near to the sun for the theory to be tested. The only other and the better alternative is to use the favourable opportunity provided by a total solar eclipse; unfortunately total eclipses are somewhat rare phenomena and bright stars are not always sufficiently near the sun at the time of eclipse for the test to be made. A unique opportunity will occur, however, in the eclipse of 1919, May 29, visible in Peru, Brazil, and Central Africa, when the sun will be in the crowded region of the Hyades and many fairly bright stars will be available for measurement. With a telescope of sufficient power the relative displacement of about three seconds between two stars one on either side of the sun will

readily be measurable. Given fine weather, this eclipse will offer a splendid chance for testing this result of the theory, and it is to be hoped that one or more expeditions will be sent for the purpose of making these observations.

Another consequence of the theory is that atomic vibrations are executed more slowly in an intense field of gravitation: therefore the spectral lines of the sun or of any star should be displaced to the red as compared with the positions of the corresponding lines obtained from a terrestrial source. The displacement in the case of the sun is equivalent to the Döppler displacement produced by a radial velocity of 0·63 kms. per sec., and for a star whose mass and density in terms of the sun are M and ρ the displacement would correspond to a velocity $0{\cdot}63\,M^{\frac{2}{3}}\rho^{\frac{1}{3}}$. Attempts have been made at the Mt. Wilson Solar Observatory to detect this displacement in the case of the sun, but no conclusive results have yet been obtained. This is not surprising in view of the number of other causes which tend to produce displacements, such as pressure and density effects, possible effects due to anomalous dispersion and the mutual influence of spectral lines and to lines originating at different levels in the sun's photosphere, etc. It is not probable that much information will be obtained in this direction either for or against the theory.

A more important test is derived from the application of the theory to planetary motions. The new law of gravitation is equivalent to Newton's inverse square law to a first approximation, but a second approximation introduces some small additional terms into planetary theory. The only effect which is of any importance and comes within reach of verification by observation is a secular motion of the planetary perihelia: the theory requires that a planetary orbit should rotate in the direction of motion with an angular velocity $24\pi^3a^2/c^2T^2(1-e^2)$ radians per revolution of the planet, where a, T, e are the semi-axis, period, and eccentricity of the orbit. This gives in the case of Mercury a rotation of forty-three seconds per century, which is *exactly* the amount of the discordance observed and which has remained for so long an unsolved puzzle. The new theory has thus obtained a striking success. The following comparison between the observed values of the secular motions of the perihelia ($e\,d\varpi$) and of the nodes ($i\,d\Omega$) of the inner planets with the values calculated (1) from Newcomb's tables,

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including the terms introduced by the new theory, and (2) from the tables without these terms, is given by de Sitter.

		Observed.	Theory.	Difference (1).	Difference (2).
Mercury	$e d\tilde{\omega}$	$+118''\cdot 00$	$+118''\cdot 58$	$-0''\cdot 58 \pm 0''\cdot 43$	$+8''\cdot 48$
	$i d\Omega$	$-92\cdot 04$	$-92\cdot 50$	$+0\cdot 46 \pm \cdot 52$	$+0\cdot 61$
Venus	$e d\tilde{\omega}$	$+0\cdot 28$	$+0\cdot 39$	$-0\cdot 11 \pm \cdot 25$	$-0\cdot 05$
	$i d\Omega$	$-105\cdot 47$	$-106\cdot 00$	$+0\cdot 53 \pm \cdot 17$	$+0\cdot 60$
Earth	$e d\tilde{\omega}$	$+19\cdot 46$	$+19\cdot 46$	$0\cdot 00 \pm \cdot 13$	$+0\cdot 10$
Mars	$e d\tilde{\omega}$	$+149\cdot 44$	$+148\cdot 93$	$+0\cdot 51 \pm \cdot 35$	$+0\cdot 75$
	$i d\Omega$	$-72\cdot 64$	$-72\cdot 63$	$-0\cdot 01 \pm \cdot 22$	$+0\cdot 03$

The agreement of the new theory is thus very satisfactory and generally better than the old.

The influence of the new terms introduced by Einstein's theory into the theory of the moon's motion have also been discussed by de Sitter. No results decisive either for or against it are obtained. He finds a slight influence on the motions of the lunar perigee and node of amount $+1''\cdot 97$ and $+1''\cdot 91$ respectively per century. The observed values are:

Brown, Cowell . . .	$d\tilde{\omega} = +14643536'' \pm 2''$
Newcomb	$\text{''} = +14643530'' \pm 2''$
Newcomb, Brown . .	$d\Omega = -6967944'' \pm 2''$

The new theoretical values are:

$$d\tilde{\omega} = +14643534'' \pm 2''$$
$$d\Omega = -6967939'' \pm 2''$$

The residuals

$$\Delta\tilde{\omega} = \begin{cases} +2'' \pm 3'' \\ -4'' \pm 3'' \end{cases} \qquad \Delta\Omega = -5'' \pm 3''$$

are of the order of the probable error and thus give no information, this being also the order of the terms added by the new theory. When the theoretical motion can be more accurately determined, some information as to the truth of the theory may be provided in this manner.

De Sitter also considers the effect of the gravitational field of the fixed stars on the stellar spectral lines and shows that the result will be to produce a slight systematic displacement of the lines towards the violet. In this way it may be possible to derive an approximate upper value for the total mass of the stars within a given distance from the sun. This may provide a rough indication of the relative numbers of the bright and dark stars, since the latter contribute to the gravitational

field and the mass of lucid stars can be estimated to a certain degree of accuracy and their number approximately determined.

It will be seen, therefore, that Einstein's theory has scored one signal success in giving an exact quantitative explanation of the anomaly in the motion of the perihelion of Mercury, for which the older theory of gravitation could not account. The next test will doubtless be obtained from observations of stars close to the limb of the sun, and a positive result to this test will establish the theory on a very secure basis. Gravitation has for so long resisted all attempts at explanation or reduction to a theory, partly owing to the difficulty of testing any theory, that it is with peculiar satisfaction that we contemplate the solution at last of the problem. It may be added that, according to the theory, gravitation is propagated with the velocity of light.

References: Einstein und Grossmann, *Zeitschr. für Math. u. Physik*, 1914, Jan.; Einstein, *Sitzungsber. Berlin*, 1914-16 (several papers) and *Ann. der Physik*, xlix. p. 769, 1916; J. Droste, *Proc. Acad. Sci. Amsterdam*, xliii. p. 998, 1914 and later; Schwarzschild, *Sitzungsber. Berlin*, 1916, p. 189; de Sitter, *Proc. Acad. Sci. Amsterdam*, xix. June and Sept. 1916, and *M.N.*, *R.A.S.*, lxxvi. p. 699, 1916, lxxvii. p. 155, 1916, and *Observatory*, xxxix. p. 412, 1916.

By James Rice, M.A., The University, Liverpool.

The Bakerian Lecture of last year was delivered by Prof. Barkla, and is printed in the *Philosophical Transactions*. An abstract of the address will be found in the *Proc. Roy. Soc.* (A, 643, Aug. 1916). Dr. Barkla took as his subject "X-Rays and the Theory of Radiation." The processes of emission of X-rays may be considered under three heads: (1) Scattered X-radiation, (2) Fluorescent or Characteristic X-radiation, and (3) Primary X-radiation. Experiments on the quality, polarisation, distribution, and intensity of the scattered radiation from any given substance show that it is emitted by electrons whose motions, in so far as they are responsible for the scattered radiation, are very completely controlled by the primary radiation. Of late the quantum theory of radiation (viz. that radiation is emitted discontinuously and in quantities which are multiples of a definite unit and which make their exit from the atom "catastrophically" when it is in a critical condition) has received considerable support and has resolved some difficulties attending the older view of continuous radiation. Now

the unmodified theory of electromagnetic radiation when applied to the emission of scattered X-radiation shows that atoms contain electrons in numbers equal to half their respective atomic weights, a view which has been confirmed by other lines of research and is generally accepted. Dr. Barkla pointed out that this result admits of no possible explanation other than the assumption that every electron in the radiating matter is disturbed by every wave of the primary radiation; that the emission of scattered radiation is a continuous process, not dependent on any critical condition of the atom; that it may take place in any quantity, and that not only is there no suggestion of a quantum or entity of radiation, but the phenomena become meaningless on any such theory. Further recent work has demonstrated that scattering takes place by groups of electrons rather than by individual electrons, when the wave length is comparable to the size of a group.

In contrast with the behaviour of the scattered radiation, the characteristic or fluorescent X-radiation displays a definite quality, an absence of polarisation effects, a uniformity of distribution, and a variation of intensity with the wave length of the exciting primary radiation which proves that this process of radiation is absolutely uncontrolled by the primary rays. It arises only indirectly from the passage of the primary beam and is the accompaniment of an exceptional disturbance of a radiating atom, which is in fact the ejection of a high-speed electron. As the character of the radiation, however, and its intensity are independent of the speed of ejection of the electron or its subsequent career, it appears that the radiation does not originate outside the atom thus bereft of an electron, nor does it originate in the electron at all. The origin is to be found in an atom from which an electron has been expelled. When measurements are carried out on the absorption of energy from the primary beam and the emission of energy in the form of characteristic radiation (the energy of scattered radiation being allowed for) and in the form of corpuscular or electronic radiation, results of considerable interest for the quantum theory of radiation are discovered. The various qualities of characteristic X-radiation from one and the same substance (which can be excited, as is well known, by primary rays of suitable hardness) are as usual denoted by the terms K fluorescent radiation, L fluorescent radiation, etc. (the K being the hardest). An

electron to whose ejection the emission of K fluorescent radiation is the accompaniment is called a K electron, etc. Prof. Barkla summarises the results of energy measurements thus: (1) The energy of primary radiation required to expel a K electron is greater than that required for the expulsion of an L electron by the energy of a quantum of K fluorescent radiation. The energy of a quantum of K radiation is therefore the energy required to move an electron from the position and state of a K electron in the atom to the position and state of an L electron. (2) The energy of primary X-radiation absorbed by each atom from which an electron is expelled varies from just less than two quanta of primary radiation to just more than one quantum as the wave length of the primary radiation diminishes and the amount of its quantum increases. This indicates therefore that absorption does not take place by quanta, thus supporting the later view of his own theory adopted by Planck, that while emission of radiation is by quanta, absorption is continuous. (3) One quantum of its K fluorescent radiation is emitted for each K electron ejected, and the emission is subsequent to the ejection.

These experimental results, though they may have to be modified by the result of still further research on a greater number of substances, afford conclusions which are in accordance with the quantum theory of radiation as far as characteristic radiation is concerned, and they offer some support to the theory of atom structure put forward by Bohr. Bohr regards electrons in an atom as possessing certain stable orbits round which they travel with a definite amount of energy subject to ordinary dynamical laws. The electrons, however, have the property of flying from one orbit to another or right out of the atom owing to the attainment of some critical condition. In flying from an outer to an inner orbit, there is an emission of a quantum of radiation of a definite frequency; in passing from an inner to an outer orbit there is absorption of energy. Prof. Barkla views the processes of characteristic radiation as, first of all, the ejection of a K electron from its stable orbit owing to absorption of energy from the primary beam, followed by the dropping into its place of an electron from an L electron circuit, or one still further out. It is in the latter occurrence which gives rise to the characteristic radiation, and the energy previously absorbed is now (except for the amount carried away as

kinetic energy of the ejected electron) re-emitted as a quantum of K characteristic radiation, or perhaps as a quantum of each of the K, L, M, etc., radiations.

One of the results of recent research has been to confirm the evidence afforded by earlier work that there are characteristic radiations emitted by the light elements harder than the K series. To these radiations Dr. Barkla gives the name J series, and he has discovered their emission from carbon, nitrogen, aluminium, and sulphur. Thus the J radiation emitted from nitrogen is about as penetrating as the K radiation from the much heavier element silver, while the J radiations from aluminium and sulphur are more penetrating still.

As regards the bearing of his work on the quantum theory, Dr. Barkla states that though X-radiation may be, and is, emitted by electrons singly or in groups as a continuous process and in any quantity whatever, it is also emitted in quanta from atoms when certain critical conditions are reached. X-radiation is absorbed in quantities which are small compared with a quantum in certain processes. In other processes it is absorbed in quantities larger than a quantum of the primary radiation. There is no evidence of absorption of X-radiation in whole quanta. In fact, X-ray phenomena do not support the quantum theory of radiation as generally understood. They indicate that the quantum is a unit of atomic energy rather than of radiation, and that this unit necessarily appears in certain processes of radiation and absorption. For example, the energy emitted at the expulsion of a K electron is a quantum because it represents that unit of atomic energy which is required to bring a K electron into the position and state of an L electron.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Osmotic Pressure.—Although the concept of osmotic pressure has played an outstanding part during the last thirty years in the development of physical chemistry, it is a remarkable fact that even at the present time no very general agreement has been reached as to the actual mechanism of the phenomenon itself. The view that osmotic pressure is a simple bombardment pressure identical in nature with the pressure exerted by a gas, the dissolved substance functioning as a gas and bom-

barding the semipermeable membrane, has long been held, though not without considerable scepticism on the part of those who rightly maintained that a solution was after all a liquid in the ordinary sense of the term, and that in a system of this nature we have to deal with forces the magnitude of which is extremely large. Undoubtedly the simple bombardment view owes its popularity to the very simple and obvious explanation which it offers of the fact that the gas law holds good for the dissolved substance in dilute solution, a fact first discovered by the eminent Dutch physical chemist van 't Hoff. On the other hand attempts have not been wanting to show that van 't Hoff's law is the limiting expression of a more complicated law which can be derived by regarding the phenomenon of osmotic pressure as primarily connected with the *solvent* and only in a secondary sense with the dissolved substance. One of the most recent attempts in this direction, and one which is at the same time eminently successful, is that of Tinker, briefly outlined in the *Phil. Mag.* vol. xxxii. p. 295, 1916, and the *British Assoc. Rep.*, 1916. Tinker bases his considerations on the Dieterici equation of fluids. According to Dieterici a pure liquid possesses what may be called a liquid pressure—when the liquid is acting as a solvent we may call this pressure the solvent pressure—denoted by the symbol π, which is connected with the free or unoccupied space in the liquid and with the temperature by the ordinary gas law. Thus if we denote the volume of the liquid by V and the occupied space by b, then the free space is $V-b$ and the pressure π is related to this free space by the equation $\pi (V-b) = RT$.

This solvent or liquid pressure is itself a bombardment pressure due to the kinetic energy of the molecules of the liquid, the pressure being exerted on any imaginary unit area in the interior of the liquid. It is a quantity which cannot be measured directly, for any direct measurement involves a contact surface at which the observed bombardment pressure is very much less than π. As a matter of fact π may be several hundreds or thousands of atmospheres, whilst the bombardment pressure actually observed is exceedingly small. The vapour pressure of the liquid, in fact, gives us a measure of this latter quantity. The reason why the observed bombardment pressure is so small is, that when a molecule approaches the surface it is drawn back by strong unbalanced forces of

attraction which tend to prevent it passing across the surface. In the interior of the liquid, on the other hand, these forces, though still as strong as before, mutually destroy one another and the molecule is free to exert an enormous bombardment pressure at an imaginary plane. Exactly the same thing may be said of the molecules of the dissolved substance, though on account of their smaller concentration in the interior they exert there a correspondingly smaller pressure. At the free surface of the solution the molecules of a solute such as sugar are so effectually drawn back that even their vapour pressure is immeasurably small. It is very reasonable to assume that a similar state of things holds at the surface of a membrane enclosing the solution, and if this be so, it is evident that the bombardment pressure of the solute upon the membrane must be quite negligible, and cannot therefore account for the observed osmotic pressure.

(To be continued)

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School.

The origin and composition of coal forms the subject of a paper by D. T. Jones and R. V. Wheeler (*J. Chem. Soc.* 1916, **109**, 707). The authors are of opinion that coal has been formed by the action of pressure and heat not exceeding 300° C. on decaying vegetable matter. By extracting with various solvents coal may be shown to be composed of "resinic" and "cellulosic" constituents. Thus pyridine extracts from coal a mixture of cellulosic and resinic compounds, which may be further separated by means of chloroform in which the resinic constituents are soluble. The cellulosic constituents on destructive distillation yield phenols which constitute the chief difference between petroleum distilled from coal and natural petroleum, and this fact is construed as evidence that natural petroleum is not of vegetable origin. The cellulosic constituents also contain compounds whose molecular structure resembles that of the carbon molecule, but there is no evidence of the presence of free carbon. The resinic compounds contain free hydrocarbons, but only a small quantity of paraffins, and they also contain alkyl naphthene and unsaturated hydroaromatic residues in a highly polymerised condition. The oxygenated compounds are most probably cyclic oxides.

Some new heterocyclic compounds containing arsenic in the nucleus are described by Zappi (*Bull. Soc. Chim.* 1916 [iv], **19**, 151 and 290). By the action of dichloromethyl arsine, CH_3AsCl_2, upon the magnesium derivative of the dichloropentane $Cl(CH_2)_5Cl$, a compound of the formula

$$CH_2\begin{array}{l}\diagup CH_2—CH_2\diagdown \\ \diagdown CH_2—CH_2\diagup\end{array}AsCH_3$$

results, which may be called methylcyclopentamethylene arsine, but in view of the analogy between this compound and the corresponding nitrogen compound methylpiperidine, the author proposes to call it methylarsepedine and further suggests the name arsedine for the arsenic analogue of pyridine. Methylarsepedine is an oil boiling at 160° C.; its smell strongly resembles that of mustard oil, and it forms a methiodide, a platinichloride, and a picrate.

Observations on the digestibility and utilisation of the proteins of the egg are of some interest in view of the increasing use of raw egg white as a diet in some forms of digestive disturbances. According to Bateman (*J. Biol. Chem.* 1916, **26**, 263) raw white of egg is decidedly indigestible to dogs, rats, rabbits, and men, only from 50–70 per cent. of the substance being utilised by the body, the rest being excreted, although by prolonged feeding a certain tolerance for the protein is acquired. Raw white of egg is rendered digestible by heat coagulation or by precipitation with alcohol, ether, or chloroform, and likewise by incubation with dilute acid or alkali or by conversion into alkali metaprotein. Egg yolk, or the other hand, either raw or cooked, is quite digestible.

The chemical nature of the vitamines has been recently investigated by Williams (*J. Biol. Chem.* 1916, **25**, 437, and **26**, 431). Assuming the correctness of the view put forward by Drummond and Funk that vitamines are derivatives of pyridine, the author has prepared a number of hydroxy derivatives of pyridine, and has tested their action on pigeons suffering from polyneuritis. Definite curative effect was noticed in the case of one monohydroxypyridine, and two trihydroxypyridines, but nicotinic, quinolinic, cinchomeronic and other hydroxy acids had no action. Very interesting is the observation that the curative action is dependent on the conditions of preparation

and whether the compound is freshly prepared or not. Thus in the case of the hydroxypyridine this substance can exist in two tautomeric forms having the same melting point; one of these crystallises in needles, while the other is granular. The former alone is capable of exerting any curative effect, and was successfully employed on fourteen birds suffering from polyneuritis, but the granular form failed to act under varying conditions in the case of fifty pigeons. The needle form changes into the granular form, especially in the presence of moisture. On the other hand the granular form may be converted into the crystalline or needle form by melting slowly, cooling, and then crystallising from a mixture of benzene and light petroleum, but this change cannot apparently be effected by the animal organism. The instability of vitamines contained in foodstuffs may be due to some tautomeric change of the above nature. In a subsequent paper entitled "Isomerism in Natural Antineuritic Bodies" (*J. Biol. Chem.* 1916, **26**, 431) the author states that he has been able to isolate from autolysed yeast a substance having antineuritic properties. This substance, which melts at 345° C. if crystallised from hot water, changes its crystalline form, and thereby loses its antineuritic properties, and in this condition it has been identified as adenine. When adenine is fused or heated with alcohol in a sealed tube for three hours at 180° C. it recovers its antineuritic power. The active substance is distinguished from the inactive one by giving a blue colour with Folin's reagent, phosphotungstic acid, and sodium carbonate, and it may be concluded that the active compound is an isomeride of adenine.

In the same connection it may be noted that Abderhalden and Ewald (*Zeitsch. Gesamte expt. Med.* 1915, **5**) have found that methylglyoxaline added to the diet induces symptoms similar to those of polyneuritis brought about by a diet deficient in vitamines, but on the addition of vitamines fatal results were rare. On the other hand, vitamines produced no improvement in disorders brought about by the addition of aminomethylglyoxaline to a diet.

In the July number of SCIENCE PROGRESS reference was made in these notes to some observations by Katz on the hardening of bread. The same author has now (*Zeitsch. physiol. Chem.* 1915, **96**, 314) made the rather remarkable observation that breadcrumbs suspended in a closed vessel containing a small

quantity of any aldehyde remain soft for forty-eight hours; ketones do not exert this action.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

General and Dynamical Geology.—A. P. Coleman's presidential address to the Geological Society of America (*Bull. Geol. Soc. Amer.* 1916, **27**, 175) deals with "Dry Land in Geology," and discusses the arid and glacial land deposits of former geological epochs. He believes in the essential permanence of the continental areas, although there has been perpetual and carefully balanced oscillations on their margins.

A new reading of the much-debated Hurlet sequence of the Lower Carboniferous in the districts of Campsie and Kilsyth (Stirlingshire) is proposed by Mr. David Ferguson (*Trans. Inst. Min. Eng.* 1916, **52**, 7). He believes that the limestone identified as Hurlet in this area really belongs to a much lower horizon at the top of the Calciferous Sandstone lavas, and is separated by unconformity from the strata of the Carboniferous Limestone Series. The question is of some economic importance as the limestone horizons form datum lines for the exploration of the coal and ironstone seams of the Midland Valley of Scotland.

Mr. M. Macgregor, of the Scottish Geological Survey, has accomplished an excellent piece of work in finally settling the origin of the coarse breccias in the Jurassic of East Sutherland (*Trans. Geol. Soc. Glasgow*, 1916, **16**, 75). These consist of angular blocks of Middle Old Red Sandstone, ranging up to the enormous size of 150 × 90 × 30 feet, embedded in fossiliferous marine Jurassic muds which are often waved and contorted around the included blocks. These breccias are of very restricted distribution, and have been attributed to river or ice action, but Mr. Macgregor shows that they represent talus deposits derived from Old Red Sandstone cliffs bordering the Jurassic sea, similar to the deposits which must now be forming along the Caithness coast.

Petrology.—In a short criticism of Bowen's recent work on magmatic differentiation (*Journ. Geol.* 1916, **24**, 554) Dr. A. Harker contends for differentiation in intercrustal basins prior to intrusion rather than differentiation *in situ*, and shows that large intrusive masses (*e.g.* gabbros and granites of Skye)

which have been attributed by Bowen to the latter process, have usually been built up by successive intrusion.

Dr. A. Scott has reviewed the evidence for the occurrence of primary analcite and analcitisation in igneous rocks (*Trans. Geol. Soc. Glasgow*, 1916, **16**, 34). Working principally on Scottish rocks he shows that the relatively great abundance of analcite, its textural relations to the other minerals, and the incontestable evidence of analcitisation provided by these rocks, leaves little doubt as to the primary nature of the mineral.

The available petrographic data for the Pacific islands is summarised by R. A. Daly (*Bull. Geol. Soc. Amer.* 1916, **27**, 325). While still scanty the facts lead to the conclusion that the Pacific is underlain and has been underlain for a long time by a primary magma of basaltic composition; that pyroxene-andesites and ultrabasic types are direct differentiates of this magma; and that the alkaline types found may possibly be syntectics due to the solution of small quantities of limestone in the primary basalt.

Dr. A. Holmes describes a new collection of rocks from the Lucalla River, Loanda, which contains porphyritic basalts (like the Markle type of Scotland), nephelinite, alkaline trachytes, and augite-andesite (*Min. Mag.* 1916, **18**, 58), thus proving a further extension of the Cretaceous or Tertiary alkaline series of Angola.

A series of Archæan igneous rocks from Virginia, which is comparable to the charnockite series of India, is described by T. L. Watson and J. H. Cline (*Bull. Geol. Soc. Amer.* 1916, **27**, 193). It consists of quartz-bearing hypersthene-andesine-syenite (equivalent to the intermediate variety of charnockite), unakite (epidotic syenite), granite, norite, and pyroxenite, thus ranging, like the charnockites, from acid to ultrabasic.

Economic Geology.—In his presidential address to the Glasgow Geological Society on "The Geological Factors affecting the Strategy of the War, and the Geology of the Potash Salts" (*Trans. Geol. Soc. Glasgow*, 1916, **16**, 1), Prof. J. W. Gregory points out the strategical significance of the coal, oil, and iron-ore fields of Europe. Many of these fields are near the frontiers of the countries concerned, and have rendered inevitable the maintenance of large armies to defend them. The tenacity of the German hold on Belgium and Northern France is partly

explained by their possession of very rich coal and iron fields. Germany has moreover the practical monopoly of the cheaply mined, soluble potash salts so essential to agriculture. As Upper Alsace contains the only well-proved potash deposits which could possibly compete with the Stassfürt supplies, the regaining of Alsace by France would mean the collapse of the present German monopoly.

A shortage of glass-making material consequent upon the war has resulted in a fortunate stimulation of the investigation of British resources in glass-sands; and Dr. P. G. H. Boswell has just produced a memoir on the subject, which is published at the instruction of the Ministry of Munitions by the Imperial College of Science and Technology (*Memoir on British Resources of Sands suitable for Glass-making*, 1916, pp. 92). This work includes excellent chapters on the nature, and methods of study, of sands in general. It is shown that while very few British sands equal the Fontainebleau or Lippe sands, especially in extent or maintenance of sample, there are very large supplies of material suitable for all but the finest grades of glass. It is to be hoped that a further memoir on other refractory material, especially moulding sands, will follow. A good account of moulding sands is to be found in an article on "Admiralty Gunmetal: Foundry Equipment," by H. S. Primrose (*The Metal Industry*, 1916, **8**, 366).

A second edition has appeared of the second volume of the Special Reports on the Mineral Resources of Great Britain, *Barytes and Witherite*, by the Geological Survey. The demand for this economic work is gratifying, and should be an incentive for the Survey to branch out in other fields of applied geology.

Important work has recently been done on oil-shales, especially in the Scottish fields. In a paper on the origin of oil-shale (*Proc. Roy. Soc. Edin.* 1916, **36**, 44), Mr. E. H. Cunningham-Craig discusses the question whether there is any essential relationship between oil-rocks (impregnated with petroleum) and oil-shales (petroleum obtained by distillation). He advances the theory that oil-shale fields represent fossil oil-fields, in which inspissated petroleum has been retained by absorption or adsorption in colloidal clays, the petroleum being supplied from the porous sands associated with the clays. In support of this it is shown that the structural and stratigraphical relations of oil-shale fields, as well as many other features, may often

be paralleled by those of oil-fields. Nitrogen and sulphur compounds become concentrated in the most inspissated or weathered products of petroliferous material, and this agrees with the production of ammonium sulphate from oil-shales, but not from oil rocks. The tendency of oil-shales to become richer at the summits of anticlines also agrees with the anticlinal concentration of oil in oil-fields.

Mr. Cunningham-Craig has dealt with the problem from a geological standpoint. Mr. H. R. J. Conacher, however, in an important paper read before the Glasgow Geological Society, and to be published immediately (*Trans. Geol. Soc. Glasgow*, 1917, **16**, Pt. 2), has attacked it from a microscopic point of view. By means of an improved technique he has been able to make very thin sections of Scottish and other oil-shales, and related materials. His principal conclusion is that the oil-yielding matter consists of resin fragments (" yellow bodies," " spores," " algæ," of other investigators), which owe their external forms to their original sites in plants, or to attrition in transport, modified by subsequent pressure; whilst the internal structure has been developed by shrinkage or pressure, by flow around resistant grains, or by distortion of vacuoles or gas cavities. The objection that very little resinous material can be extracted from oil-shales by organic solvents is met by the facts that there is not only great variation in the behaviour of resins with solvents, but that their solubility decreases with the age and exposure of the material. He does not countenance the view that inspissated petroleum plays any part in the production of torbanites or the Lothian oil-shales, as it is difficult to understand how, in the case of torbanites, 10 or 20 per cent. of mineral matter could retain 80 or 90 per cent. of inspissated petroleum. Moreover rocks containing unquestionable inspissated petroleum show no structures comparable with the " yellow bodies." The Scottish oil-shales are believed to have originated as quiet estuarine mudflats, to which an abundant supply of finely macerated vegetable matter was contributed by water flowing from swamp areas.

A paper of some interest in relation to this subject is one on the chemical composition of petroleum in relation to its genesis and geological occurrence, by C. F. Mabery (*Econ. Geol.* 1916, **11**, 511). The presence of sulphur and nitrogen compounds in oil is discussed, and the author concludes that the quality of a

petroleum depends chiefly on the conditions of temperature and pressure under which it was formed, and on chemical changes due to the contemporaneous or subsequent contact with sulphur.

In a comprehensive work on the interrelations of fossil fuels (*Proc. Amer. Phil. Soc.* 1916, **45**, 21–203) J. J. Stevenson brings together the known facts regarding recent and Pleistocene peats, and Tertiary coals. He emphasises the wide area on which peat is at present being formed, an area greater than that covered by carbonaceous deposits in any other period of similar duration.

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S.

Morphology.—The interpretation of the grass embryo has long been a matter of dispute amongst morphologists. Worsdell has recently described some abnormal seedlings of maize in which the coleoptile exhibited more or less marked bifurcation of the apex (*Annals of Botany*, vol. xxx. pp. 509–25, 1916). In discussing the interpretation to be placed on this phenomenon the author deals with the general question of the morphology of the grass embryo. The conclusion is arrived at that the scutellum is to be regarded as the lamina of the cotyledon and the coleoptile, or plumular sheath, as its ligule. With Celakovsky the author regards the epiblast as comparable to the auricles present in the foliage leaves of some grasses. In such cases as have been recorded of monocotyledonous embryos with two cotyledons (*Agapanthus*, *Cyrtanthus*) the second cotyledon is explained not as a reversion but as due to excessive development of the sheath of the normally present terminal cotyledon. The forked coleoptile of the seedlings described is taken as indicative of the derivation of the ligule from a pair of stipular appendages.

Takeda (*Annals of Botany*, vol. xxx. pp. 197–214, 1916) has recently investigated a number of the four-leaved bedstraws and their allies in relation to the morphology of the nodal structure. He finds that in the development of the seedlings of species with normally six leaves at the node the four-" leaved " condition precedes the six-leaved. On the assumption that the ontogeny recapitulates the phylogeny of the group, the conclusion is arrived at that the four-leaved condition (two true leaves and two stipules) is the primitive one for the Stellatæ. In the four-" leaved " species described

the stipules frequently show indications of a double structure with two midribs or a bifid apex. Such cases may be taken as reversions to the ancestral condition of the Rubiaceæ as a whole, in which there were probably two true leaves and four stipules at each node.

Heredity.—De Vries, to whom we owe so much regarding our knowledge of mutation, has recently described (*Bot. Gaz.* 1916) four mutants of *Œnothera* in which at every generation the offspring produced are of two kinds in almost equal numbers. Those belonging to the one group resemble the parent, whilst the remainder are typical *Œnothera Lamarckiana*, thus reverting to the original stock from which the mutants sprang. From the results of crossing experiments it appears that the characters of these "dimorphic" mutants are only carried by the ovules and not by the pollen. Thus one of them (*O. cana*), when fertilised by pollen of *O. Lamarckiana*, yielded nearly equal proportions of the two types, whilst when the latter was fertilised by pollen of *O. cana* 98 per cent. were typical *Lamarckiana* whilst the remaining 2 per cent. were mutants of the *nanella* type.

Ecology and Physiology.—An extremely valuable contribution to our knowledge of osmotic pressure as influenced by the environment has been made by V. Iljiu, P. Nazarova, and M. Ostrovskaja (*Journ. of Ecology*, vol. iv. pp. 160–73, 1916).

The subject-matter of their account is based upon estimations of the osmotic pressure in the roots and leaves of species growing in different habitats, viz. steppe, meadow and swamp. The average rate of evaporation in the three conditions was approximately in the ratios 0·8 : 2·2 : 3·4. The moisture in the soil was similarly least in the steppe, whilst the swamp exhibited permanent saturation. The roots of various species from the three habitats showed a striking correspondence with the humidity of the environment. Roots of plants from the steppe gave osmotic pressures of from 0·4—0·48 (normal Na Cl soln.), from the meadow 0·19—0·3, and from the swamp 0·13—0·2. Comparable results were obtained where the same species was taken from different habitats and moreover, even in the same habitat and with the same species, the osmotic pressure showed a decrease when the water content of the soil attained a maximum owing to heavy rainfall. Determinations of the osmotic pressure of the leaves likewise yielded a

maximum for the steppe plants and a minimum for the swamp plants. In the same plant the different parts were found to have an osmotic pressure that varied inversely with the moisture of their respective environments. Even in one and the same leaf it was found that provided the blade were broad the osmotic pressure near the margin was sensibly higher than near the midrib.

The conclusion seems warranted that osmotic pressure is dependent upon the humidity of the environment whether conditioned by external influences or internal structure.

Several of the different types of woodland in Britain have now been described more or less fully for considerable areas, and to these must now be added the oakwoods (*Quercus Robur*) with hornbeam undergrowth as represented in Hertfordshire (Salisbury, *Journal of Ecology*, vol. iv. pp. 83–117). These are associated with the lighter clays and loams of the clay-with-flints and the same type occurs on similar soils in Essex, Kent, and Middlesex. It seems to be a quite natural type not met with on the heavier clays where the undergrowth is hazel. In the herbaceous vegetation several societies can be recognised, marked by the abundance of either bracken, wood anenome, dog's mercury, lesser celandine, or creeping buttercup. The soil water is least where the bracken flourishes and increases in the other societies in the order named. Dog's mercury, however, sometimes abounds in dry situations where the acidity is low. The evidence available would appear to indicate that the diffuse light intensity in woodlands is always the limiting factor to assimilation. Two phases can be recognised depending upon the illumination, viz. a *light phase* (from leaf fall in autumn to leaf expansion in spring) and a *shade phase*. During the former the light intensity is from 30 per cent. to 40 per cent. of that outside the wood, whilst during the latter it falls to from 1·3 per cent. to 0·16 per cent. The biological relations of the herbs are intimately bound up with this diminution, a fact strikingly illustrated in their flowering periods and the duration of their foliage. The changes consequent upon coppicing the shrub layer are very pronounced and mainly related to the great increase in illumination. For example the number of species in the interior of the wood before coppicing may be only eleven, whilst two years after removal of the shrub layer the number may be over sixty,

and, further, the individuals show a similar numerical increase. This additional flora comes in from the margins of the wood and the rides, whither it is once more driven as the sprouting stools again diminish the light. Another effect of coppicing, important to the forester, is the increased acidity due to the breakdown of humus and a consequently lowered water capacity.

Taxonomy.—An interesting abnormality has been encountered by Small (*Annals of Botany*, vol. xxx. pp. 191–2) in the groundsel *Senecio vulgaris*, which is probably of phylogenetic significance. In several instances the ovaries were found to contain two ovules and in one case the ovary was bilocular. In all the examples the ovules were attached parietally and there was evidence that the series exhibited all four placentæ of a bicarpellary ovary. Such observations gain from the fact that more than one ovule has been recorded in the ovaries of the dandelion and species of zinnia.

ZOOLOGY. By Chas. H. O'Donoghue, D.Sc., F.Z.S., University College, London.

Protozoa.—Mast and Root have made a series of "Observations on *Amœba* fed on Rotifers, Nematodes, and Ciliates, and their Bearing on the Surface-tension Theory" (*Jour. Exper. Zool.* July 1916). *Amœba* cuts the paramœcium in two, and this was done experimentally and the energy required calculated, from which they deduced the energy employed by the amœba. They conclude that surface tension of the protoplasm is at best an insignificant factor in the process of feeding. In another series of "Observations on Ciliary Currents in Free-swimming *Paramœcia*" (*ibid.* Aug. 1916), Mast and Lashly point out that there is no feeding cone while paramœcium is swimming about but only while it is at rest.

Invertebrata.—A new form of sponge spicule is dealt with by Dendy in a note "On the Occurrence of Gelatinous Spicules and their Mode of Origin in a new Genus of Siliceous Sponges" (*Proc. Roy. Soc.* B, No. 616, 1916). They are termed "colloscleres" and as far as can be ascertained consist of colloidal silica.

Cary writes on "The Influence of the Marginal Sense Organs on the Rate of Regeneration in *Cassiopea Xamachana*" (*Jour. Exper. Zool.* July 1916), reaching the conclusion that regenera-

tion is subject to the influence of the nerve centres, as are many other functional activities. In the same journal (Nov. 1916) Parker and Titus treat of "The Structure of *Metridium* (*Actinobola*) *marginata* Milne Edwards, with special reference to its Neuro-Muscular Mechanism." They distinguish four types of muscle cells with various relations to the nervous system which itself is not limited to ectoderm or entoderm, but penetrates the mesaglea and connects the two layers. Parker (*ibid.*) describes four varieties of "The Effector Systems of Actinians," the mucous, the nematocyst, the ciliary, and the muscular.

The polyzoa are treated by Lang in "A Revision of the 'Cribrimorph' Cretaceous Polyzoa" (*Ann and Mag. Nat. Hist.* July 1916), and by Walters in "Some Species of *Crisia*" (*ibid.* Dec. 1916).

The "Factors affecting Male-production in *Hydatina*" have been investigated by Shull and Ladoff (*Jour. Exper. Zool.* July 1916), who find that various chemicals reduce male-production and oxygen increases it. The results are not due to osmotic pressure, acidity or alkalinity.

The same journal contains accounts of the behaviour of a sex chromosome complex in "The Germ Cells in *Ascaris incurvata*" by Goodrich, and of the control of head production in pieces of worm, viz. "Studies on the Dynamics of Morphogenesis in Experimental Reproduction and Inheritance: IX. The control of Head form and Head frequency in Planaria by means of Potassium cyanide" by Child. In note xxxix. from the Gatty Marine Laboratory M'Intosh treats of "Polychætes" (*Ann. and Mag. Nat. Hist.* Aug. 1916).

Clark writes "On New Starfish and five New Brittle Stars from the Galapagos Islands" (*ibid.* July 1916). The eggs of the Echinodermata from their size, and ease with which they can be manipulated, are favourite things for experimental work and have been utilised in three papers. Dpanchakoff in "Studies on Cell Division and Cell Differentiation: I. Development of the Cell Organs during the first Cleavage of the Sea-urchin Egg" (*Jour. of Morph.* Sept. 1916) claims that the differences exhibited by the most essential cell organs are easily demonstrated. They appear regularly at definite stages of cell mitosis and are to be regarded as the result of physico-chemical cycles. Lillie in "The Physiology of Cell Division: VI.

Rhythmic Changes in the Resistance of the dividing Sea-urchin Egg to Hypotonic Sea-water and their Physiological Significance" (*Jour. Exper. Zool.* Oct. 1916) puts forward two propositions. "Division results from (1) a definitely localised increase of surface-tension, resulting directly from increased permeability and decreased electrical polarisation of the cell-surface, over two symmetrical areas centring at the poles and extending to near the equator; and (2) a secondary or adjuvant effect of the same kind due to the diffusion of electrolytes from the astral centres or centrioles which become chemically active at this time." Lastly we have the "Experimental Control and Modification of Larval Development in the Sea-urchin in Relation to the Axial Gradients" dealt with by Child (*Jour. Morph.* Dec. 1916). Low concentrations of certain substances bring about differential effects along the axes and seem to show that the investigation of the relation between susceptibility and metabolic rate affords a method for the investigation of many tenatological forms observed in nature.

New species of Mollusca form the basis of: "Notes on the Cephalopoda of the Irish Atlantic Slope" by Massey (*Ann. and Mag. Nat. Hist.* July 1916); "Descriptions of Eight New Species of Marine Mollusca from the South Shetland Islands" by Preston (*ibid.* Sept. 1916); "*Pisidium supinum* A. Schmidt and *P. parvulum* Classin, fossil in Ireland," by Woodward (*ibid.* Oct. 1916); and "Descriptions of Two New Mollusca of the Genera *Leptothyra* and *Mitra*" by Sowerby. Churchill, who has studied "The Absorption of Nutriment from Solution by Freshwater Mussels" (*Jour. Exper. Zool.* Oct. 1916), has no doubt that the mussels may make use of some kinds of food which are in solution in the water. A discussion of "Shell-banding as a Means of Protection" is put forward by Trueman (*Ann. and Mag. Nat. Hist.* Oct. 1916).

Crustacean forms are recorded in: "On *Paragnathia*, a Genus of the Crustacean Family Gnathidæ," by Cooper (*Ann. and Mag. Nat. Hist.* July 1916); "*Parapherusa crassipes* (Haswell), an Amphipod of Australian Seas" by Chilton (*ibid.* Aug. 1916); and "A New Species of the Crustacean Genus *Squilla* from West Africa" by Calman (*ibid.* Oct. 1916). Chilton also gives an account of "The Gribble (*Limnoria lignorum* Rathke) attacking a Submarine Cable in New Zealand" (*ibid.* Aug. 1916). The "Chemical Control of Rhetaxis in *Asellus*"

has been tested by Allee (*Jour. Exper. Zool.* Aug. 1916), who finds that potassium salts are most effective in producing greater positive results. Hurst writes " On the Occurrence of the Tropical Fowl Mite (*Liponyssus bursa* Burlese) in Australia and a new Instance of its attacking Man " (*Ann. Mag. Nat. Hist.* Aug. 1916). " The Genera and Species of Mallophaga " by Harrison (*Parasitology*, Oct. 1916) is a very long, useful, and probably complete list of the Mallophaga so far recorded with a revision of the nomenclature, a bibliography, and a scheme of classification.

Distant has two notes on Hemiptera : " Rhynchotal Notes : LX." (*Ann. and Mag. Nat. Hist.* July 1916), and " Rhynchotal Notes : LXI." (*ibid.* Sept. 1916).

The previous work of Payne has been extended, and a discussion of the various constituent parts of the germ cells given in " A Study of the Germ Cells of *Gryllotalpa borealis* and *Gryllotalpa vulgaris* " (*Jour. Morph.* Dec. 1916).

The Hymenoptera are dealt with by Cockerell in " Descriptions and Records of Bees : LXXII." (*Ann. and Mag. Nat. Hist.* July 1916); by Turner in " Notes on Fossorial Hymenoptera : XXII. On some Australian Genera " (*ibid.* Sept. 1916) and " Notes on Fossorial Hymenoptera: XXIV. On the Genus *Nitela* Latr." (*ibid.* Oct. 1916). " *Triœxhra gossi*, a New Genus and Species of Odonata from the Eocene of Bournemouth," is placed on record by Campion (*ibid.* Aug. 1916).

Edwards describes " New and little known *Tipulidæ* chiefly from Formosa " (*ibid.* Sept. 1916) and " Two New Australian Diptera " (*ibid.* Dec. 1916). It is suggested by Williams in " Photogenic Organs and Embryology of Lampyrids " (*Jour. Morph.* Dec. 1916) that the light of the firefly is for bringing together the sexes. All organs are of the same general plan and consist of an upper reflector layer and a lower or photogenic layer of cells. Tracheæ and nerves penetrate both layers but are more highly developed in the lower. About eighty species of diptera were examined by Metz and described in " Chromosome Studies on the Diptera : II. The Paired Association of Chromosomes in the Diptera and its Significance " (*Jour. Exper. Zool.* July 1916), who concludes : 1. The paired arrangement of chromosomes is selective to the highest degree. 2. Each maternal chromosome becomes associated with a definite, similar paternal chromosome and with

no other. 3. Chromosome pairing is dependent upon the qualitative (physico-chemical) similarity of the chromosomes.

The Lepidoptera furnish ground for a series of publications: "New South American *Arctiadæ*" (*Ann. and Mag. Nat. Hist.* July 1916), and "New Delias and other Butterflies from the East" (*ibid.*), both by Joicey and Talbot; "Descriptions of new *Pyralidæ* of the sub-families *Epipaschianæ*, *Chrysanginæ*, *Endotrichinæ*, and *Pyralinæ*" (*ibid.*) and "Descriptions of New *Pyralidæ* of the sub-families *Epipaschianæ*, *Chrysanginæ*, *Endotrichinæ*, and *Pyralinæ*" (*ibid.* Oct. 1916), both by Hampson; "New Indo-Malayan Lepidoptera" (*ibid.* Aug. 1916), and "New Species of Butterflies and Moths from Australia, Africa, and the Indo-Malayan Region" (*ibid.* Dec. 1916), both by Swinhoe. Hawkes has found in testing "The Effect of Moisture upon the Silk of the Hybrid *Philosamia* (*Attacus*) *ricini* (Boisd.) ♂ × *Philosamia cynthia* (Drury) ♀" (*Jour. Exper. Zool.* July 1916), that in all but two cases the white or fawn cocoons become various shades of red-brown when placed in a very moist atmosphere.

Records of Coleoptera occur in "On New Neotropical Curculionidæ" by Marshall, and "On the Lamellicorn Coleoptera of Lanat Island" by Arrow (*Ann. and Mag. Nat. Hist.* Dec. 1916).

Vertebrata.—Fish are treated by Regan in "The British Fishes of the Sub-family Clupeinæ and Related Species in other Seas" and "A new Loricariid Fish of the Genus Cyclopium from Ecuador" (both in *Ann. and Mag. Nat. Hist.* July 1916), and by Boulenger in "Description of a new Fish of the Genus *Barbus* from the Niger" (*ibid.* Dec. 1916). Cockerell gives a description of "The Scales of the Brotulid Fishes" (*ibid.* Oct. 1916).

A "Description of a New Genus of the Family *Lacertidæ* from Central Africa" is provided by Boulenger (*ibid.* July 1916). Goodrich puts forward suggestions "On the Classification of the Reptilia" (*Proc. Roy. Soc.* B, 615, 1916), in which he regards them as not a true monophyletic group but a grade of organisation, and bases the classification mainly on the presence of a hook-shaped metatarsal and the sub-division of the aortic trunk. Sollas and Sollas have extended their observations "On the Structure of the Dicynodont Skull" (*Phil. Trans. Roy. Soc.* B, No. 346, 1916) by the examination of a

further specimen cut in serial sections by means of which they have been able to check their first account and add certain details to it. Baumgartner has worked out "The Development of the Hypophysis in Reptiles" (*Jour. Morph.* Dec. 1916). He finds the epithelial portion arises as a single primordium in turtles, lizards and snakes, and probably alligators. Rathke's pouch appears first, then the two lateral buds, and finally the anterior bud. The three parts of the adult hypophysis are distinct ontogenetically and histologically.

Some light on the vexed question of lens production in vertebrates is shed by Werber in "On the Blastolytic Origin of the Independent Lenses of some Teratophthalmic Embryos and its Significance for the normal Development of the Lens in Vertebrates" (*Jour. Exper. Zool.* Oct. 1916). He thinks that fully differentiated free lenses found in some teratophthalmic embryos are due to a chemical stimulus of blastolysed potential optic cup substance on any part of the ectoderm with which it may chance to come into contact.

In the class Avies we have a paper by Riddle on "Sex Control and Known Corrélations in Pigeons" (*American Nat.* vol. l. July 1916). A considerable body of evidence exists which points to the fact that germs prospectively of one sex have been forced to produce an adult of the opposite sex. Neither selective fertilisation, differential maturation, nor a selective elimination of ova in the ovary can account for the results obtained. The difference seems to rest on modifiable metabolic levels.

On the mammalia we have a long series of publications by Thomas, all in the *Annals and Magazine of Natural History* for 1916. "On the Rats usually included in the Genus *Arvicanthis*" (July), "On Rattus as a Generic Name, with a Note on the Nomenclature of *Echimys* and *Loncheres*" (*ibid.*), "On the Generic Names applicable to the Chevrotains (*Tragulidæ*)" (*ibid.*); "On Small Mammals obtained in Sankuru, South Congo, by Mr. H. Wilson" (Aug.), "On the Generic Names *Rattus* and *Phyllomys*" (*ibid.*), "Three New African Mice of the Genus *Dendromus*" (*ibid.*); "On the Classification of the Cavies" (Sept.), "Some Notes on the *Echimyinæ*" (*ibid.*), "Two New Argentine Rodents, with a new Sub-genus of *Ctenomys* (*ibid.*); "Two New Species of *Akodon* from Argentina" (Oct.), "The Grouping of the South American *Muridæ*

commonly referred to *Akadon*" (*ibid.*); and "Two New *Muridæ* from South America" (Dec.). Pocock also has a series in the same publication "On the Hyoidean Apparatus of the Lion (*F. Leo*) and Related Species of *Felidæ*" (Aug.); "Some Dental and Cranial Variations in the Scotch Wild Cat (*Felis sylvestris*)" (Sept.), "On the Tooth-change, cranial characters and classification of the Snow-Leopard or Ounce (*Felis Uncia*)" (*ibid.*); and "The Structure of the Auditory Bulla in existing species of *Felidæ*" (Oct.). New forms are recorded by Blackler in "On Two New Carnivores from Asia Minor" and "On Two New Sub-species of Roedeer" (both in *Ann. and Mag. Nat. Hist.* July 1916).

The *Quarterly Journal of Microscopical Science*, vol. lxii. Pt. 4, 1916, contains two papers. That of Edgeworth "On the Development and Morphology of the Pharyngeal, Laryngeal, and Hypobranchial Muscles of Mammals." This is a continuation of a paper in vol. lix. The various muscles in the regions named are dealt with, and the author concludes with a very useful list of the points in which Monotremes are more primitive than Marsupials, and these in turn than the Eutheria. Interestingly the pterygo-tympanicus retains more primitive conditions in some Edentates than in Marsupials. That of O'Donoghue "On the Corpora lutea and Interstitial Tissue of the Ovary in the Marsupialia" includes a description of two new varieties of corpus and a general review of that structure in the Marsupials. Interstitial tissue, a tissue *sui generis*, appears to be present in the Diprotodontia and absent in Polyprotodontia. Martin has examined "Tooth Development in *Dasypus novemcunctatus*" (*Jour. of Morph.* Sept. 1916), and gives the dental formula of the lower jaw as M. 1, P.M. 7, C. 1, I. 6 or 5. He confirms Tomes as to the presence of an enamel organ.

Very well preserved casts even showing the position of blood vessels enabled Moodie to write "On the Sinus Paranasalis of Two Early Tertiary Mammals" (*Jour. Morph.* Dec. 1916). He is led to believe that the organ of the sinus paranasalis is to be found not in early mammals but in remote ancestors.

Experiments by Hopkins on "The Growth of the Body and Organs of the Albino Rat as Affected by Feeding various Ductless Glands (Thyroid, Thymus, Hypophysis, and Pineal)"

(*Jour. Exper. Zool.* Oct. 1916), when compared with careful controls and norms, show the following: Thyroid feeding produces a decided hypertrophy of the heart, liver, spleen and kidneys and suprarenal glands; thyroid in sub-toxic doses, thymus, hypophysis, and pineal no appreciable result.

Lastly in Man a "Detailed Study of Superficial Characters of the Brain in Twins of the Same Sex and also other Points" made by Sano (*Phil. Trans. Roy. Soc.* B, No. 349, 1916) supports Galton's observations on the functional similarities. And Kernan has described "The Chondrocranium of a 20 mm. Human Embryo" (*Jour. Morph.* Sept. 1916), which is intermediate between the 18·5 mm. and 23 mm. embryos of Von Noorden.

General.—Four of the general papers deal with more or less cytological problems. Harvey has provided a useful article in "A Review of the Chromosome Numbers in the Metazoa" (*Jour. Morph.* Dec. 1916), in which is given a list of all the various and in some cases contradictory chromosome numbers that have been published since Van Beneden's work in 1883 up to 1915. It includes only the Annelida, Arthopoda, and Cœlenterata. A method for "The Calculation of Linkage Intensities" is given by Emerson (*American Nat.* July 1916). He claims that the formulæ suggested afford a convenient method of approximating gametic ratios from zygotic series when the observed frequencies are in fair accord with a series based on a given formula, or the formula of Bateson and Punnett. Should the observed frequencies be far from this type no method gives a close fit between observed and calculated results. In the same journal Muller discusses "The Mechanism of Crossing Over."

Packard has carried out experiments on "The Effect of Radium Radiations on the Rate of Cell Division" (*Jour. Exper. Zool.* Aug. 1916). Such radiations have an appreciable effect and it is suggested that such results are due to their direct action upon the endoenzymes. These in their turn affect all the chemical processes in the cell and also the rate of cell division.

Lastly Werber furnishes a long account and discussion of "Experimental Studies on the Origin of Monsters" (*ibid.* Nov. 1916). He states that in general the results of such experiments go far towards justifying the hypothesis on which

they were based, namely that parental metabolic toxæmia may be the cause, or at least the chief cause, underlying the origin of monstrosities.

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc.

FEW sciences are habitually treated in a more purely academic spirit than anthropology, but few have in reality such important practical implications. On previous occasions I have drawn attention to the fact that no nation has more need, or so much need, to encourage anthropology as Great Britain has; since the Imperial State which rules some 400,000,000 of people of almost every living race has constantly to deal or attempt to deal with all those extremely complex problems which arise when white men come into contact with other races and cultures. On these problems the scientific anthropologist has as yet scarcely made his voice heard, although, of course, it is he who should be in a position to make the most important contribution towards the settlement of questions connected with the political and social treatment of "natives." Our attention is drawn to this aspect of anthropology by an official report, which was recently issued, dealing with a small but significant negro rising which occurred in Nyasaland early in 1915. This rising was not a sudden blow struck by an aboriginal tribe against the ever-encroaching Europeans, nor was it connected in any way with the Anglo-German war which was in progress contemporaneously along the Nyasaland border. It was a politico-religious movement carried out by negroes who had come into the closest contact with European civilisation, and was under the leadership of an American negro. The rising was small and was speedily suppressed; but several murders were committed, and these "Christian" negroes carried off the head of one of the murdered white men, and deposited this gruesome relic on the altar of one of their churches, where a thanksgiving service for the supposed success of the rising was held. A Commission was appointed to investigate the causes of the rising, and the commissioners' report was published last year. (See the *Nyasaland Times* for February 10, 1916.) The commissioners found that the chief cause of the rising was the inflammatory character of the religious propaganda of certain Christian (or

so-called Christian) sects, notably the Ethiopian Church and the Seventh Day Adventists, though a subsidiary cause was to be found in the unduly hard economic conditions prevailing on certain estates. The commissioners made a series of recommendations, of which the most important was that missionising sects of this emotional and inflammatory character should be excluded from Nyasaland.

This instructive episode illustrates well the unexpected and often dangerous responses which the Ethiopian mind is apt to make when coming in contact with higher cultures. It is notorious that there has always existed a sharp division of opinion between practical colonists on the one hand and Christian missionaries and philanthropic theorists on the other hand in regard to the question of the status and treatment of coloured races. The colonists have laid much stress on white superiority, whilst the philanthropists have often advocated an extreme extension of democratic doctrines of equality, which were originally evolved under European conditions. In a long view, the problem is almost the greatest awaiting solution by human statecraft. One may be fairly confident that the trained anthropologist will usually be much less ready than the ordinary politician to propound a theory. Very little is really known of race-psychology. There are, however, a few obvious comments to be made on the question, which should be, but are not, platitudes. Firstly, it is clear that there will need to be in the British Empire as many fundamentally distinct native policies as there are markedly distinct coloured races. The gap between the European and the Indian is undoubtedly less, I opine much less, than the gulf separating the Indian from the negro. Secondly, very little should be taken for granted, and the aptitudes of natives should be investigated and tested in as many directions as possible. The mind of another race when seriously approached is certain to yield constant surprises. Thirdly, when instilling a new culture into a lower race the motto should evidently be to proceed slowly.

Further contributions to the question of early and extensive migrations have been made by Prof. G. Elliot Smith and Mr. J. Wilfrid Jackson. The former scholar has published a paper in the *Journal of the Manchester Egyptian and Oriental Society*, 1915–16, which is entitled " Ships as Evidence of the

Migrations of Early Culture." The paper is of considerable length, and deals in detail with the distribution of different types of ships. As an instance of the manner in which primitive peoples may migrate vast distances by water, Prof. Elliot Smith cites the case of the populating of the Polynesian Islands. A new paper by J. W. Jackson is entitled "The Use of Cowry-shells for the purpose of Currency, Amulets, and Charms" and is to be found in the *Memoirs and Proceedings of the Manchester Literary and Philosophical Society*, vol. lx. pt. 3. In regard to the use of the money-cowry in America, a continent to which the mollusc is an alien, the author dissents strongly from the view that it was not introduced until the time of Columbus.

As the reader will be aware, there exists in this country as well as abroad a school of anthropologists who hold what may be termed, without offence, extreme views on the antiquity of man. They believe that *Homo sapiens* lived in Europe long before the Aurignacian Age, they would trace the human family far back into the Miocene, and they find eoliths which they credit in Pliocene and even in Miocene strata. Of the representatives of this school, none is more active and enthusiastic than Mr. J. Reid Moir of Ipswich, and attention may be called to two of his recent papers. One is entitled "The Evolution of the earliest Palæoliths from the Rostro-Carinate Implements" and is to be found in the *Journal of the Royal Anthropological Institute*, vol. xlvi. and the other is published under the title "A Series of pre-Palæolithic Implements from Darmsden, Suffolk," and has now been reprinted from the *Proceedings of the Prehistoric Society of East Anglia*, vol. ii.

NOTES

Sir Alfred Keogh, G.C.B.

All men of science will be greatly pleased that the high honour of the Grand Cross of the Bath has been conferred upon Sir Alfred Keogh, the Director-General of the Army Medical Service. For many years past he has been engaged in raising the Royal Army Medical Corps to the height of efficiency which it has now reached, and the services of the Corps in connection with the war are known to all.

On February 3 the *Times* published a very generous letter from Lord Esher, admitting the error made (before the war) by himself and his Committee for the reconstitution of the War Office in not listening to Sir Alfred's objection to the D.G.M.S. being placed under the Adjutant-General. Lord Esher now recommends Lord Derby to strengthen the Army Council by placing the D.G.M.S. upon it. We may remark that the defect exists everywhere in British administration, the heads of Medical Services being generally kept in subordinate positions and seldom being placed upon Executive Councils. For years past medical men and the medical press have been trying in vain to obtain a reform in this matter.

White Fakirs

The Indian fakir seated by the roadside, lost in visions of *nirvana* or some equally impossible superstition, living upon the admiring charity of ignorant peasants, too lofty to work or to talk or even to kill the flies that sting him, and clothed in rags; has his counterpart in Europe, and especially here, in the transcendental persons who would teach us that what is useful is useless and what is obvious is untrue. But the white fakirs, though they still keep the admiration of the unwise, do by no means hold the beggar's bowl but often sit in the seats of the academically mighty and flood the world with golden words—which float away from their lips into the air as beautiful but as empty as soap-bubbles. Whatever is

suggested for the betterment of things—science and modern languages in education, payment of research, experimental pathology, town-planning, sanitation—they lift their voices and cry, "Think not of this barren world; fix your minds upon the absolute. What does anything matter here? but there, in *nirvana*, eternal bliss awaits you." And loud applause greets the lofty pose. Thus when an extensive sanitary campaign was once proposed to the governor of a certain colony, he, who was suffering from fever at the time, replied sadly but bravely, "After all, what does it matter? We are here to suffer because of our duty; let us ignore our suffering, and consider only our duty." And his poor people continued to die of the disease in hundreds. Later, in another capacity, he did perhaps more harm to his country during the war than most men who are not politicians.

The opposition to more science and modern languages in education is principally due to this spirit of fakirism. Children will be debased by being taught useful things: fix not their eyes upon what is of earth earthy; give them character rather than knowledge, direction rather than force, good intentions rather than capacity; so shall they win to Heaven. We see much of this caste of thought everywhere in this country, and may mention as an example a brief report in the *British Medical Journal* of a paper read at the Royal Society of Arts on December 20 last. After pointing out, fairly correctly, that the true division of education was, not into ancient and modern, but into literary and scientific (though even this is probably not quite accurate), and admitting that the ideal was a combination of literary and scientific education, the speaker, added that "There was too great a disposition to think that knowledge in itself was valuable. It was valuable precisely in so far as it inspired and animated and trained the mind. The mere knowledge of facts was unimportant. . . . A thing which was practically useful had not necessarily any educational value." And the Chairman of the meeting summed up by saying that "The great aim of education was to stimulate curiosity and to create the habit of observation. It was not knowledge that was important but the love of knowledge."

From this we gather that the effort is everything but the result nothing. So also we may say, it is not food which is important but the love of food; and our great aim should

be to stimulate the flow of saliva by the exhibition of dishes which, like those put before Sancho Panza, are not to be eaten. Thus, ultimately, we shall reach the *nirvana* of absolute ignorance (and starvation) where we shall remain for ever seated in the beatific contemplation of our own virtue (and hunger).

A false philosophy, this modern English one of fakirism—as false as the opposite German one of thuggism. Neither for fakirs nor for thugs has the world any use. Both are only morbidities—the fakir the worse because of his decadence.

The world requires people who do things—not those who can do things if they wish but do not do them. Such are only wastrels; and the complaint against our education is that it breeds too many wastrels. For centuries instruction has been given in the schools because the instructed person is useful to his fellows, while the uninstructed person is fit only for unskilled manual work; and the experience of centuries is apt to teach something of truth. It is idle to talk of the love of knowledge being something apart from knowledge—one might as well admire the love of swimming in one who cannot swim. *Per contra,* the world judges rightly that one who cannot swim is too lazy to learn and that one who knows nothing is a loafer. To tell the young that they should seek knowledge but that knowledge when found is of little account, is a paradox which they will detect at once but employ as an excuse for idleness; yet from every side comes the complaint that this is the doctrine actually taught in many of our schools. Science is a bore, literature ridiculous, languages too much trouble; the only things that matter are games and the cut of one's collars. This kind of spirit will serve no longer. It breeds only intuitive philosophers, conscientious objectors, anti-vivisectionists, party politicians, and other charlatans; and a nation which accepts it too frequently will itself become a fakir sitting begging by the roadside of progress.

Fakirism is the degeneration of minds which possess intellectual tendencies but not the vigour required to digest the wholesome food of facts which alone can raise intellectual tendencies to the difficult summit of achievement. It is a paralysis which often overtakes fine but delicate souls, as heart-failure often overtakes athletes. We shall always find it easier to give up the ascent, to sit down, sigh, and complain

that the peak is not worth the pains. But success is the only sound proof of capacity; and the caste-mark on the brow of the fakir is—failure.

Learned Procrastination

I yield to no one in respect for the Royal Society; but my admiration would be increased if I were convinced that it was taking its full place in the life of the Empire as the great central organisation for the encouragement of natural science by every possible means. I fear, however, that before it reaches this position it must reform itself in many directions. For example, as with many learned societies, its procedure regarding the publication of papers seems to many of us to be out of date; and young men, especially, have several times commented bitterly to me of the delay and other difficulties caused by the present rules of the Society. The following case is one regarding the facts of which I can personally vouch. Early last July a Fellow of the Society submitted an article on a scientific subject regarding which he is an amateur, and on submitting it asked specially that it might be returned at an early date if it was not likely to be acceptable. As a matter of fact it was kept for several months before the author heard anything further on this point. He was then informed that there were considerable difficulties regarding its publication, as one out of the three referees to whom the paper had been sent was opposed to its publication; and the final result was that the author had to withdraw it and received it back in December last, more than five months after it had been submitted. Even then, according to the rules, he was asked to return the manuscript to the Society. What information was given to him was supposed to be confidential, the names of the referees and the objections made by one of them being kept secret, so that he could not lodge a formal complaint; while the Society apparently escaped from the onus of formal rejection by taking refuge in the intricacies of its procedure. Really, is this sort of thing proper for the present day? The author admitted himself to be merely an amateur and had no grounds for complaining if the article was not good enough for the Society; but he has grounds for complaining at the long delay, which appears to be totally unnecessary and due merely to procrastination, since there seems to be little excuse for keeping for five months a paper which can be read in an hour or two; and it is doubtful whether he can now ever publish it anywhere, although two of the referees approved of it.

The general principles upon which such a meticulous attitude is adopted by the Society towards papers are at least open to discussion. It really does not matter much whether a paper is a little below par, because its demerits will easily be detected by expert readers and the cost of printing nowadays is extremely small. On the other hand such delay as occurred in this case is very discouraging to contributors, whose future work is often blocked for months in consequence, while they may lose priority. And in fact, in this case another paper covering nearly the same ground had just appeared without the knowledge of the author, or apparently of the referees, a little before he submitted his article, so that he has now lost what credit he might have obtained if his article had been promptly brought out. Then again the extraordinary secrecy with which these matters are conducted appears to be quite behind the times. We do not wish our articles to be subjected to the secret condemnation of a *Vehmgericht*, while I have heard young men moot the suspicion that the secret referees may refuse papers and

yet use the ideas in their own work—which I hope seldom happens, but which nevertheless may just possibly occur. Why not adopt a perfectly open attitude throughout—compel the referees to give their opinions within a week, and acquaint the author with their names and their reasons for rejecting or accepting?

The final result of its rules is that the Society does not receive as many papers as it should do. Thus the whole of its Proceedings during the last fifteen years occupies only two shelves of my book-cases—surely a somewhat small output for such an important institution (to which we subscribe £3 a year). It is openly said that very original articles are seldom submitted to the Royal Society, just for fear of such delay and rejection; and I am certain that the best scientific article which I for one ever wrote would never have had a chance of publication by the Royal Society. I have such respect for the Society that I would like to see a root and branch change made in this matter, and believe that many other Fellows agree with me.

The same remarks will apply to some other learned societies—which appear to attach more importance to their own dignity than to progress in the subjects with which they deal. And very similar remarks were actually made before the Royal Society itself in 1904 by Mr. J. W. Buchanan, F.R.S., a *résumé* of which will be found in his fine new book, *Comptes Rendus* (University Press, Cambridge, 1917). He compares the procedure of the Royal Society with that of the French Academy of Sciences, and points out that in the latter Fellows have a right to fifty pages annually in the publications of the Academy, and that "the communication, reading, and publication of a paper presented to the Academy is . . . an affair of the inside of a week, and it is a certainty." These remarks were made before the Royal Society thirteen years ago, and yet nothing has been done. We British do not appear quite to appreciate how fast we are losing credit throughout the world by our apparent incapacity to correct abuses, however clear they may be and whatever trouble they may cause.

R. R.

The Association of Public School Science Masters (C. L. Bryant, The School, Harrow)

The seventeenth annual general meeting of this Association took place at Eton on January 3 and 4. The discussions were well attended and sustained in view of the interest recently taken in the position of science in education. There was an exhibition of apparatus by members of the Association in the spacious laboratories of the College. In this, exhibits of materials, apparatus, and books used in the teaching of military subjects in schools formed a prominent part; and Mr. J. Christie (Eton) gave demonstrations with a magnificent collection of physical apparatus.

Prof. H. H. Turner presided throughout the discussions as well as during the more social part of the meeting. In his opening address he spoke of the difficulties in the way of modern-day research. First of all there was the difficulty of finding careers for investigators. It was not that there was insufficient work to be done—work was less likely to fail than workers—for with every advance in knowledge new fields for research were discovered. As an example of work which urgently needed doing he instanced the survey of our Empire, geodetically, magnetically, gravitationally, bathymetrically, and in other ways; and there were forestry and fisheries and various kinds of industrial research. But the difficulty lay in the uncertainty in the minds of young men qualified to take up the work of investigation as to their prospects in life. To remedy this he suggested the

formation of a Research Civil Service, parallel to the existing Administrative Civil Service. His second point was the embarrassing length of modern-day researches. But he said that it was not impossible for schools to assist in these, and he suggested a number of researches which schools (rather than individuals at schools) might undertake. Upper-air research was one of these, and he quoted Captain Cave's opinion that such work was suitable for boys and might be of real value to science. Referring to the popular movement in favour of a more scientific education, Prof. Turner said that he feared lest too much was expected from compulsory education in science. He thought that many boys were hopelessly unscientific, just as many were hopelessly unmusical.

Mr. C. E. Ashford (Headmaster of the Royal Naval College, Dartmouth), in proposing a vote of thanks to the President, called attention to the change in note in the address from that to be found in the writings of Huxley and his school, who insisted on the suitability of science for all, while Prof. Turner found the position of science so secure now that he could safely emphasise the fewness of those suited to research. It was not, however, the only aim of science in general education to produce investigators; every boy should be taught to know something of the value and scope of the subject, even if he was unscientific himself. After the vote of thanks had been passed, Sir Ronald Ross was elected President of the Association for the coming year.

Mr. C. L. Bryant (Harrow) presented to the members the report of the Committee on the year's work. This showed clearly that they had not neglected the chief duty of the Association—to promote the teaching of natural science in the public schools. Towards the end of the year 1915 it was decided to attempt to arouse public opinion on the poor position which science takes in education in this country by means of a campaign in the Press. A letter was written on the subject and received the signatures of thirty-six men well known in the world of science and in public affairs. This letter gave great stimulus to the growing discontent; and, as a result, Mr. M. D. Hill, then Chairman of the Association, was able to form what is now known as the Neglect of Science Committee. Since its birth the latter has worked independently of the parent Committee, though they are in accord as regards their general aims.

In a great many of the public schools at the present time science is not a compulsory subject. In the State-aided schools, of course, science is taken by all boys. But in both classes of schools the teaching of natural science seems to have been designed to suit the needs of future specialists. To most boys science means little more than the elements of hydrostatics, heat, and chemistry; they have no vision of the wider realms of the subject. The Committee have, therefore, been insisting that training in science should form part of the education of every boy, and at the same time they have indicated the general lines which such training should follow in the public schools. For the latter purpose they have drawn up a syllabus entitled "Science for All," in which water-tight compartments are broken down and the human aspects of the subject are prominent. In this, special attention has been given to the service of science to man and to the inculcating of scientific method rather than to the acquiring of detailed knowledge in a restricted field. The syllabus suggests a general method of treatment and is not intended as a prescription; the preamble, indeed, expressly states that the teaching in the different schools ought to follow lines varying with the localities and other circumstances in which they are placed. In this connection, at a subsequent stage of the meetings, the following resolutions were passed *nem. con.*:

"1. For purposes of science teaching in general education—
(*a*) Elasticity is essential.
(*b*) Freedom to draw on all subjects is advantageous and desirable."

"2. The report of the Committee on 'Science for All' is hereby generally approved."

To indicate the aims of science in general education, the Committee have published the following memorandum:

"Natural science in education should not displace the 'humanistic' studies, but should be complementary to them. In this capacity natural science meets two needs in particular:

"(1) *Search for Truth.*—Imaginative power indicates new fields in which further knowledge of truth may be revealed: its subsequent establishment depends on accurate observation, with constant recourse to nature for confirmation. The one aim of natural science is, in fact, the search for truth based on experimental evidence rather than on authority. Hence the study of the subject implies accurate observation and description, and fosters a love of truth. The special value of natural science in the training of mind and character lies in the fact that the history of the subject is a plain record of the search for truth for its own sake.

"(2) *Utility.*—There are certain facts and ideas in the world of natural science with which it is essential that every educated man should be familiar. A knowledge of these facts assists men (*a*) to understand how the forces of nature may be employed for the benefit of mankind, (*b*) to appreciate the sequence of cause and effect in governing their own lives, and (*c*) to see things as they really are and not to distort them into what they may wish them to be. It is the business of natural science in education to bring this knowledge within the range of all."

A deputation from the Association has recently been received by the Government Committee on Science in Education. The deputation gave evidence of many disabilities under which the teaching of science is labouring in the public schools. Prominent among these is the fact that the most prominent boys are diverted from scientific studies quite early in their careers owing to science finding no place in the examinations for scholarships from the preparatory schools to the public schools: the boys afterwards continue to devote themselves to the subjects which they have found to pay in the past. In support of this argument, instances were quoted of schools where the classical sixth forms were composed almost entirely of boys who were entrance scholars, while few, if any, were to be found on the modern side. And in certain instances the extra fees levied upon boys who took science were grossly unfair in view of the small outlay on scientific apparatus.

It was not urged by the deputation that *formal* science should be taught in the preparatory schools: nature study and practical measurements are the proper introduction to the later study of the subject: examinations in these should be compulsory on all candidates for scholarships on entrance to public schools.

The deputation did not favour the setting up of another barrier to a boy's entrance to the university by insisting on his passing an examination in science (as in the proposals now before the University of Oxford). They had seen too much of the evils of compulsory Greek and Latin to wish to add another of a similar nature. They pleaded for removal of compulsion in other subjects rather than for the adoption of compulsion in their own. What *was* insisted on was that every boy should have the opportunity of studying science; and if he had made an honest effort to learn he should not be debarred from going to a university simply because of failure to attain a certain standard in any one subject alone.

Examinations have a cramping effect on education as a whole, and the teaching of science has suffered much from this cause. Examination syllabuses tend to become restricted to what it is easy to examine in, and often have little regard for what it is good for a boy to learn. Science in general education has two main aims: (*a*) informative, and (*b*) the inculcating of scientific method: that is to say, giving to the minds of boys that bent which will lead them to put problems (not only the problems in the realm of science, but also those of life in general) to the test of experiment, and not to rely merely on precedent and authority. It is difficult to see how any simple examination can test the success of this second important aim in the case of a boy of sixteen or so. And if there is an examination the science teaching is sure to tend to become restricted to the syllabus of it. The deputation suggested that the difficulty might be overcome by an extension of the practice of inspection of schools combined with the granting of certificates of industry and ability by properly qualified masters.

In view of the prominence recently given to the position science should take in general education, the chief interest of the meeting centred in a discussion on "Science for the Rank and File." Prof. R. A. Gregory, in opening the debate, pleaded for a broader basis in the teaching of science at school. In the course of his speech he said:

"In the teaching of science it is necessary to distinguish clearly between courses of work suitable for the rank and file, and those intended as preliminary training for scientific or industrial careers. In the one case we are concerned with science as an essential element of a liberal education, and in the other with vocational instruction."

"It is a mistake to regard science and what are now called the humanities as opposing elements in education. Modern life requires that the elements of scientific method and knowledge should be included in every educational course. School work should not, however, be concerned with the training of experts in science, any more than of specialists in classics, but with the imparting of the rudiments of a liberal education to all pupils, so as to awaken interest which will continue when school-days are over. There are reasons for believing that the descriptive and qualitative school science of a generation or two ago was better adapted to promote such continued attention than is the quantitative work in the narrow fields mapped out for instruction to-day. Little justification can be found for the concentrated attention given to a few subjects, with a view to imparting knowledge of experimental methods, when such a course means that the wonders of the fields beyond are kept outside the range of vision."

"One result of concentrating attention upon experimental method in recent years is that the questions set in many public examinations aim at testing practical knowledge only and afford no credit for acquaintance with the history of science or interest in nature as a whole. It is most desirable, from the point of view of the education of the rank and file, to introduce descriptive lessons and reading intended to stimulate interest in scientific work and achievement and their relation to modern life, instead of limiting the teaching to dehumanised material of physics and chemistry which often leaves little impression upon the minds of boys, and the chief purpose of which is to prepare for examinations. There must be more of the human touch in the teaching."

Mr. F. S. Young (Headmaster of Bishop's Stortford College) said that for reasons of organisation it was desirable to have in any one school the same course of training for all boys under the age of sixteen, and he emphasised Prof. Gregory's point that the rank and file should not be taught as if they were to become specialists in science. They should rather be trained to appreciate the value of science and to be capable of forming a judgment on problems or opinions presented

to them. For this purpose some acquaintance with experimental method was essential, but also as wide and comprehensive a survey of science as possible. Coming to the question of the provision of time for science in the school curriculum, he said that that given to languages was at present a very large proportion of the working hours. Every boy up to sixteen learnt three languages—either two ancient and one modern, or one ancient and two modern. Probably, up to sixteen, two languages would have to suffice: for in a general education two modern or two ancient languages could not be defended, the kind of training both gave being the same. That would mean that classical specialists would begin Greek, modern language specialists German, and science specialists their specialised science at sixteen.

Rev. S. A. McDowall (Winchester) said that in teaching boys of sixteen or less it was useless to deal with big generalisations. Up to that age there is great power of assimilating facts, but little power of correlation. The earlier stages of teaching must aim at giving a groundwork of fact and simple deductions therefrom: it must be largely dogmatic. Only in the later stages is it possible to introduce big generalisations and scientific doubt.

The question of the relative positions of Latin and science in the school curriculum was dealt with by several speakers. Rev. A. L. Cortie (Stonyhurst) maintained that Latin was the best introduction to the study of science. Rev. F. G. Forder (Charterhouse) agreed that for formal training Latin was unsurpassed, but science did not compete with Latin on that ground. Prof. H. E. Armstrong said that the distinctive asset of science was the experimental method, which gave science the superiority over all other subjects. It was their duty to make known the inestimable value of real scientific teaching. Their first effort must be to develop an attitude of mind in their pupils—to make clear that science was the endeavour to get at truth—the worship of truth, in fact. Mr. A. Vassall (Harrow) said that the danger was that generalised science might be spoilt by using it for formal training, a purpose for which it was not adapted. This danger would be enhanced if the courses mapped out became stereotyped by means of examination syllabuses before experiments had been made in the methods of teaching on those lines, for the examinations would almost certainly be designed to test formal training and not a scientific habit of mind.

Mr. J. Talbot (Headmaster of the Royal Grammar School, Newcastle) said:

"The mechanism of civilisation is to-day based entirely on science: a nation whose rulers are totally ignorant of science, and careless of this fact, is bound to fail. It is essential that the schools should see that no boy goes out into the world without an understanding of the aims and methods of science. One of the most important of these is the method of experimental investigation. A training in this confronts a boy with bedrock facts which he can neither burke nor belittle when they do not fit with preconceived ideas. The habit of facing such facts is one which is too often missing in the man who has not had this training."

Col. A. Smithells (Leeds University), congratulating the Association on its work, said that English education had been, and was likely to be for some time, dominated by Oxford and Cambridge. If the members of those universities who had gone out to teach science in the public schools could present, with something like unanimity, a policy for the proper treatment of science in the educational curriculum, they would, he thought, exert an influence beyond any other that could be brought to bear at this critical juncture. He had read with great satisfaction their scheme of "Science for All." It seemed to suggest that linking to realities and things of common human interest which had been greatly lacking in science

teaching in the past. In teaching science it was never to be forgotten that however perfect might be the inculcating of scientific method, however sound the mental discipline, however powerful the intellectual implement they supplied, unless they showed how science bore upon the realities, the environment, and the avocations of human life—unless, in fact, they humanised it—science could never flow effectually into the general culture of the nation.

Prof. Gregory, in his reply, said that what was needed was appreciation of the fact that the pursuit of science means the acquisition of new knowledge and not the contemplation of past performances. Accepted principles or theories of to-day may have to be discarded to-morrow because new facts show them to be untenable. This absence of finality is the soul of science. Boys, of course, cannot be brought right into the living stream of scientific work, but the attitude of mind which will understand its power can be promoted, and the opportunity has come for science masters to produce courses of study to attain this end.

Mr. E. R. Thomas (Rugby), in opening a discussion on "School Science in its Relation to Modern Industrial Problems," said: "When all is said and done, school education must remain a compromise: a compromise whose failure has been only partial because boys, though individually distinct and different, do, nevertheless, fall into certain well-defined groups. The schoolmaster's duties are two—to provide opportunities for the development of each of the types of boys under his care, and to stimulate them to take advantage of these opportunities. The wise teacher knows that he can do no more. At present, the bias of school teaching is too much in favour of the boy whose self-expression is predominantly literary, at the expense of the boy whose mental development must take place—especially before the age of fourteen—mainly through the activities of the body. This latter is the boy who will ultimately become the so-called 'Captain of Industry,' and he is at the moment, from a national point of view, rather more important than any of his fellow-pupils. The actual opportunities of self-expression to be offered to his pupils is largely a matter of the teacher's own knowledge, training, and temperament; but there are two ways in particular in which corporate action on the part of the Association will help to remedy the glaring defects of the present system:

"1. A course of manual training in the preparatory school must be insisted upon as a necessary qualification for candidates taking the Common Entrance Examination. The educational value of such training—properly conducted—has been proved by experiment in many different types of schools: in schools, for example, as unlike one another as are the Naval College, Dartmouth, and elementary schools of the Liverpool City Council.

"2. The Association should get into touch with the newly formed Association of British Chemical Manufacturers, with a view to organising and increasing the facilities for studying industrial chemistry on the works themselves. Holiday courses for masters and boys might be arranged."

Mr. G. N. Pingriff (University College School) opened a discussion on the place of text-books in the teaching of science. In the course of his speech he said the chief aim of natural science is "the search for truth based on experimental evidence rather than on authority." A very important aim—not the only one—of science teaching is to give training in and a knowledge of the above process; in fact, of the experimental method. It is not enough to tell a boy about scientific method in, say, important work of the past; he must also experience it himself to some extent in his own work. This is not possible if he works with an ordinary text-book, for the book gives him an alternative and easier method of acquiring

the desired knowledge; he will appeal to the authority of the text-book rather than to the method of experiment. There is much work in science which must be taught didactically, but that does not render less imperative the need to treat heuristically those parts of the subject which are eminently suitable for such treatment.

The Indian Science Congress (Charles Stanley Gibson, M.A., B.Sc., M.Sc., Professor of Chemistry in H.H. The Maharaja's College, Trivandrum, Travancore, S. India; on War Service, Cambridge.)

Compared with that of similar workers in Western countries, the position of the scientific investigator in India has many drawbacks. Apart from the fact that, at present, very few institutions are adequately equipped for modern research both as regards laboratories and libraries, the difficulties of obtaining requisite materials quickly, of having to work under atmospheric conditions which are not only trying to him personally, but which often cause unforeseen troubles with the material on which he is working and with the instruments he is using, can only be realised by experience. But probably the most serious disadvantage under which he labours is that of having little or no opportunity of discussion with others of similar interests; and this "scientific" loneliness may prove a serious obstacle to the progress of an investigation.

In these circumstances, the formation of an Indian Association for the Advancement of Science with aims similar to those of the British Association seemed most desirable. Perhaps those immediately interested in the scheme were reluctant to adopt the full title, or perhaps they wished the new institution to prove itself worthy before claiming relationship with the British Association, or perhaps the word "Congress" is specially Indian. Whatever the reasons for adopting the title, Dr. J. L. Simonsen, Professor of Chemistry in the Presidency College, Madras, and Mr. P. S. MacMahon, M.Sc., B.Sc., Professor of Chemistry in Canning College, Lucknow, did a most useful service to scientific research in India in bringing about the formation of "The Indian Science Congress" some four or five years ago. The Congress has been successful in the highest degree, and it is justified in claiming recognition as a most important factor in the development of science in India.

The formation and running of such an institution in India is attended with exceptional difficulties. An annual meeting is necessary, and immediately the geographical difficulty is realised. Although the Science Congress at once received support from all parts of India and Ceylon, and the annual subscription of five rupees was small enough to exclude but very few, yet it was realised that the huge distances to be travelled by members would probably prohibit large attendances at the meetings. Fortunately, the recognition of the Congress (with the consequent payment of the travelling expenses, especially of those reading papers in some section of the Congress) by the Government of British India and to some extent by the Governments of Native States, togteher with facilities offered by some of the railway companies, have very largely reduced the cost of attendance on the part of members. The majority of the members are on the staff of educational institutions or in other branches of Government service, and it is often difficult or highly inconvenient for an officer to be away from his "station" for any long period at a particular time. For a meeting lasting four days it may easily be necessary for a member attending the Congress to be away for three weeks, even if the effects of such long and continuous railway journeys

on the otherwise enthusiastic delegate be passed over in silence. In educational work the cold season, and therefore the most suitable time in India for all Congresses, is perhaps the busiest time of the academic year.

The Indian Science Congress held its first meeting at Calcutta in January 1913, and since then its meetings have been held at Madras, Lucknow, and recently (January 1917) at Bangalore. Each meeting has been very successful, and the attendance has been larger than one might have expected. A Science Congress is on the whole very much like that of a meeting of the British Association. There is the large meeting with the Presidential address, there is the formal reception, there are the sectional meetings, public lectures, joint discussions, and there are the excursions which, in India, do not have to be curtailed on account of the war.

The meeting at Bangalore was held under the patronage of His Highness the Maharaja of Mysore, G.C.S.I., G.C.I.E.; and Sir A. G. Bourne, K.C.I.E., F.R.S., Director of the Indian Institute of Science, Bangalore, and formerly Director of Public Instruction in Madras, was the President. As usual each of the six sections into which the Congress is divided held its own meetings under its own President. In all sections a considerable amount of work was got through, and it may surprise many to know that eleven papers were read in the Agriculture section, fifteen papers in that devoted to Botany, nineteen in Chemistry, seven in Geology, nine in Physics and Mathematics, and eleven in Zoology and Ethnography. Unfortunately, lack of space prevents any adequate description being given of the communications, but it is interesting to note that pure and applied science were both well represented. In connection with the latter, attention may be directed to the communications in the Chemistry section by members of the staff of the Indian Institute of Science at Bangalore, which show that scientific work of great economic importance to Mysore is in active progress. Of the three public lectures one was on the topical subject of "Explosives." The full report of the meeting will be published later by the Asiatic Society of Bengal, and this will afford interesting reading to those interested in the development of scientific research in India.

That those responsible for the success and progress of the Science Congress do not regard it as having yet attained its full development, is shown by the fact that one of the discussions was on "The Future of the Indian Science Congress." From a scientific point of view India teems with immense possibilities. The difficulties in the way of rapid progress in science are perhaps greater there than in most places. These difficulties can be surmounted, and the Indian Science Congress, even in a short time, has shown that amongst its members are Englishmen and Indians capable of working for a healthy development of India and Indian resources.

The Committee on the Neglect of Science.

This Committee has not been idle since its Conference of May 3rd, 1916.[1] It has distributed 13,000 copies of the Report of the Proceedings of the Conference, which include those forwarded to Members of both Houses of Parliament, Fellows of the Royal Society, Editors of the leading newspapers, the Chambers of Commerce, the Prime Minister and Minister for Education in every self-governing Colony, and members of many other distinguished bodies.

Representatives of the Committee have come into touch with Lord Crewe and Mr. Stanley Heath, C.B., Civil Service Commissioner, in order to discuss with

[1] SCIENCE PROGRESS, July 1916, p. 146.

them the position which Natural Science ought to take in the educational system of Great Britain and the Civil Service Examinations respectively, and Lord Crewe has appointed a Committee to inquire into the matter. The following statements issued by the Committee show how moderate are the demands of the scientists, and completely refute the unfounded charge made by some advocates of the classics that Science wishes to banish Greek and Latin entirely from the curriculum :

1. That their essential object is to secure for the teaching of natural science a position of no less importance, as an essential component of a liberal education, than that which is now held by the teaching of languages and history.

2. That, although they make no proposal to displace these subjects from the curriculum of Public Schools (the Schools represented at the Headmasters' Conference), they recognise that for the attainment of their object it is necessary that time now spent on the study of Latin and Greek grammar and composition and of Roman and Greek history must be greatly diminished.

3. That it is necessary that the position of a boy on entering the school, and in later years, should no longer be determined mainly, as it is now in many schools, by his proficiency in "Classics."

4. That the classical teachers should not be more numerous nor more highly remunerated than those engaged in teaching the natural sciences; in fact, a good form master should be able to teach boys both natural sciences and literary subjects.

Neglect of Science

Prof. Ernest Glynn of the University of Liverpool makes some interesting notes on the neglect of science in his pamphlet *Microbes and the War, with Comments on the National Neglect of Natural Science* (C. Tinling & Co., Liverpool, price 3*d.*). Dr. Glynn begins by regretting that so many of our industries have gone to Germany in past years, and cites the notorious case of apparatus in use in bacteriological laboratories. "Indeed," he says, "I am inclined to think that the only articles of British manufacture of any importance found in the laboratory were soap—Sunlight, and the *Daily Mail*!" That the British people fail to realise that science is a vital factor in life is shown from the following illustrations. "During the present war the Research Department of the Royal Woolwich Arsenal advertised for university-trained research chemists at *wages of £2 0s. 6d. per week*!" "In the whole history of the British Government there has been only one Cabinet Minister who was a trained professional man of science—the late Lord Playfair." "The esteem in which science is held may be measured by the suggestion in Lord Dunraven's scheme for the reform of the House of Lords, that in the future it should consist of 400 members, whereof *two* should represent art, literature, and science." In his section on "Vested Interest in Classics," he brings forward many facts and figures to show that the number of posts in the Government, Fellowships in the universities, and positions in the schools given to men versed in the classics, far exceeds those given to men with a knowledge of science. Discussing the Foreign Office he remarks: "How much of the Turkish muddle and the Balkan muddle is due to our idiotic and unscientific educational methods? Neither the late British Ambassador at Constantinople nor any of the staff spoke the language of the country except one man—he was recalled!" Speaking of Darwin and Lister he says: "The lives of such leaders of modern science are as ennobling to study as those of the greatest men of ancient Greece and Rome." Worshippers at the shrines of dead languages will doubtless receive a salutary shock when they learn from Dr. Glynn that the original *raison d'être* for the study of Latin was chiefly that the Latin "language was

the *living tongue of scholars,*" and "people wanted to know Latin, not to write Latin verses in imitation of Vergil, but *to speak it or to read the latest work on theology or tactics or geography.*"

Vocational Education

At the Conference of Educational Associations on January 3, 1917, Mr. J. C. Maxwell Garnett, M.A., Principal of the Municipal School of Technology, Manchester, lectured on "The Vocational Outlook in Education." He opened his lecture with the remark that "Our education prepares most children for vacations and not vocations," and this provided his chief theme. His experience at the Manchester School of Technology forced him to the conclusion that the education of the child in a general way, hitherto adopted, was unsatisfactory; for, as he said, "if you want a man to be a rower, you would teach him to row"; and, similarly, if you want to produce a good business man, you must teach him something that he can find useful in his career. The great point at issue is what is the real aim of education, a point on which there is no agreement as yet. In his opinion, education is not an end in itself, and is only useful in so far as it fits the pupil for life and the furtherance of the good of his country. The energies of the greater part of the people of Great Britain after the war will be devoted to recouping its industrial losses, and to this end, those minds found suitable to such labour should be specially trained. It should be the duty of the teachers to find the chief bent of their pupils, what he called their "single wide interest," which should be encouraged and cultivated on special lines, but he denied the accusation that such a system would be over-specialisation. Thus every unit of the community would be an effective unit. He laid down the proposition that no man ought merely to live to work, but that he should be trained in the work which he himself preferred to all others, so that his work should become his pleasure, apart from any pecuniary consideration. He recognised the fact that there existed some very mechanical labour which could fascinate no one; but this should be reduced to seven hours a day, thus leaving the workman time for other and more congenial employment. Dealing with the Universities, he maintained that they should throw their doors open to all, provided the would-be students showed themselves fitted for such education, and not insist that the secondary schools should be the sole channel of entry into university life; and further, that if a boy was compelled to earn his living after leaving school, this should not debar him from obtaining university instruction in later years. A useful hint was given in the discussion that followed by a headmaster of an elementary school. He said he was often consulted as to a suitable career for his pupils, but the method of education was such that the pupil had no free time in which to develop his own interests, and in consequence he (the master) knew nothing of his pupils except their success or failure in examinations.

Port Sunlight and the Housing Problem

Port Sunlight is the materialised expression of the ideals of Sir William Lever, carried out in the generous spirit of its founder, and its success gives him the right to speak authoritatively on the present question of the shortage of cottages, a problem which has grown infinitely more acute since the outbreak of war. The underlying principles in the laying out of the estate for the benefit of the operatives in his own soap manufactory are that man being individual as well as gregarious

needs separate dwellings to live in, instead of tenement houses in which he is herded like cattle and where, experience suggests, he rapidly degenerates; that the dwellings must satisfy the wants of the tenant even when these requirements are not quite in accord with the views of the builder, and that a little ground allotted to each cottage will be turned to profit by the tenant, and therefore become an asset to the nation. His experience in building, together with his sympathy and knowledge of the needs and psychology of the working man, has led him to criticise the schemes for better housing now mooted and to suggest what he considers a workable remedy. In Sir William Lever's opinion the matter admits of no tinkering, and some drastic step will have to be taken to insure, not only adequate building in the present, but a continuous supply for the future. He combats the scheme that has been brought forward (for the Government to take in hand all these building operations) on two grounds: (1) that it would drive the private builder out of the field through his inability to compete with the Government. Thus, if the Government were unprepared to maintain their building operations for an indefinite period, building would languish after the Government had ceased to deal with the question; and (2) that the Government is unsuited to the work, owing to its ignorance of local conditions. In proof of this statement he instances a case of a London architect erecting in Yorkshire a house eminently suitable for Surrey, but not even rain-proof in the stormy climate of the former county, and also the mistakes of Government in laying out an estate at Woolwich, where the roads and buildings were so planned that the children had to go a mile and a half to the nearest school, and the only connection with the main roads was at the two ends of the estate. As the Government architect could not deviate from the order to build 1,400 cottages, he therefore could not allow space for the necessary playgrounds, schools, etc. Sir William Lever's remedy is for every municipality to purchase suitable land in the suburbs of its own town and give it *free* to the builders for the erection of cottages at such a rent as will enable the tenants to pay their fares to their places of work and still live cheaper than they could in the slums. He says: "As to the objection that it may be unjust to the remaining portion of the population, the rates payable on the property built on this free land would not only pay for the land which was being given, but, in addition, result in a profit to the municipality adopting this policy." Neither is this giving of land as revolutionary as it seems, for it has a precedent in the principle of the free education of the people—entirely State-supported—the utility of which is rendered abortive by compelling the children to live in conditions that "absolutely neutralise all the benefits derived from education." To sum up in his own words: ". . . Dear land is the chief cause of high rents for cottage houses. The cheapening of the land will be the most powerful factor in reducing cottage rentals."

Lack of space forbids the detailing of the cogent reasons given in support of these theories and Sir William Lever's views in general, but those interested in this vital question will find them given in the *Liverpool Courier* of November 4th and 6th; *Three Addresses by Sir William Lever, Bart.*, at Bolton, October 7th, 1916, Manchester, October 20th, 1916, and Liverpool, November 8th, 1916; and *Port Sunlight, a Record of its Artistic and Pictorial Aspect*, by T. Raffles Davison (Batsford, Ltd.).

Bos, Probus, and Pacifism

Once upon a time there was an old farmer called Bos, who lived in a clearing in a great forest. By dint of hard work he had gradually become rich; and

therefore sent his sons, Probus and Pacificus, to be educated in the distant city at the academy of the famous Publius Schola, who, in conformity with his renowned principles according to which knowledge is of no importance, taught them nothing but character based upon grammar—with the result that they both became most admirable persons. Thus when they returned home after their education had been completed, Probus and Pacificus did not care to do any farm work, but discussed the philosophy of Plato and Balfurius. Probus was so scrupulous in his dealings with every one that when he bought anything he gave the vendor all his (that is, his father's) money; and Pacificus had such a sweet temper that if a little dog bit him on the calf he would merely turn round and pat it. Bos had another and an older son called Manlius, who was, however, of no account because he had no character and was employed merely upon the mean work of the farm.

In the same forest, there lived at some distance another farmer called Gerymon, who was very envious of Bos because his own clearing was not so sunny as that of Bos and was situated further away from the high-road; and he spent his time in drinking beer and sharpening the swords and daggers which hung on his wall, instead of confining himself to the cultivation of his excellent potato patches. Now this Gerymon was a big stout man whose temper had become very bad and jealous with his potations.

One morning Probus and Pacificus, who had got out of bed at 11 a.m., and were walking in the forest airing scented handkerchiefs in the sunshine, discussing the existence of ghosts with the most admirable lodgic, making remarks upon the beauty of butterflies, and writing sentiments in notebooks with jewelled pencils, heard a most terrific noise. On looking up they found Gerymon attacking their father Bos. Gerymon's belt was full of enormous swords and pistols and he was hammering Bos down to the ground, because the latter, who had become distinctly stupid in his old age, had nothing to defend himself with but a bludgeon, and no weapon of offence but a contemptible little sword, which was of the finest temper but was too small to reach his adversary. Seeing his sons, Bos shouted for help; but they held back. "My dear fathah," said Probus, "I am not at all sure whether your quarrel is a just one. In the nachah of things dear Mr. Gerymon has as much right to your potato patch as you have, and I think it would be distinctly wicked in me to join in this struggle." Pacificus also said, "It is quite against my principles to contend with any one. I fear, my dear fathah, that it would be wicked in me to assist you, though you are my fathah."

With that Gerymon knocked over poor Bos and was just about to jump heavily on his body, when Manlius (helped by some neighbours) rushed in to save his father, overpowered Gerymon, and tied him up hand and foot so that he could not do any more mischief. Then Manlius and the neighbours, who had observed the conduct of Probus and Pacificus, said, "You miserable curs, do you not possess enough character to understand that a man must help his own father when in mortal danger, right or wrong, and not wait to argue?" With that they beat them about all through the forest; and when Bos recovered from his shock he forbade them to consider their characters any more, forbade them also to continue their studies on bergosophy and lodgic, and made them hoe potatoes for the rest of their lives. Poor Gerymon also remained tied up for a long time and gradually grew so thin that his skin hung in folds upon his ample chest—and he too became wiser in time.

Roman Brigandage—New Style (Sir R. Ross)

Men of science have many troubles to contend with, and perhaps the most disheartening of these is due to the fact that, if after much labour they succeed in making some important discovery, then certain brigands often fall upon them in the darkness and rob them of their credit. Prof. Dr. Pietro Canalis of the Institute of Hygiene in the University of Genoa has recently reconsidered[1] and further exposed an old attempt of this kind—which, fortunately, failed.

One of the scientific discoveries of the most direct importance to humanity ever made was that of the cause of malaria, by Dr. A. Laveran in 1880. For five years no one believed him; and the scientists in Rome (which is very fortunately situated for the study of malaria) led the way in this scepticism because they then accepted an extraordinarily spurious speculation which attributed the disease to a bacillus. Later, however, when the evidence for Laveran's protozoal parasite became overwhelming, largely in consequence of the work of Canalis, they turned round, added a few secondary details, and then claimed Laveran's discovery as their own! I am, of course, familiar with the subject because I frequently lecture upon it; and have no hesitation in pronouncing the piracy to be one of the most impudent ever attempted. Laveran has frequently exposed it in his books on malaria, but the Roman bandits still persist in their falsifications, and it is in reply to the last of these that Prof. Canalis has written his recent article.

I may as well take the opportunity to quote some hitherto unpublished letters from Koch, Laveran, and Lord Lister upon what was, if possible, a still more impudent piracy, by some of the same group of scientific pretenders—this time, of my own work on malaria. That work was done between 1895 and 1899, mostly in India, and proved that the parasites of malaria pass an "alternate generation" in certain mosquitoes—this being the first time that the law of metaxeny or alternating generation was extended to the protozoa, thus opening a new door for parasitology, showing exactly how the infection of this most important disease is conveyed, and indicating the kind of mosquito which conveys it. But my work was frequently interrupted by various circumstances,[2] and before it was completed, Profs. Bignami, Bastianelli, and Grassi of Rome, who had previously disbelieved in the mosquito hypothesis, read my preliminary publications, obtained some of my specimens which I had sent from India to England, and then proceeded to discover my discovery independently for themselves. Their writings are the most perfect models of intentional scientific plagiarism ever printed, and the book of the last-named ingenious gentleman, entitled *Studi di uno Zoologo sulla Malaria*, published by the *R. Accademia dei Lincei* (sic) in 1900, commences with many pages of abuse of my work with falsifications precisely of the same indescribable nature as the Germans now employ against the Allies in connection with the war!

Unscrupulous persons can easily do this kind of thing in science, where there are seldom more than three or four persons who know the facts. On February 10, 1901, however, Robert Koch, who was of course intimately acquainted with the whole subject and had been the first to confirm my work, wrote to me:

"In my opinion it is not permissible to take account of the moral defects of a man even if he is a rogue, in considering his scientific deserts so far as he possesses

[1] "Per una più completa ed esatta storia sugli studi della malaria," *L'Igiena Moderna*, Genova, Novembre 1916.

[2] See my Nobel Lecture, *Journ. Roy. Army Medical Corps*, London, April, May, June, 1905; and also my *Prevention of Malaria* (John Murray), London, 1910, 1911.

any. For this reason, although I consider * * * to be a rogue and a robber in scientific domains, I should not pass over his scientific deserts where they ought to be mentioned. But it is my conviction that he has no such merits. What he claims as his, is either stolen or fabricated, and the remainder is too small for me to consider myself under any obligation to mention it as a valuable addition to knowledge. Statements regarding the development of the malarial parasite in the stomach of the mosquito, if he really saw them as he states (which, by the way, I do not believe), are only a confirmation of your discoveries. His illustrations are nothing more than copies of yours. The first infection experiments, which were made in Rome by * * * and his colleagues, and so very loudly advertised to all the world, I consider to be inventions; for they were made in a season when there are no fresh infections in Italy. Unfortunately * * * is not the only one in Rome to manufacture science in this manner. His immediate colleagues do not behave better, as I myself experienced. And in earlier times * * * tried to rob Golgi and Laveran in a similar way as * * * has done with you—Laveran has described it in his *Traité du paludisme*, 1898, p. 42, note 2. It seems to me one has to be very careful and also very sceptical as regards the so-called Roman School. I do not believe these people further than is indisputably proved and is testified to by reliable witnesses."

I sent this letter to Dr. Laveran and he replied to me on March 26, 1901:

"Je vous remercie de votre lettre, ainsi que de la copie de celle de Koch. Je connais trop vos adversaires dans ce débat pour m'étonner de leur manière d'agir à votre égard. Ceux qui ont suivi attentivement vos recherches savent à quoi s'en tenir au sujet des assertions de Grassi, mais c'est le petit nombre. Vous faites donc bien de vous défendre et de dévoiler les procédés mesquins et malhonnêtes de ceux des auteurs italiens qui cherchent a diminuer à leur profit l'importance de vos travaux." [1]

I then sent both these letters together with the facts of the case to Lord Lister—at his request, as he was then investigating the history of the subject for his address as President of the Royal Society; and he replied on April 1, 1901:

"Your letter is not too long, considering the interest of its contents. It is terribly sad to think of such roguery in men who call themselves scientific; but you show too serious grounds for believing that it exists in * * * and Co. . . ."

Compare his address, 1901.

Lastly, next year, the Committee of the Nobel Medical Prize, after scrupulous examination, finally adjudicated upon the claims of the pirates. Nevertheless, I see that some of the more simple people who write upon these matters without verifying statements still believe in their pretensions. This is largely due to the fact that, by an astute Italian trick worthy of Cæsar Borgia, B. Grassi dedicated his book to a third person who has always been good enough to claim me as his pupil, but who never had the decency to disclaim the falsifications about my work published under the sponsion of his name.

Another case was that of the late Prof. Calandruccio of Catania, who in 1902 complained to the Royal Society that it had given a medal to the Zoologo mentioned above for work which he (Calandruccio) had not only really originated but had mostly done! And this was a hard case because Calandruccio never received adequate recognition for his labours (on the life-history of eels).

So long afterwards, such occurrences have their humorous as well as their serious side, especially when the bandits are foiled; but at the time my experiences so disgusted me with the not very admirable class of men who seem to be engaged

[1] The English journals were not bold enough to publish my reply, and it had to be brought out in Italian!

in these branches of science that I could never bring myself to touch parasitology again. This has, I know, been the feeling of others also. In my opinion it is the duty of learned societies to take vigorous action against any taint of such dishonesty—as the Royal Society did in the case of Newton and Leibnitz. Conversely it is disgraceful that a society like that of the Lincei should allow such thefts to be perpetrated under its *ægis*; and, I consider, the Royal Society was far from blameless in the Calandruccio-Grassi affair. Learned societies are, however, extremely timid, and seldom have the courage to stand up against any abuse, however flagrant. It is a pity that a science of such importance as parasitology should so often be done in what may be called the Grub Street of Science.

The First of the Protozoologists (Sir R. Ross)

When a man leaves the Royal Society (which is seldom left except in one way), he is generally dismissed with a biography; and the writers of these biographies must often find the task difficult. In the biography of the late E. A. Minchin, Professor of Protozoology of the University of London, he is called by the writer (a zoologist) "the first of the protozoologists of his time." Unfortunately the same claim was made for the late F. Schaudinn—another case of war between England and Germany. As there were only two professional zoological protozoologists, one claim must be right and the other wrong. I believe that I was the first to suggest the word protozoology in 1898 when I sent a paper on my researches on malaria containing the word to a zoological publication. Unfortunately the editor refused the paper because he could not find the word in a dictionary, and because I did not insert the zoological names of the mosquitoes which I had experimented with, the names not being then ascertainable in India—while I am a mere doctor. But, letting that pass, it is curious what a divergence there often is between professional science and creative science. Protozoology is of great importance to the human race because it deals with certain minute parasitic animals which cause some of the most widespread and devastating diseases of humanity; and the truth is that nearly all the great work on this subject has been done, not by professional zoologists, but by doctors. We owe to a French doctor, Dr. Laveran, the discovery of the very important parasites which cause malaria, and to Sir David Bruce many invaluable researches on the dangerous organisms called trypanosomes; and my work on malaria has just been mentioned. It was really these investigations which established protozoology, just as the work of doctors established bacteriology. Nevertheless the protozoologists have tried to claim the chief credit regarding protozoology just as the botanists tried to claim the chief credit regarding bacteriology.

Minchin was appointed to his Chair by a Committee consisting chiefly of zoologists, and was selected in preference to workers who had really created the subject—a sort of thing often occurring in England. The successful candidate had really done little of importance in the line except translate certain zoological papers; and his subsequent work was laborious, but can scarcely be called anything more than good, and an Irishman might even describe it as the extreme of mediocrity. Schaudinn, also, owes the high claim made for him to the fact that he was a zoologist and not a doctor; and he was hailed all over the world as the creator of protozoology on the strength of a fantastic and utterly unproven theory that different kinds of parasites are able to develop from each other—a hypothesis which the zoologists swallowed with open mouths as a great discovery. Here again, with our characteristic generosity, when a well-paid chair dealing with such

subjects was founded in Cambridge, we offered it first (I understand) to Schaudinn, probably because he was not an Englishman and was a zoologist—though the whole subject had been founded on the labours of Englishmen who were not zoologists. So the world goes. I am beginning to think that almost all big scientific work is done by amateurs, or at least by men who were amateurs when they did the work; and that professional scientists write the text-books and obtain the credit! But I mention all this old matter now in SCIENCE PROGRESS (as I have mentioned the matter in the previous note) only in order to indicate some of the troubles which attend scientific research—and I have no longer any personal interest whatever in the subject.

Pamphlets and Periodicals

The newspapers of to-day everywhere reflect the dawn of a better industrial era and a probable solution of the difficult problem of Capital and Labour. The equal rights of both are championed by Sir William Lever in his addresses at Manchester, October 20, 1916, and Liverpool, November 8, 1916, and are discussed by the *Australian Manufacturer* in an article entitled "About Strikes and Industrial Unrest" (November 25), which gives a similar scheme for the harmonising of these conflicting forces. But Sir William Lever takes a more comprehensive view of the situation, and his ideas of "prosperity sharing" for both parties are worked out in fuller detail. The article "Facing the Man Problem" in the same issue of the *Australian Manufacturer* treats the subject from a rather different side, and outlines a practical method of raising the standard of human efficiency in factories by making it worth the while of the working man to put forth his best efforts. The effect of the lack of application of science to industry in this country is forcibly brought out by the publication (*Australian Manufacturer*, December 9, 1916) of the result of a British census of production in 1907, and a corresponding census in the United States for 1909. This list covers a wide range of industries and shows that the net produce per worker per week in the United States is in most cases double, and in some more than double, that of our own country. The writer says: "These figures prove beyond all possibility of disputation that the wonderful expansion of trade in America and the wonderful prosperity of that country are due mainly to the fact that science, invention, and organisation have been drastically applied to industry, with the result that labour has become more productive." The subject of Science and Industry receives attention in the *Scientific Australian* (December 1916), which advises a Central Laboratory subsidised by manufacturers to ensure the maximum output of the chemist's knowledge together with secrecy for the manufacturer in regard to his particular trade processes, and gives an account of the work done on these lines at the Mellon Institute—work which has had successful results. That other branches of science still await full recognition is shown by the following exerpt from Prof. T. Brailsford Robertson's article on "The Strategics of Scientific Investigation" in the *Scientific Monthly* of December 1916: "When the master-investigator of all time, Michael Faraday, ventured . . . to apply to Government for a minute fraction of the recognition to which his incalculable services had entitled him, he was received by Lord Melbourne with the epithet 'humbug.' In 1914, when one of the greatest medical investigators of our day preferred a similar request to Government, his plea was received by the official overlords with the silent contempt of utter indifference." The article also puts in a plea for scientific research *per se* to counteract the tendency of the State

trying to run the gold of science exclusively into the mould of industry. The whole article is well worth reading—together with "The Historical Continuity of Science" by the same author (*Scientific Monthly*, October 1916). *Science* speaks a useful word in its issue of January 19, 1917, when it says that "Specialists themselves go wrong if they never fully realise the broad foundation of their specialities. A narrow specialist is a bad specialist." Specialists of the future should "have a broader foundation for their work through a better realisation of the dependence of one branch of science upon the others." It is to meet just this need that we publish every quarter our section on "Recent Advances in Science."

The *Aurora* under the command of Capt. J. K. Davis, with Sir Ernest Shackleton on board, arrived at Wellington, N.Z., February 9, bringing the survivors of the Ross Sea party of the Imperial Antarctic Expedition. According to *Nature* no new geographical discoveries were made, but the meteorological records are of value.

The Imperial College of Science and Technology, acting on the instruction of the Ministry of Munitions of War, has published a valuable Memoir on British Resources of Sand Suitable for Glass-Making, with Notes on Certain Crushed Rocks and Refractory Materials, by P. G. H. Boswell (Longmans, Green).

The utilitarian factor of education advocated by Dr. Garnett (*Vocational Education*, page 666) is upheld in the *Hindustan Review* (December 1916) by Miss E. H. White in her article "Education—to what End?" She urges the unifying of aim in our different educational bodies and expresses the hope that India may be to the fore in the matter of reform. This idea of unity is also emphasised by Sir Napier Shaw, Sc.D., F.R.S., in his pamphlet *The Lack of Science in Modern Education* (Lamley & Co.), in which he recommends the Chancellors of our Universities to sink their rivalries and to meet in order to define their educational aims and to construct a framework into which the requisite detail might be fitted. His pamphlet is also remarkable for the view he takes that Government is not the only culprit in the failure of science to reach the public, and he shifts part of the blame on to the body of scientists of which he himself is a distinguished member. He considers that "when science entered upon its greatest development we began the degeneration by borrowing from the older studies the practice of controlling education by examinations with a limited time, admirable for Latin prose or arithmetic or elementary algebra, but ruinous for natural sciences." Those who favour the opinion that the cause of science might be furthered by an expansion of the method of popular lectures will like to read a small publication of the British Association of what occurred under this head in Section L (Educational Science) at its meeting in 1916. "More about Science in our Schools" (*Australian Manufacturer*, November 25, 1916) is a welcome proof that the educational reform now going on in England is not confined to these shores but is spreading throughout the Empire. And this world-wide wave of educational reform has touched not only our own Empire, but also that of Russia, where "The Ministry of Education," says the *Times* (Russian Section, January 27) "is at the present time engaged in drafting programmes for lower elementary schools," and is endeavouring "to create conditions under which pupils of elementary schools may really understand the living nature that surrounds them." According to this article ("Educational Reform in Russia") the chief difficulty at present seems to lie in the formation of a really adequate teaching staff, but appears to be in the process of being overcome.

The subject of Forestry, which unfortunately arouses so little interest in the general reader of this country, is ably represented by *American Forestry*. The number for December 1916 contains, in addition to discussions of its main theme, many interesting and beautifully illustrated articles on kindred matters such as bird-life, game-preserving, and parks.

Mr. J. Holland Rose in *The New Europe*, February 15, challenges President Wilson's unfortunate phrase "Peace without victory," and brings forward incidents of history to support the usual view that an indecisive war will only prove the forerunner of fresh wars. Some American papers, however, do not admit that President Wilson's phrase bears this interpretation, but claim that he intended to convey that after victory was gained the victor should not demand the uttermost farthing from the vanquished.

Mr. Arnold J. Toynbee has made an attempt to rouse the British mind from apathy to action with regard to the Armenian Atrocities in a penny pamphlet bearing that name and prefaced by Lord Bryce's speech in the House of Lords (Hodder & Stoughton). One hopes that the time is not far distant when a nation will not be allowed to murder 800,000 defenceless people while the world sits idly by contenting itself with a mere verbal expression of horror. The *Revue Philosophique* contains in its January number able essays on the subjects of Energy and Force, and of Liberty. The November number of *Scientia* includes good articles on "The Influence of the Theory of Evolution in Morphology," by E. S. Russell, and "The War and International Law," by G. H. Goudy.

This and That (The Editor).

The Report of the Speaker's Conference on **Electoral Reform** was issued on January 30. It is proposed to give the vote to "every person of full age, not subject to any legal incapacity, who" . . . occupies premises of not less than £10 yearly value. The representation of universities is to be maintained. Redistribution is governed by the principle that each vote shall, as far as possible, command an equal share of representation. Oxford and Cambridge are to return two members each; the remaining English universities are to return three members; the Scottish universities the same number: and the possession of a degree shall be the electoral qualification for these constituencies. Proportional Representation is largely accepted throughout the report. At a general election all polls are to be held on one day. The expenses of elections are to be largely reduced and borne by the State; but a candidate must lose his nomination deposit of £150 if he does not obtain an adequate proportion of votes. Candidates' expenses are to be allowed a maximum of sevenpence per elector, or less. The Conference objected to the "growing practice" by which political organisations incur expenditure in the furtherance of the views of particular candidates. Soldiers and sailors are to have votes under certain conditions. Some measure of woman suffrage is to be conferred, and absentee voting allowed.

On the other hand the total number of members of the House of Commons for Great Britain (567, with an additional 103 for Ireland) is to remain substantially the same. The suggestions made in SCIENCE PROGRESS for January last by Mr. Cowan, namely, that the duration of parliaments be fixed, and that voting in the House be by ballot, dealt, I suppose, with questions outside the terms of reference of the Conference. On the whole, many will think that these terms were not wide enough; and, also, that the recommendations do not strike hard enough at the principal defect of our political system—the tyranny of the party

caucuses. These actually have the power of commanding that a constituency shall elect one of only two or three candidates previously allowed by the caucuses to stand; so that the field of selection offered to the British voter largely contains political schemers, while the best men in the country are generally automatically excluded. That fraud, the two-party system, is one of the evil results of this monstrous anomaly—which produces the rule of the tongue rather than of the brain.

Regarding the **War**, in the *Times* of February 8 the Poet Laureate narrates that in 1913 a Prussian informed an American that when the Germans had conquered England they would impose an indemnity of £20,000,000,000, and would extract it from the people by aid of the lash. But this amiable design dates from much further back; for I remember that when I was at school, during, or shortly after, the Franco-German War, our German Master said something of the same kind to our Mathematical Master, who replied, laughing, "Oh, our police force will be enough to keep you out!" The fact is, that the successful German campaigns against Austria, Denmark, and France taught them that scientific warfare may be a paying game. That is, the whole German nation has been secretly at heart a nation of brigands for the last fifty years, and fat, unscientific England has long been their ultimate objective. As Mr. Bridges suggests, they missed their spring in the present war only because we joined in the fray before they had the time to crush France. If we had not done so, they would next have absorbed Holland and Denmark, and would then have gorged our millions at leisure. Perhaps the principal sorrow which the war has inflicted upon humanity has been the revelation which it has given of the unutterable beastliness of a large proportion of the human race—and the blindness and stupidity of another portion.

In referring to the able reply which **Dr. Charles A. Mercier** published in SCIENCE PROGRESS of July 1916 to an educational manifesto, the *Australian Manufacturer* of October 28, 1916, makes some of the best remarks on the matter which I have seen. It says: "Dr. Mercier has little difficulty in completely demolishing the foolish contentions of the advocates of what is called classical education, but what is really monkish education; for it was the monks of the middle ages, and not the great thinkers of Greece and Rome, who, instead of using their own brains, were content to spend their lives copying the writings of dead thinkers. The great Greeks thought for themselves. And the thinkers of one age did not hesitate to reject the teachings of the thinkers of other ages. Socrates did not flinch from rejecting the popular teachings of his time. Nor did the ancient Greeks waste their intellectual energy acquiring a knowledge of dead languages. Their own great language was good enough for them, and ours ought to be good enough for us. Certainly, in these days, when there is keen industrial competition between the nations, and when science is proving itself to be so helpful to industry, we cannot afford to devote to a knowledge of words the time and thought that ought to be given to a knowledge of things. Science is of supreme importance, because it helps us to earn our bread—to live and to prosper. And since life must come before art and culture—for there can be no art and culture if there is no life—the claims of science are primary. . . . There is nothing in science, which is merely a deeper and more accurate knowledge of nature, calculated to make a man a materialist in any objectionable sense. We know, as a matter of fact, that the great modern scientists were kindly, loving gentle men. In private life Darwin, Huxley, Lord Lister, Tyndall, Faraday—men whose souls were steeped in science—were all pure-minded, pure-living, gentle souls, a fact which gives the lie to those who talk as if the nation will go to the devil if it

insists on teaching its children the living truths of God's living Universe. . . . The man who says that men would be degraded by science knows nothing about science and less about men."

It is good to know that **Zeppelins as big as cigars** have been sighted! When some people wish to describe the angle subtended by a body such as a comet or a Zeppelin at a distance from the eye they are very apt to say that the said body appeared a foot or a yard or so in length; and we all remember the classical case in which a cloud was described as being "like a man's hand." But as the describer does not define how far off the foot or yard or man's hand is from the eye, the listener is unable to form any notion whatever of the angle referred to. It shows how defective our system of education is, that this type of description appears in our press after each Zeppelin raid, and harrows the intelligent reader almost as much as the bombs do. For instance, one evening paper of September 25, 1916, said that a Zeppelin "did not look more than six or eight feet long." The *Pall Mall*, of a forgotten date, made an equally brilliant remark; but horror really entered into our souls when the great *Times* of September 27 said that a Zeppelin "looked only about the size of a big cigar." Now, a cigar may look any size according to the distance it is held from the observer's eye. If it is an inch from the nose a cigar will subtend nearly half the horizon, and if it is a yard away it will look smaller, and at a mile's distance will be invisible. Which of all these distances are we to apply to the description of "our own correspondent"? Yet the *Times* is the Boanerges of the classical (? grammatical) education. Again, on October 3, another correspondent tells us in the *Times* that "with the aid of a night-glass she appeared about a yard long (experts can estimate how high she was)"! Some people seem to think that experts can do anything.

In the **Poetry Review** for January last Mr. Theodore Maynard says that "For the last twenty years or more poetry has been left by the English to languish in the dungeons of derision. The very nation which has produced more great poets than the rest of the world combined has treated its poets worst." The accusation was, of course, immediately and indignantly repudiated in the press, as, for instance, in the *Literary Supplement*. The truth of it can, however, be easily demonstrated by figures. That fine literary review, the *Supplement* itself, actually boasts that it sells over thirty thousand copies every week. Taking the population of the United Kingdom at forty-five millions, we see that only one copy of the *Supplement* is sold for every thirteen hundred and fifty persons in the kingdom—not counting the Colonies—surely a quite insignificant proportion. The price of the *Supplement* is one penny, and yet every day in the trains we see hundreds of worthy citizens reading trash of the same monetary value purchased several times daily. As a writer in SCIENCE PROGRESS for October last pointed out, "We may doubt whether, excepting reprints of classical works, sold chiefly for prizes and presents, and 'topical verses,' as many as five thousand books of poetry are disposed of annually to the forty-odd millions of people in the country." Yet, as poetry may be defined as the perfected utterance of the human spirit, we might suppose that a larger proportion of people in an intelligent country might take some interest in it. Probably, however, not one Briton in a thousand ever looks at it. To me it seems that the neglect of poetry is due to the same cause as the neglect of science, namely, intellectual sloth or a low standard of intelligence; and philosophy, art, sanitation, and nearly all the branches of effort which are really useful to the human race are in the same case, while games, party-politics, money-making, low drama, and dogma are the things which most interest us as a nation.

At two meetings of the **Poetry Society** (16 Featherstone Buildings, Holborn,

London, W.C.) I suggested that Government should give the Society apartments in order to provide a reading-room and a library, as it does in the case of many learned societies and the Royal Academy. My proposal has been erroneously taken to mean that I wish the Government to subsidise the writing of bad poetry. Not at all. The whole of the British Empire, do what it will, cannot create a single great line of poetry, just as it cannot bring about a great scientific discovery. That was not my point at all. But a place of meeting where the greatest of the arts can be displayed to the public will be an educative measure of the first importance for a nation which requires to have its low intellectual tone raised. Frankly, I do not believe much in the intellectual capacity of the modern British nation, and think that if it had paid a little more attention to the great arts and sciences, and much less to the amusements mentioned above, it would have been a much greater people and more worthy of the Empire which fate and our forefathers have given to it. Is it not shameful that out of some fifteen million pounds spent annually in this country for so-called education, scarcely a penny is devoted to the honour and the teaching of the greatest of the arts?

The following is an amusing example of the very clever manner in which some of our **learned societies** are managed. A certain Medical Society long possessed a laboratory which it was unable to use because it could not pay for the petty expenses. Some years ago, however, it appointed a medical man as Honorary Director, and this gentleman found friends who were generous enough to pay the said expenses. Presently the laboratory grew so much in importance that an extension was required, together with the purchase of much new apparatus. The same donors again came generously forward, and put down a substantial sum of money expressly for these purposes. Now, however, the Society apparently began to think that some of the cash might be used with advantage for other purposes; allotted certain small sums for various items not in connection with the laboratory; and then, without the knowledge of the Director, wrote a letter to the donors so cleverly worded that, if the contained suggestions had been accepted by the donors, the Society would have been able to take more than one-third of the total sum for its general purposes. Fortunately the donors, who were clearly more astute than the poor Society, smelt a rat, and refused the suggestion. This little *contretemps* may have been excused as being due to the excess of zeal on the part of the Society's officials, but unfortunately, the Society now adopted a very lofty tone, and instead of apologising to the generous donors, tried to fix blame upon them! The ultimate result was that the Society was obliged to give back the residue of the money, together with all the apparatus purchased under the gift; and, still worse, one of the donors, who without any one's knowledge had been good enough to bequeath no less than five thousand pounds to the Society, now withdrew that bequest. Thus the poor Society is left mourning; and the Honorary but dishonoured Director will probably think twice before trying again to collect money for science and learned societies.

The **Athenæum** (September 1916) says, "We hope it will be recognised that men of science have much to learn in the way of clear expression of their results." Agreed; and, while literary persons invariably express themselves perfectly, we hope they will recognise that they seldom have anything to express. It would be an excellent thing if a literary man were to be appointed to every laboratory in order to attend to the style of the investigators and, also, to learn the difference between real and imaginary work.

After the war something like **starvation** will face all the nations of Europe, combatant and non-combatant, simply because the food-makers have so long been

employed in fighting. It is curious that the Germans do not seem to have reflected how their foul submarine war will ultimately recoil on their own foul heads—because if it is successful there will not be left enough ships to carry American food to them after peace is declared. We may suspect that the war after the war will be the more terrible.

British law is beyond the understanding. On February 8 Mr. Justice Coleridge fined two young men for attempting to bribe persons serving in the Army Clothing Department, and was reported to have said that "having regard to the small sums which were received, and to the circumstance that no public mischief was in fact done, he would not imprison the defendants." Such judgments may appear inexplicable to many. Should a man who has attempted to murder another for money, or to forge a cheque, be excused merely because the sums involved were small or because the attempts were detected in time to be prevented? Is crime mitigated by failure or smallness of stake? Yet this is war-time, during which the supply of inferior clothing to soldiers in the field may have the most serious results for the men who are sacrificing their lives for their country. On the other hand a tired soldier may be shot for sleeping on duty!

A writer in **Science** of January 5 makes the very good suggestion that publishers should issue advertisements of new books on cards of 3 by 5 in., suitable for filing—so that the recipient of the advertisement can always file the card in his card index, whether or not he buys the book at the moment when he receives the advertisement. Of course the card should give all the details regarding the book which are usually given in card indexes, and be also inserted in publishers' advertisements. Messrs. John Wiley & Sons, the American publishers, have already agreed to adopt the suggestion.

Mr. Murray has been well advised to reissue for a cheap price (2*s*. 6*d*.) that admirable old work "**The Moon**, considered as a Planet, a World, and a Satellite," by James Nasmyth and James Carpenter, originally published in February 1874. The work is really excellent as a semi-popular textbook. The lucidity with which the authors state in the beginning of the book the general principles of spectrum analysis, conservation of energy, and gravity is quite admirable; and the photographs of the surface structure of the moon might tempt one to explore such a wonderful country if possible. These photographs were prepared from plaster models of the moon constructed by Mr. Nasmyth; and I asked Mr. Murray to let me know his interesting reminiscences on this matter. He writes in reply: "What I told you about dear old James Nasmyth was this: for many years of his later life, his two hobbies were telescope work and gardening. For a very long time he made the moon his special study, and every clear night he used to map out a bit of it, draw it, measure it, and test all his figures. From these he made a plaster model, and this again was always tested and re-measured until at last he got what was approximately a model of the surface of the moon, far nearer to the truth than anything else that had ever been done before. This model he photographed, and the photographs form the main feature of his book. It had a great success, and scores of people used to apply in a casual way, asking if they might copy his photographs for some use of their own; but old Nasmyth was very firm about this, and would not allow any copying, because these photographs represented years of labour. They were unique, and they form the most valuable part of his volume. He was a most charming old man, and many a long conversation I have had with him. Amongst his sayings were these: 'A man ought to have eyes at the end of his fingers'; and 'Unless a man can bore a hole with a saw, and cut a board with a bradawl, I do not think much of him as a workman.' This,

of course, was to illustrate the necessity of resource in all handicraft." Some of the remarks in the book are perhaps a little out of date, but it should be read in connection with Mr. H. G. Wells' *The First Men in the Moon*. After all, most boys learn their science, and perhaps learn it best, from such works as these.

Income-tax anomalies are the constant theme of letters to the press, but remain unamended in spite of all protests; while our Houses of Parliament, instead of attending to the business of the country, spend their time in talking the platitudes called politics. In the *Times* of December 28 last, Mr. Bernard Shaw called attention to the facts that, while "windfalls" obtained by inventors are not taxed as income, similar sums obtained by writers for exceptionally successful books or plays are so treated—so that they may actually have to disburse a third of such sums to the State—a gross abuse amounting to mere confiscation. In the *Times* of next day Prof. H. H. Turner exposed another absurdity, the *per saltum* method of fixing higher rates of income tax. Thus a man with a gross income of £1,010 is taxed at the rate of 3*s*., pays £151 10*s*., and has a net income of £858 10*s*.; nearly £10 a year less than a man of a gross income of only £992, who, because he is taxed at the rate of 2*s*. 6*d*., pays only £124. Prof. Turner says: "I venture to suggest that the whole machinery of graduation by sudden steps is wrong, and is an instance of the lamentable unfamiliarity of those in high places with the simplest mathematical procedure." Quite so; and they are ignorant of almost everything else, and, moreover, do not do the work for which they are paid. In the *Times* of March 23 Mr. John Galsworthy points out that a professional man who has been earning £3,000 to £5,000 a year and then abandons his work for national duties, say on £300 a year, will still have to pay income tax on the average income of three years, amounting to over £1,000! Would it not be possible to employ Parliament during the war on compulsory and really useful national work by making it sit down to amend the numerous anomalies which exist in our laws and administration?

On March 21 **His Excellency M. Paul Hymans**, the Belgian Minister, read an essay by the late Emile Verhaeren before the British Academy, and afterwards said that Belgium deserves the support of the whole world against the barbarians on account of its magnificent contributions to art. He was quite right, because nations, like individuals, vary in merit more than in size, and those deserve most from the world which contribute most to the world's civilisation.

The great political events of the moment are the revolution in Russia, the probable entry of the United States into the war, and the Report of the Dardanelles Commission. What would a philosopher conclude after considering all these matters together? The Russians appear to think that they have won a happy future by deposing the Tsar and adopting democratic forms of government; but when we read the Dardanelles report, we doubt whether this form of government is really in any way superior to despotism. Similarly we observe the monstrous mistakes made by despotism in Germany, and on the other side the curious hesitation of the Americans in joining the forces of civilisation against the savages. Wherever we look we seem to see nothing but misgovernment. Really, autocracy and democracy come to the same thing in the end—the mismanagement of the world. After the war the world must find a better method of government than it has yet created.

ESSAYS

The Art of Perpetuation (Bruce Cummings)

JUST as the ancient hunter shot a fish with a spear, so we may imagine the ancient philosopher separated the Thing, caught it up out of the Heracleitean flux, and transfixed it with a name. With this first great preservative came the first great museum of language and logical thought. Ever since we have been feverishly busy collecting, recording, and preserving the universe or as much of it as is accessible. Perpetuation has become an all-absorbing art.

It is only recently that certain interesting, not to say remarkable, refinements in the technique of the art have been developed and come into common use, such being for example the museum, the printing press, the camera, the cinema film, the gramophone record. By the Ancient Greeks and Ancient Romans the desire to collect and above all to conserve the movable furniture of the earth was only indistinctly felt. As storehouses, museums were almost unknown. Small collections were made, but merely as the mementoes of a soldier's campaign, or a mariner's curiosities, like the gorilla skins brought home from Africa by Hanno.

The assembling of curiosities, drawing-room curios, bric-à-brac, and *objets de vertu* was still the immature purpose of the conservator even so late as the days of Sir Hans Sloane, Elias Ashmole, and John Hunter. Ashmole's gift to the University of Oxford was laconically described as "12 cartloads of curios." Hunter's museum, as every one knows, was a gorgeous miscellany of stuffed birds, mammals, reptiles, fossils, plants, corals, shells, insects, bones, anatomical preparations, injected vascular preparations, preparations of hollow viscera, mercurial injections, injections in vermilion, minerals, coins, pictures, weapons, coats of mail. It is obvious that in those days the collector had not passed beyond the miscellany stage. According to his pleasure, he selected say a Japanese midzuire, a scarab of Rameses II, a porpentine's quill, a hair from the Grand Cham's beard, and saw the world as an inexhaustible Bagdad bazaar. Now he sees it as *exhaustible*, and is grimly determined to exhaust it as soon as may be.

To-day everything is changed. Mankind is astride of the globe from pole to pole like Arion on the dolphin's back. With all the departments of human knowledge clearly mapped out in the likeness of his own mind, man now occupies himself with collecting and filling in the details. He ransacks heaven and earth, armies of collectors brigaded under the different sciences and arts labour incessantly for the salvation of the globe. All objects are being named, labelled, and kept in museums, all the facts are being enshrined in the libraries of books. We are embarked on an amazing undertaking. A well-equipped modern expedition apparently leaves nothing behind in the territory traversed save its broad physical features, and as Mont Blanc or the Andes cannot be moved even by scientific Mahomets, the geologist's hammer deftly breaks off a chip, and the fragment is carried off in triumph to the cabinet as a sample.

It is estimated that there are about seven millions of distinct species of insects, and naturalists the world over have entered upon a solemn league and covenant

to catch at least one specimen of every kind which shall be pinned and preserved in perpetuity for as long as one stone shall stand upon another in the kingdom of man. There are already an enormous number of such types, as they are professionally called, not only of insects, but of all classes of animals and plants jealously guarded and conserved by the zealous officials of the British Museum.

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When I was a small boy I greedily saved up the names of naval vessels, and inscribed each with a fair round hand in a MS. book specially kept for the purpose. Now the financial or æsthetic motives that may be said to govern the boy collector of postage stamps, birds' eggs, cigarette cards, must here be ruled out of court. For if half-a-dozen of the rarest unused surcharged Mauritius, a complete set of Wills's "Cathedrals" or Player's "Inventions," or a single blood alley of acknowledged virtue minister to the tingling acquisitiveness of the average schoolboy, it is difficult to say the same of the hunting down in newspapers and books of battleships, cruisers, and T.B.D.'s. At least I am inclined to think that my subconscious motive was a fear lest any of His Majesty's ships should be overlooked or lost, that it was indeed a good example of the instinct for simple conservation uncomplicated by the usual motives of the collector.

The joy of possession, the greed, vanity and self-aggrandisement of the collector proper, are deftly subverted to the use of the explorer and conservator of knowledge who, having a weak proprietorial sense—bloodless, anæmic, it must seem to the enthusiastic connoisseur—is satisfied so long as somewhere by some one Things are securely saved. The purpose of the arch-conservator—his whole design and the rationale of his art—is to redeem, embalm, dry, cure, salt, pickle, pot every animal, vegetable, and mineral, every stage in the history of the universe from nebular gas or planetismals down to the latest and most insignificant event reported in the newspapers. He would like to treat the globe as the experimental embryologist treats an egg—to preserve it whole in every hour of its development, then section it with a microtome.

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People who are not in the habit of visiting or considering museums fail to realise how prodigiously within recent times the zeal for conservation—or, as Sir Thomas Browne puts it, the diuturnity of relics—has increased all over the world in every centre of civilisation. A constant stream of objects flows into the great treasuries of human inheritance—about 400,000 separate objects being received into the British Museum in Bloomsbury per annum, and there is scarcely a capital in Europe, or a big town in America, in which congestion is not already being felt.

In a museum you shall find not only the loincloth or feathers of the savage, but an almost perfect series of costumes worn by man down through the ages in any country. Man's past in particular is preserved with the tenderest care. It is possible to go and with the utmost pride and self-satisfaction observe the milestones of our progress from the arrow-head to the modern rifle, from the sedan chair and hobby-horse to the motor-cycle and aeroplane, from the spinning wheel to the modern loom, from the Caxton printing press to the linotype, from Stephenson's "Rocket" to the railway express engine, from the windbag of the Roman invaders to the latest ocean greyhound in miniature. It is all there: china, tobacco pipes, door-handles, iron railings, bedsteads, clavichords, buttons, lamps, vases, sherds, bones, Babylonian and Hittite tablets, the Moabite stone, the autographs and MSS. of every one who was anybody since writing came into common practice, scarabs and coins, scarabs of the Rameses and Amenheteps,

coins of Greece and Rome, coins of Arabia, coins of Cyrenaica, coins from Colophon, Tyre, Sidon,—Nineveh's Winged Bulls.

I knew a police inspector who saved and docketed the cigar ashes of royal personages, and I once heard of a distinguished chiropodist who saved their nail parings. Mr. Pierpont Morgan owns the largest collection of watches in the world, and another American is the proud possessor of the only complete collection of "Crusoes" in existence: *i.e.* the editions of *Robinson Crusoe* by Daniel Defoe.

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But not only is the past retrieved in fragments; in some museums and exhibitions, and to a certain extent in historical plays, it is actually reconstructed: in London is displayed the interior of an apothecary's shop in the 17th century, with its crocodile and bunches of herbs, or the shop of a barber-surgeon, or a reconstruction of the laboratory used by Liebig, or the Bromley Room, or Shakespeare's Globe Theatre in exact facsimile, or Solomon's Temple, while, for the purposes of illustration, Madame Tussaud's must for the moment be classed with the Pantheon. The cinema is going to keep alive the persons and events of the present generation within the most sluggish imaginations of the next—for those who perhaps won't read history or visit museums. This need not mean the gradual atrophy of the imagination, as some Solomon Eagles portend—to discuss which would, however, mean a digression. In any case, I fancy the most lively imagination would scarcely ignore the opportunity if an authentic film were in existence of seeing Dr. Johnson, let us say, walk down Fleet Street tapping each lamp-post with his stick or of listening to a gramophone record of Rachel or Edmund Burke.

Wherever one turns it is easy to see this thriving instinct of the human heart. There are enthusiastic leagues for preserving woods, forests, footpaths, commons, trees, plants, animals, ancient buildings, historical sites. In times to come, nearly every private house in London will have historical connections and bear a commemorative tablet. In anticipation of its extinction the hansom cab has already been lodged behind the portals of its last depository. Everywhere enthusiasts are expending a vast amount of energy in inducing people to stick to the old—pedants will have you use the old idioms and spellings, the language must be preserved in its original beauty; no ancient rite or custom can be allowed to lapse into desuetude but some cry of reprobation goes up to heaven in righteous anger. There are anniversaries, centenaries, bicentenaries, tercentenaries—glutinous tercentenaries!

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Perhaps the most valuable instrument for perpetuation is the printing press. No sooner is an event over, than it is reported in the daily press and the newspaper preserved in the British Museum for all time. In future there will be no historical lacunæ. In virtue of our elaborate precautions it is improbable that London will ever become a second Nineveh. Immediately a discovery is made or a research brought to its conclusion the world is copiously informed. In the present era of publicity, we need never fear that a man's secrets will die with him. It were safe to prophesy that there will never be another Mrs. Stopes, for the good reason that his contemporaries will never let a second Shakespeare slip through their fingers, so to speak. The scholar's lament over the loss of the Diakosmos of Demokritus, or the naturalist's over that of the observations of William Harvey on the Generation of Insects—destroyed in the fortunes of war—will probably never be heard again. Within the sacred rotunda of the British Museum Reading Room may be perused the novels of Charles Garvice as well as the great Chinese Encyclopædia of the Emperor K'ang-hi in 5020 volumes.

In books our knowledge to date is rounded up and displayed; you may read a book on a lump of coal, a grass blade, a sea worm, on hair combs, carpets, ships, sticks, sealing-wax, cabbages, kings, cosmetics, Kant. A very thick volume indeed was published last year upon the *thorax* of a field cricket. It would require a learned man to catalogue the literature that deals with such comparatively trivial subjects as the History of the Punch and Judy Show, or the History of Playing Cards.

At the present rapid rate of accumulation, the time must come when the British Museum, thousands of years hence, will occupy an area as large as London, and the *Encyclopædia Britannica* be housed in a building as big as the Crystal Palace; an accumulation of learning to make Aristotle and Scaliger turn pale.

For let us not forget that man is only at the beginning of things. The first Egyptian dynasty began 7000 B.C. and we are now only in A.D. 1917. Every day sees the birth of entirely new things that must be collected and preserved, new babies, new battles, new books, new discoveries, so that—to take a moderate figure—by A.D. 3000 we shall have saved up such a prodigious quantity of the relics and minutiæ of the past, that only a relatively small fraction of it will be contained in the united consciousness of the men of that time. Everything will be there and accessible, but for reference only. Knowledge will be an amazing organisation (let us hope it will be done better than the Poor Law system), and battalions of men of the intellectual lineage of Diderot and D'Alembert will be continuously occupied in sifting and arranging our stores of information whereby the curious by handing a query over the counter will be given all the knowledge in existence in any particular subject. Yet for the most part human knowledge will be left stranded high and dry in books—entombed, embalmed, labelled and clean forgotten—unless the human brain becomes hypertrophied.

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Conservation is a natural tendency of the mind. One might lay down a certain law of the conservation of consciousness to indicate our extreme repugnance to the idea of anything passing clean away into the void. What insinuating comfort in those words that every hair of our heads is numbered!

True, the chain of causation is unbroken and in a sense every effect is the collection and preservation of all its past courses, and if to live can be said to exist in results, then no man ever dies and no thought can perish, and every act is infinite in its consequences. Yet I fancy this transcendental flourish will not satisfy the brotherhood of salvationists who desire to possess something more than the means embodied abstractly in the result, nor will it cause them to abate one jot their feverish labours to forestall their common enemies—cormorant-devouring Time, man's own leaky memory, Death's abhorred shears, the Futurist, the Hun, the Vandal, the carrion-worm or the Devil.

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The instinct for conservation in different men has different origins. To the scientific man, Nature is higgledy-piggledy until she is collected, classified, stored, and explained according to his own scheme; every phenomenon unobserved or imperfectly comprehended escapes and flows past him, defeating his will to understand. In politics, conservatism means a distrust of the unknown future suited to a comfortable habituation to current customs and current statecraft or—to quote Fluellen—the ceremonies of it and the cares of it and the forms of it and the sobriety of it and the modesty of it. In still another direction, the desire to conserve is simply a sentiment for the old forgotten far-off things and tales of long ago. The

flight of time, its likeness to a running stream, the great world spinning down the grooves of change, endless change and decay, have been food for the melancholy ruminations of philosophers from the earliest times. "Tout ce qui fut un jour et n'est plus aujourd'hui incline à la tristesse surtout ce qui fut très beau et très heureux," says Maeterlinck.

But regard for the old is not always vague sentiment alone. In one of his essays, Emerson remarks that Nature often turns to ornament what she once employed for use, illustrating his suggestion with certain sea shells in which the parts which have for a time formed the mouth are at the next whorl of growth left behind as decorative nodes and spines. Subsequently, Herbert Spencer applied the idea to human beings, remarking how the material exuviæ of past social states become the ornaments of the present, for example ruined castles, old rites and ceremonies, old earthenware water-jars. The explanation of this metamorphosis simply is, that so long as a thing is useful its beauty often goes for the most part unobserved. Beauty is the pursuit of leisure, and it was probably in those rhythmic periods of relaxation when the primitive potter or stone carver paused from his labour that the æsthetic sense according to some was given birth.

Now it is certain that there be some to whom the perpetuation of Stonehenge or the Diplodocus is a matter of large indifference, in whom arises no joy in the fruits of the conservator's art upon handling, say, a Syracusan tetradrachm or a folio of Shakespeare with "the excessively rare title-page 'for Richard Meighen.'" Yet over the question of self-perpetuation these same men will be as desirous as others. Few men, save Buddhists, relish the idea of self-extinction. No one likes the thought of the carrion worm in the seat of intellect. The Egyptians bravely fought the course of Nature and gained some solace, we may assume, by embalming. Christians, if they resign themselves to the decay of the body, labour in its stead to save the soul. On his death, every man at least claims a tombstone. The surface of the earth is stippled with crosses (especially in France), with monuments, obelisks, mausoleums, pyramids, cenotaphs, tombs, tumuli, barrows, cairns, designed to keep evergreen the memory of the dead, to forestall oblivion lurking like a ghoul in the background. Look at Keats's naïve preoccupation with his future fame, his passionate desire to be grouped among the heirs of all eternity. If we are to believe Shakespeare and the Elizabethan sonneteers, their common obsession was to combat brass and stone with their own immortal lines.

No doubt there are a few apparently sincere, high-minded gentlemen ("Rocky Mountain toughs" William James calls them) who emphatically declare that when they die they will, after cremation, have their ashes scattered to the winds of heaven, who scoff at the salvation of their souls, and quote Haeckel's jibe about man as "a gaseous vertebrate," who are indifferent to fame and spurn monuments that live no longer than the bell rings and the widow weeps. In short, since conservation must always be o'erswayed by sad mortality in the long run, they will have nothing of it. "Give me my scallop shell of quiet," they would say—and let the world pass on its primrose way to the everlasting bonfire.

But conservation cannot be so summarily set aside. Every man, willy nilly, collects and preserves, for his consciousness is of itself an automatic collecting instrument and his memory a preservative. After a life of it a man's mind is a museum, a palimpsest, a hold-all. In the heyday of manhood we may perhaps go adventuring on in lavish expenditure of life, nomads, careless of the day as soon as it is over. Yet he must be a very rare bird indeed, the veteran who, when all the

wheels are run down, does not choose to write his memoirs or even to relate reminiscences around the fireside, the broken soldier who never shoulders his crutch, the barrister who never recalls his first brief. Two old men will haggle with one another over the fixation of a date, they will pull up a conversation and every one must wait on account of a forgotten name.

In 1768 Fanny Burney made this entry in her Journal: "I cannot express the pleasure I have in writing down my thoughts at the very moment . . . and I am much deceived in my foresight if I shall not have very great delight in reading this living proof of my manner of passing my time . . . there is something to me very unsatisfactory in passing year after year without even a memorandum of what you did, etc." This is the true spirit of the habitual diarist speaking. At heart, every one is a diarist. There is no child who has not kept a diary at some time or another, and there is no one who, having given it up, has not regretted it later on. The confirmed journal writer, however, possesses a psychology not altogether common, being one of those few persons who truly appraise the beauty, interest, and value of the present without having to wait until memory has lent the past its chromatic fringes.

. . . . . .

It is strange that so many gallant knights clad in the armour of steely determination should fight on unthinking against such overwhelming odds. For the conservators, in trying to dam back time, in resisting change and decay, wrestle with the stars in their courses and dispute the very constitution of the universe. But the imperative instinct must be obeyed. The ominous warnings of Sir Thomas Browne are unavailing. "There is no antidote for the opium of time." "Gravestones tell truth but a year." "We might just as well be content with six feet as with the moles of Adrianus." And "to subsist but in bones, and be but pyramidally extant, is a fallacy in duration." To erect a monument is like trying to insert a stick into the bed of the Niagara. No memorial as large and wonderful as the Taj Mahal can stay the passage of a grief, no pen can preserve an emotion held for a while in the sweet shackles of a sonnet's rules. Neither pen nor brush nor chisel knows the art of perpetuation.

As the torrent races past, frantic hands stretch out to snatch some memento from the flood—a faded letter, an old concert programme, a bullet, the railway labels jealously preserved on travellers' portmanteaux, a lock of hair. "Only a woman's hair," said Swift in the bitterness of his heart as he handled Stella's tress.

There are some things we can never hope to recall, even so long as the world lasts, except by divination or Black Magic. The hopeless science of Palæontology offers its students no tiniest ray of comfort—a Pterodactyl, a Dinosaur, or an Archæopteryx will never be disclosed to us in the flesh. There are many things lost for ever: Who was the Man in the Iron Mask? or Junius? or Mr. W. H.?—Louvain?

"All is vanity, feeding the wind and folly. Mummy is become merchandise, Mizraim cures wounds, and Pharaoh is sold for balsams"—to borrow once more from Sir Thomas Browne's organ music.

"Tarry awhile, lean earth!
Rabble of Pharaohs and Arsacidæ
Keep their cold court within thee; thou hast sucked down
How many Ninevehs and Hecatompyloi
And perished cities whose great phantasmata
O'erbrow the silent citizens of Dis."

Life is expenditure. We must always be paying away. It is sad to behold the conservators—ecstatic hearts—following like eager camp followers in the trail of the whirlwind, collecting and saving the fragments so as to work them up into some pitiful history, poem, biography, monograph, or memorial.

Why pursue this hopeless task? What is the use in being precious and saving? Nature wastes a thousand seeds, experiments lightly with whole civilisations, and has abandoned a thousand planets that cycle in space forgotten and cold. Both collection and recollection are insufficient. The only perfect preservation is re-creation. Surely our zeal for conservation betokens a miserly, close-fisted nature in us. It cannot be very magnanimous on our part to be so precious since God and Nature are on the side of waste. Let us squander our life and energy in desire, love, experience. And, since so it is to be, let us without vain regrets watch the universe itself be squandered on the passing years, on earthquakes, and on wars. The world is an adventurer, and we try to keep him at home—in a museum. Let us not be niggardly over our planet nor over ourselves.

Yet it is easy but fatuous to sit at a writing desk and make suggestions for the alteration of human nature. Conservation is as deeply rooted as original sin.

The Organisation of Scientific Literature (Philip E. B. Jourdain, M.A., Cambridge).

In this article I shall not be concerned with the important question of the organisation of scientific research in the laboratory or class-room, but with the systematisation of those results of thought and experiment which are published in various scientific books and periodicals. There is not the least doubt but that, as a nation, we need more systematic teaching and more laboratories, but the organisation of scientific literature is, in most cases, a necessary preliminary to the organisation of thought and experiment in mathematics and the natural sciences; above all, perhaps, in the science of pure mathematics—and we may almost add philosophy, now that it has become scientific—for here we are not wholly dependent even on the laboratory of nature.

The exigencies of the strenuous present have brought out clearly the utter uselessness of those vague and leisurely generalities which used to be such a common feature of articles on scientific method or its aids, and it is not the purpose of this article to deal with vital questions in the way in which they were often dealt with before the war. There was something about those articles that was very like a sermon or those long advertisements of a certain mild and not unpleasant medicine of which we used to see so much. First would come a quotation from the Bible or some poet or classical author about, say, human destiny or Plato and the butterfly; then would come various dull and estimable platitudes which seemed at first quite off the point of the whole discourse, but they gradually arrived at this point—the price of the remedy, . . . or, in the case of the sermon, the collection.

The basis of my arguments, then, will be formed by a rather narrow domain of scientific literature with which I claim a special acquaintance: mathematical literature. Even during this war, in which Germany is undoubtedly suffering far more than Great Britain, and acutely felt the shortage of compositors in its printing offices long before we did, the work of printing the enormous and almost unique *Jahrbuch über die Fortschritte der Mathematik* has gone on slowly but persistently. At the end of 1916 I received through Switzerland the first part of

the volume containing a fairly complete and well-arranged account of the mathematical literature of the whole world for 1913: usually the *Jahrbuch* appears about two years after the articles it reviews. Apart from the short quarterly reports given in this journal, which make no pretension to completeness, there are only two other systematic accounts of the progress in mathematics published: one is the *Revue semestrielle des publications mathématiques*, published by the Mathematical Society of Amsterdam in half-yearly parts, and the other is a supplement to the *Bulletin des sciences mathématiques*, published in Paris. Of these the *Revue* has the great advantages of brevity, references to all scientific reviews of books, and publication about six months only after the articles to which it refers. The notices are not critical, and only contain a brief summary of the matters treated by the author. The reviews in the *Bulletin* are much longer, but only deal with original papers in periodical literature, and do not appear in a form in which the notice of any particular paper can be found at once. The notices in the *Jahrbuch* are concerned with both periodical matter and separate books, but do not, as a rule, mention reviews and notices of books, which are not usually classed as "original contributions." In short, the *Jahrbuch*, while having the defects of appearing long after the matters of which it treats and of neglecting book-reviews, which occasionally contain quite valuable remarks, has an advantage over the other publications in being thoroughly systematic and arranged for each year in order of the matters dealt with, easy of consultation, and tolerably detailed.

The part that Great Britain has taken in the indispensable aid to research provided by such publications as the *Jahrbuch* and the *Revue* is so small that it compares unfavourably with that of practically every other European country. At the present time, judged by the number of collaborators on this international work, it is the same as Serbia. Indeed, it seems that the only European countries which do less than Great Britain in this respect are Montenegro, Monaco, Luxemburg, Bulgaria, Turkey, Norway, Finland, Spain, Portugal, and Greece. In the past Dr. J. W. L. Glaisher of Cambridge and Prof. G. A. Gibson of Glasgow have sometimes appeared among the contributors to the *Jahrbuch*, but for many years neither of these two men has been able to contribute, and at the present time the only Briton who does work for either the *Jahrbuch* or the *Revue* is the present writer. Outside Europe, India and Japan do as much as Britain, and America more. Some Britons may perhaps feel that these facts are compensated by the fine work done by the Royal Society in indexing, and by the Physical Society in publishing abstracts of the world's literature in physics. I do not see that this forms any excuse for lazy acceptance of the fruits of other nations' industry in other domains.

It is certainly necessary for the nations which form the Entente, and more particularly for Great Britain, to make a serious effort to put an end to this discreditable state of things. It may be said that this effort is necessary because Germany has used its scientific publications to help towards an establishment of German civilisation over the whole of Europe, and thus that, for political reasons, it is desirable for the Entente to put an end to its condition of dependence on German civilisation. About this I cannot offer any opinion that would be of the slightest value: an opinion, to be of any value, would have to be backed by a knowledge of the German official minds. If it is true that, as Gibbon hinted, politicians make use of the religious beliefs of others, it seems likely enough that a government would use the scientific ideals of others for its own advantage. It seems fairly clear to me that, whether or no German scientific men have been

cleverly used as tools by those in power, it is a very sincere love of science that has prompted the scientific men to do much valuable, laborious, and self-sacrificing work: it is certainly not hope of financial gain that lies at the back of their minds. I happen to know that the rate of payment offered to collaborators of the *Jahrbuch* is 1 mark 50 pfennigs, or slightly less than eighteenpence, per printed page of their writings and summaries. A printed page of the *Jahrbuch* represents about 500 words; and it is useful to remember that the cost of having what would fill one of these printed pages typewritten would be, in all, about eightpence. So, if a contributor studies, as all contributors ought to study, the patience and eyesight of the editor and printer, the rate of payment would be at the glorious rate of about one penny for ten lines of text. By the side of this, a "penny-a-liner" would seem a person who was paid at a very high rate. Further, many of the contributors prefer to receive a complete copy of the *Jahrbuch* instead of any money payment at all. It seems that those who do a vast amount of work for a reward whose money-value is less than thirty-six shillings may fairly be absolved of any motive except the creditable ones of duty and ideals.

It may possibly be of some slight interest to give an example of the eagerness with which points of view which are not those of the average German mathematician are accepted in the *Jahrbuch*. For many years past, owing to the able and important work done by mathematical logicians in Italy, Great Britain, America, and France, and owing to the lack of such valuable work by Germans, it has become almost a commonplace among mathematical logicians that the floundering attempts of German mathematicians to deal with logical difficulties are quite futile. Indeed, since the time of Leibniz, there has been only one really great logician in Germany, and Frege's splendid work is not even yet appreciated by his countrymen. At the International Congress of Mathematicians held at Cambridge in 1912, it was rather amusing to notice the invariable method of dealing with perplexing logical questions on the principles of mathematics which was used by eminent German mathematicians. When any such difficulty was mentioned, this method was to smile indulgently and remark that such questions belonged to "*Philosophie*." It was quite a recognised way of discussing such awkward questions to call them by another name; so that "*Mathematik*" would ultimately seem to denote a branch of science or art which had no principles that needed serious discussion. Of course, the assumption that philosophy, which presumably included logic, and mathematics were mutually exclusive domains implies that there is no common domain, and a certain German tactlessly asserted to a friend of mine who has done much good work on the principles of mathematics that "a philosopher-mathematician is a contradiction in terms." Of course the only possible reply was: "Very well then, you are sitting next to a contradiction in terms."

Now, it appeared to me about nine years ago, and still appears to me, that the best thing to do in this case was to bring as clearly and frankly as I could the work of those of us which is published in periodicals dealing with philosophical questions as well as logical ones—such as *Mind*, the *Monist*, and the *Proceedings of the Aristotelian Society*—before the eyes of mathematicians by profession for the sake of the light which such logical investigations throw on the principles of mathematics. This, then, is the motive of much of my particular work in the organisation of scientific literature. But we can and ought to take a wider view.

Let us consider the relation of the collection and critical examination of the published researches of others to truth and our own search for it. In most cases, our research will not have as its object the statement of the results and opinions of

people in the past. This is a matter that is a part of the work of historians of science. What most of us want ultimately to find out is what propositions are true, not what are the names of the people who have thought such-and-such propositions to be true, and when they thought so. To attain this object, one way is to examine the work of others for (1) indications as to the propositions already established and how they do in fact follow from axioms, (2) indications as to those to be examined, and (3) suggestions for the best way of examining them. If, then, literature is not put in a readily comprehensible and accessible form, the work of investigators is made difficult and sometimes even impossible. A depression of spirits is produced, in many cases, by a knowledge that there is an enormous mass of literature on almost every subject in, say, mathematics, and that in this mass there may be something that has previously been written that throws light on some point that happens to interest us, but that something is quite hidden. A corresponding rise of spirits is produced by evidence that we need not work through this mass. The resulting attitude of mind is not unlike that which, possibly for different reasons, animated Descartes at the beginning of his career. It appeared to him that he might escape from the burden of learning bequeathed to him by his predecessors, and, under the conviction that nothing which is true can be hidden from a calm and thoughtful inquiring mind, shut himself away from the world in a warm room and constructed the whole universe of thought and things out of his own thoughts. We know that much of what he did is of value for all time, but none the less we find that he often unconsciously made, to the ideas of his despised predecessors, a step backwards from which a knowledge of the history of science might have saved him. Even at the present time we see many physicists, in particular, who have such a contempt for what they call "metaphysics" that they abstain from finding out anything about it, and quite unconsciously fall back into a way of thinking about a homogeneous and continuous æther that is not unlike parts of Descartes' doctrine—or indeed Parmenides' or Aristotle's.

This aim of lightening the burden of learning for seekers after truth is undertaken by those who busy themselves with complete summaries of researches in science, and also by those who write in encyclopædias and really up-to-date text-books from modern points of view. Here, again, Germany has been for years past in advance of other nations, but it has been fairly closely followed by France during the last sixteen years. In Italy and America also there have been really serious efforts to produce scientific books that appeal to the more advanced students of science. In Great Britain, with a few notable exceptions, the output of the printing presses which may be classed as "scientific" is devoted to elementary education; and in such books three parts seem adapted from other books and the remainder seems usually the result of a very inferior type of originality.

There is one other point in which Great Britain is notably inferior to the chief nations of Europe. It is in the publication of good and needed editions of collected works. France is continually publishing carefully edited and beautifully produced editions of the works of her great men, and, for instance, the last edition of the works of Descartes is a thoroughly competently-done national edition. Italy has given us a very fine national edition of the works of Galileo. In Germany fine editions of collected works are almost as common even as fairly great men. In Great Britain there have been quite a number of editions of the works of modern men of science produced, for the most part, by the University of Cambridge, just as there have been in other countries of Europe on a far more

imposing scale. But in the case of Newton, who is certainly as important for Britain as Descartes is for France or Galileo for Italy or Leibniz for Germany, no attempt at a complete edition of his works has been made since the time of Horsley. And many things have come to light which have enabled us to fix Newton's place in the history of science much more accurately than we have been able to before. Further, Newton's manuscripts, some of them containing work of very great importance, as I happen to know from actual examination, are still buried in various public and private libraries.

Even if, with the help of France, Russia, Italy, and our other Allies, we were to succeed in thoroughly eradicating the political pretensions of Germany and its Allies, we would not have advanced a single step towards clearing ourselves of the disgrace we have earned by our treatment of science. And it will be on such charges that we shall be judged and condemned by the future unless we change our point of view and prize knowledge more than power. Wastage of human life is reprehensible on merely humanitarian grounds, except possibly where there is only a choice between life and honour. But we are wasting something far more important than life. We are wasting one of the few things which make the lives of those of us who are not mere brutes worth living at all: the pursuit and contemplation of knowledge. Such things as comfort and security are means to ends of which this is one, and it is an unpardonable sin to waste means of promoting the growth of our knowledge or to put obstacles in the way of this growth. It may be necessary to deprive Germany of some of the necessities of life in order to obtain results of military or political importance, just as the Germans found it necessary to starve Paris in 1870, but when, no doubt with excellent intentions, we deprive one of the few peoples of the world who provide certain unselfish scientific work of the means of carrying on this work, we are, I think, bound to take up this work ourselves for the sake of certain ideals which should be even dearer to us than increase of power, trade, or even comfort.

We ought to realise what we are leaving undone. This realisation must come to us by British means, such as this journal. It would be very humiliating to our national pride if we were forced to see ourselves dropping behind, say, Bulgaria or Turkey in some paths of knowledge.

REVIEWS

GENERAL

Discovery; or The Spirit and Service of Science. By R. A. GREGORY. [Pp. viii + 340, with 8 Plates.] (London: Macmillan & Co., 1916. Price 5s. net.)

IT is seldom that a book becomes a classic between the date of issue and the date of review. This has happened with Prof. Gregory's really fine book on Discovery. Every one interested in science appears to have studied it already, and a long review has therefore now become unnecessary; and we must express only our apologies to the author for not having reviewed it before. Of all the histories every written, those of scientific discoveries are and should be both the most useful and the most interesting. They deal with the great labours of the greatest minds—which generally achieve successes that are of perennial benefit to the whole of humanity without causing sorrow to any one. How different from the sordid struggles between nations, the mean machinations of potentates, and the selfish pretences of politicians, which, forced into the minds of the young under the name of history and "the humanities," create in them the false ideals that cause constant and barren strife and that lead to devastating wars and the misery of millions. Prof. Gregory's book is a brief survey of this true history of mankind—and it is extraordinary that more works of the kind should not have been brought out before. But of course it is only a survey, because a full record would occupy many volumes. The title of the book and the name of the author will by themselves suffice for a review.

R. R.

Fundamental Sources of Efficiency. By FLETCHER DURELL, Ph.D. [Pp. 368.] (Philadelphia and London: J. B. Lippincott Company, 1914. Price 10s. 6d. net.)

WE are all busily trying to work as efficiently as possible. Writings like Münsterburg's *Industrial Efficiency* discuss the most effective methods of securing specific aims in business and trade. In war, in teaching, in commerce, in industry, in social amelioration, in thinking itself, methods are overhauled, tested, criticised, and discussed. In the work under review, Dr. Fletcher Durell endeavours to outline a complete Science, or Philosophy, of efficiency.

The word "sources" tends to prepare a surprise for the reader. He is ready for the "few elemental principles," but hardly, perhaps, for the very numerous "forms and sources of efficiency" that the work is "an attempt to analyse" into their elements. The book is an extensive classification of the ways in which results are achieved or by which they arrive, and an attempt to penetrate through them to the essential factors of efficiency. The survey is very ample, for it includes in its infinite variety the quick handling of goods by packing them in a barrel and the concept as examples of the advantageous use of "Groups"; an idea in a book and its various uses by a reader as an illustration of "The Unit and its

Multiplier"; the telegram, the algebraical X, the metaphor and the Braille alphabet to indicate that "Symbolism" is "the tongue of efficiency, and in some respects also its brain"; memory as a "groupish and efficiental" organisation; and the superior efficiency of the Christian method to retaliation.

An efficient process is defined as "one in which the available results exceed the expenditure." Whatever may be thought of this definition, with its attempt to translate ALL efficiency into quantitative terms, it is not to be understood as confined to purely economic or material advantage. The reference to "elemental chemical ideas," such as the "carbon molecule," as concepts for dealing "efficiently with phenomena" is in itself enough to remove any such misunderstanding. Mechanical and industrial efficiencies are examined and analysed; but the mental receives its appreciation as the primary source, and the educational, the social, the ethical, and even the religious efficiencies are discussed. In the appendix on "The Categories and a General Philosophy of Life," the principles of efficiency are traced in the fundamental notions of the mind.

Re-use is emphasised as a primitive and, "in a sense, fundamental source of efficiency"—memory re-uses perceptions, an idea or process may be applied many times in or out of its original connection. The "Group" combines economy and re-use. A slide rule is a "multiplicative group system." We utilise "Externality" as an efficient principle when we use external things or entities. These principles and many others, such as directive agencies, rhythm, and the significance of limitations, are examined, classified, and discussed.

The chapter on "Dialectic" lays stress on the advantages resulting from unexpected developments. The depredations of the Colorado Beetle led to improved methods of potato culture; Perkin accidentally produced mauve when studying quinine. This chapter may confirm the general impression of some readers that, like logic, the general science of efficiency is interesting but sterile. We learn, on this rendering, how some efficiencies have come, but not how to deal with any particular situation. But a study of methods, if adequate, should lead to better understanding and hence to improved practice.

Exercises are appended to the various chapters to add to the value of the work if used as a textbook.

JOSHUA C. GREGORY.

The Birth-Time of the World and other Scientific Essays. By J. JOLY, M.A., Sc.D., F.R.S. [Pp. xvi + 307, with 28 plates and 15 illustrations.] (London: T. Fisher Unwin, Ltd. 10s. 6d. net.)

THIS volume contains twelve essays, being for the greater part studies which have been contributed to scientific reviews or delivered as popular lectures during recent years. Some are expositions of views the scientific basis of which may be regarded as established; others are attempts to solve problems which cannot be approached by direct observation. In all of them we find that charm of style and lucidity of expression which is so noted a characteristic of Dr. Joly's writings and lectures.

The essay which gives its name to the book is a summary of results obtained by various methods concerning the "age of the Earth," *i.e.*, the lapse of time from that epoch at which sediment began to deposit on the underlying floor of igneous and metamorphic rocks and the vapours began to condense from the hot and heavy atmosphere. The various methods of investigation referred to are those connected with the mass of the sediments, with the amount of sodium in the ocean, and with

the transformation of radioactive elements contained in various minerals which gives rise to a continual evolution of helium. The first two methods yield results which agree very well with a period of the order of 100 millions of years, the balance of probability being that this is somewhat too great. The measurements based on the evolution of radioactive products yield results which in their superior limits exceed those derived above from nine to fourteen times. However, Dr. Joly points out the weakness of certain assumptions employed in the third method of investigation, and shows that we have grave reason to question the existence throughout the ages of a uniform rate of evolution of radioactive products.

In an essay on the "Abundance of Life," the author inquires into the underlying causes of the advance of life upon the earth, the "aggressiveness of vitality"—to use his picturesque phrase. He finds them to be, physically, more fundamental than evolution, and to be dynamical rather than naturally selective. In the general dynamic conditions attending the motions of inanimate matter, we find ever present that dissipation of available energy—at one time mainly gravitational potential energy—into heat, whose course is summarised in the Second Law of Thermodynamics. In the general dynamic conditions attending the behaviour of animate bodies, such as the absorption of energy by a leaf, the division of the leaf so as to open up new channels of supply, the flattening of its form, the orientation of its greatest surface towards the source of energy, we find an "attitude" towards available energy which causes an accelerated transfer and retards dissipation. It is this active attitude, rather than the passive one implied in natural selection, which determines progress.

A paper entitled "Other Minds than Ours" discusses the possibility that the well-known markings on the surface of Mars—usually called the "Martian Canals"—may indicate the existence of intelligent beings in that planet. Prof. Lowell, who has just died, extended the early observations of Schiaparelli, and has been a most uncompromising advocate of the view that Schiaparelli's "canali"—mistranslated "canals"—were the work of a highly advanced civilisation seeking to store its fast-dwindling water supplies by draining the water obtained from melting polar snows into huge oases, and transmitting it to its populous centres, presumably studded along the banks of these huge artificial rivers and living on the rich alluvial plains created by such an immense irrigatory system. Dr. Joly, while not combating this view, points out that in so far as it has been accepted, the acceptance is due to the exclusion of other less probable alternatives rather than to any strong positive evidence in its favour, and he puts forward a new hypothesis which certainly cannot be put aside as an unlikely alternative to Lowell's belief. He remarks that many of the small asteroidal bodies which populate that part of the solar system lying between the planets Mars and Jupiter cut across the orbit of Mars at certain times. It is not improbable that some of them are thus drawn towards Mars and become satellites of it—in fact this is probably the history of Phobos and Deimos. Some of these, especially if their orbital motion round Mars happened to be opposite to the planet's axial rotation, would be gradually drawn in and finally crash down on the surface. Before doing so, however, such a satellite would travel many times round the planet at such a close distance that its gravitational attraction would probably rend the crust along two lines parallel to the projection of its path on the surface and lying to right and left of it. This would account for the well-known double character of these surface markings. Their form and curvature would depend on the velocity of the satellite and the axial rotation of Mars, and it

appears that Lowell's maps agree remarkably well with the course indicated by Prof. Joly's hypothesis.

Limitations of space prevent any but the briefest reference to the other essays. Several are devoted to radioactive phenomena, others to geological processes involved in the formation of mountains; while the concluding essay deals with a speculation concerning a pre-material universe, and discusses certain objections to the Kant-Laplace hypothesis of the existence of a primitive condition of wide material diffusion, and necessarily too of material instability, infinitely prolonged.

Every essay is a model of clear and popular exposition, and delights the mind of the reader by its fertility of ideas and its charm of style.

J. R.

MATHEMATICS

William Oughtred: a great Seventeenth-century Teacher of Mathematics. By F. CAJORI, Ph.D. [Pp. vi + 100.] (Chicago and London: Open Court Publishing Co., 1916. Price 4s. net.)

OUGHTRED played a large part in the teaching of mathematics in the seventeenth century in England, and in view of the increasing interest in the history of that subject this monograph is happily devised. It gives a short, clear, and readable account of the man and his works.

The son of the writing master at Eton, Oughtred was placed on the foundation there, went thence to King's College, Cambridge, as a scholar, and succeeded in due course, while yet an undergraduate, to a fellowship. He said that above the subjects expected of him at the University he worked privately at mathematics, and in fact he then composed two monographs on dialling. The writer of this review thinks it likely that Oughtred's mathematical work while a student was done largely under the influence of the well-known Edward Wright of Caius College. It should be also noted that at this time Briggs of St. John's College was teaching geometry, so that though mathematics may not have been part of the ordinary academic course, the statement that they were then neglected in the University needs qualification.

Oughtred vacated his fellowship three years after his M.A. degree, took orders, and spent the rest of his life in parish work—the last fifty years at Albury, near Guildford. Here he gave instruction on mathematics freely to all who came to him, and among his pupils were Ward, Wallis, and Wren. Cajori mentions a portrait of Oughtred painted in 1646: it was in fact drawn in 1644, and there are engravings of it by Hollar and Faithorne: it would have been interesting had one of these been reproduced in this book.

It was at his rectory that Oughtred composed his *Clavis Mathematica* and *Trigonometria*, which became standard textbooks on arithmetic, algebra, and trigonometry, and contain an excellent exposition of these subjects as then known. These works are notable for the free use of symbols, and this marks an important advance in thought and practice: his trigonometry contains some mathematical tables. He also wrote on navigation under the title *Circles of Proportion*, and on sundials and their construction. He is chiefly known for the textbooks he wrote, but he should also be famous for his invention of the slide-rule, which provides a mechanical way of obtaining logarithmic results.

In this booklet Oughtred's writings and symbolism are fully described, his originality vindicated, and his influence on the development of mathematics discussed. In the last chapter our author has brought together from scattered

sources various statements by Oughtred as to how mathematics should be taught —an interesting conclusion to the work.

W. W. ROUSE BALL.

Combinatory Analysis. By MAJOR PERCY A. MACMAHON, F.R.S., D.Sc., LL.D. (late Royal Artillery), of St. John's College, Cambridge. Vol. II. [Pp. xix + 340.] (Cambridge: at the University Press, 1916. Price 18*s*. net.)

THE issue of the second volume of Major MacMahon's "Combinatory Analysis" follows close upon that of the first.[1] It is welcome for many reasons. We are glad to have this great work complete; for it has been an unamiable weakness of English mathematicians often to interrupt their tasks for so long that the first parts of their works have become tired of waiting for the companion volume, and settled down into a state of unblessed singleness upon our shelves. This volume redeems, within a year or so, the pledge made by its predecessor, and fully upholds the high expectations based upon the part first published. In a certain sense the two volumes are so much one book that they might have been bound together within a single cover; thus treated they would have formed a book of some 650 pages—a size exceeded by some treatises of similar format published by the Cambridge University Press. One disadvantage of this course would, however, have been that we should not have had the brief historical note in the Introduction of the present volume upon Waring's Formula for the sum of the powers of the root of an equation, in which the writer restores to its true author a formula which, on German authority, he had in the first volume attributed to Albert Girard.

In the contents of Vol. II. Major MacMahon has relied even more than in the earlier part upon his own work. These problems are often so highly specialised that it is impossible to deal with them as fully as they deserve in these pages; their solution depends upon analysis which would be intelligible only if presented at very great length. The volume consists of five new sections dealing with partitions of numbers, partition in two dimensions, and symmetric functions of several systems of quantities.

In the account given of the partition of numbers Euler's theory naturally claims the first place, but much of the work of Jacobi, Gauss, Cayley, Sylvester, and Glaisher is presented to us, and enriched by the author's own work. The mathematician whose turn of mind inclines him to studies in algebra is always attracted to these intractable series, which crop up so unexpectedly in the theory of partitions and in elliptic functions. Such a series is Jacobi's series, celebrated by our author as "justly considered to be one of the most remarkable in the whole range of pure mathematics."

$$(1-x)^3(1-x^2)^3(1-x^3)^3 \ldots = 1 - 3x + 5x^3 - 7x^6 + \cdot + (-1)\, x^{\frac{1}{2}i(i+1)} + \ldots$$

It is, perhaps, the part which series of this kind play in connection with other branches of mathematics that in the present state of our knowledge justifies their study, if any justification were needed; they have no special theory of their own apart from the applications in which they play so notable a part. Their mystery has been deepened by a remarkable group of identities (presented in this book perhaps for the first time) discovered by the young Cambridge mathematician, Mr. Ramanujan. These identities have applications in the partitions of numbers, and resemble in form some of Euler's results; but they are curious because,

[1] See SCIENCE PROGRESS, April 1916.

although they have been verified to a large number of terms, they have not been established generally. They thus resemble Fermat's last theorem, which has withstood all attempts to solve it for two hundred years; how long Ramanujan's identities will remain unproved it were idle to speculate upon; but there is one difference between the two cases besides that of date. We know that Ramanujan cannot at present prove his identities, but whether Fermat had a method of solving his theorem will probably never be known. It will be enough, perhaps, to quote here the first of the identities. It is written:

$$1+\frac{x}{1-x}+\frac{x^4}{(1-x)(1-x^2)}+\ldots+\frac{x^{s^2}}{(1-x)(1-x^2)\ldots(1-x^s)}+\ldots$$
$$=(1-x)^{-1}(1-x^6)^{-1}(1-x^{11})^{-1}\ldots\ldots(1-x^4)^{-1}(1-x^9)^{-1}(1-x^{14})^{-1}\ldots\ldots$$

the indices of x in the last product increasing by 5 in the factors. The identity expresses that the partitions whose parts are limited to one of the two forms $5m+1$, $5m+4$ are equi-numerous with those which involve neither repetitions nor sequences. This section of the book abounds with deductions from the theory of partitions of strange identities; thus the author establishes in this way the equality of the two series:

$$\sum_{m=1}^{m=\infty}\frac{x^m(1+4x^m+x^{2m})}{(1-x^m)^4}\ (m\ \text{odd}).$$

Hitherto a partition of n has been defined as a collection of positive numbers whose sum is n, but in the seventh section the author restricts the collection by prescribing an order amongst the parts, namely the descending (or rather non-ascending) order of magnitude. The generating function of Euler—

$$(1-x)^{-1}(1-x^2)^{-1}\ldots(1-x^s)^{-1}$$

gives as coefficient of x^n the number of partitions of x^n when the function is expanded in powers of x. Major MacMahon substitutes for this function a crude generating function—

$$(1-\lambda_1 x)^{-1}(1-\lambda_2 x/\lambda_1)^{-1}(1-\lambda_3 x/\lambda_2)^{-1}\ldots\ldots(1-\lambda_s x/\lambda_{s-1})^{-1}$$

in the expansion of which all negative powers of λ's are omitted, and then the λ's are all put equal to unity; the coefficient of x^n gives the number of partitions in which the parts have a descending order of magnitude. Beyond this an ultra-crude generating function is introduced, but thither a review can scarcely follow. Such partitions are regarded as one-dimensional.

In the following sections the author considers two-dimensional partitions of a number the parts of which are arranged in descending order whether we pass along a row from the left in the direction of Ox or along a column in the direction of Oy. The complete solution of the problem is given, having yielded to the lattice function and its derivatives which the author invented for its subjugations. The question of partitions in solido is broached, but the complete solution has not been reached; we are told that the subject bristles with difficulties. Partitions in hyper-space have not yet been introduced.

The book contains ideas which are new, and which may well prove the starting-point of future investigations. In this particular branch of analysis the English mind has proved itself skilful and adventurous before. But the subject is one of very great difficulty, and it is not given to every one to bend the bow of Achilles.

The University Press of Cambridge has a reputation for printing which it would be idle to deny. In mathematical printing, however, the printer has

always to co-operate with the author, and it is very rarely that authors, in writing their manuscripts, reflect upon the effect that it will have when it is set up by the compositor. It would seem as if there was room for a third party who could take the complicated formula as it leaves the writer's pen, and without altering its meaning, yet by help of solidus or negative indices arrange the masses of symbols into a more attractive and artistic form than that in which they generally issue from the press.

C.

Ruler and Compasses. By HILDA P. HUDSON, M.A., Sc.D. [Pp. vi + 144, with Diagrams.] (London: Longmans, Green & Co., 1916. Price 6*s*. net.)

IT is well known that Euclid put at the beginning of his *Elements* the postulates that a straight line might be drawn between any two points, that such a straight line might be produced to any length in the same straight line, and that a circle might be described from any centre and at any distance from that centre; and that all the constructions used in the first six Books are made with these three operations only. The many attempts to solve the great problems of the duplication of a cube, trisection of an angle, and quadrature of a circle by what were thus called "Euclidean methods" failed, and such attempts were analytically proved in more modern times to be necessarily vain. On the other hand, certain constructions can be carried out by Euclidean methods which were not given by Euclid. It is the purpose of this excellent little book to investigate, both analytically and geometrically, how far Euclidean constructions can carry us, although no attempt is made to give an account of work on transcendental numbers. It is well remarked (p. 3) that "the ancient or classical geometry lends itself curiously little to any general treatment; and even modern geometry lacks a notation or calculus by which to examine its own powers and limitations"; thus it has to be by using co-ordinate methods that Miss Hudson attacks the general question as to the problems which can be solved by ruler and compass (Chap. II.). The second chapter also contains an answer to the question as to what regular polygons are within our powers of construction, and Richmond's (1909) construction of one of seventeen sides is given on p. 34. Chaps. III. and IV. are devoted to ruler constructions and ruler and compass constructions respectively; Chap. V. is concerned with a classification of methods; and Chap. VI. is devoted to a comparison of methods. The last two chapters (VII. and VIII.) are concerned with the solution of Euclidean problems by drawing only one circle and the requisite number of straight lines (Poncelet and Steiner), or else by drawing circles only (Mascheroni and Adler).

This is an exceedingly competently written book, and there are only a few little criticisms that might be offered. In the first place, the way of printing analytical formulæ does not strike one as wholly pleasant, though this is probably due to the fact that one is used to small italic letters for the purpose. In the second place, it is surely rather misleading to the student continually to speak as if the geometry of the ancient Greeks were intimately concerned with drawing instruments and the material on which figures were drawn (cf. pp. 1, 6, 70): although the solution of the problem as to why Euclid stopped at a postulate that lengths may be carried about from place to place, when he had admitted that one end of a straight line may be carried about if the other end is fixed, may, perhaps, be answered historically by such considerations. This book is a very valuable addition to "Longmans' Modern Mathematical Series."

PHILIP E. B. JOURDAIN.

ASTRONOMY

David Gill, Man and Astronomer: Memoirs of Sir David Gill, K.C.B., H.M. Astronomer (1879-1907) at the Cape of Good Hope. Collected and arranged by GEORGE FORBES, F.R.S. [Pp. xii + 418, with 14 portraits and illustrations.] (London: John Murray, 1916. Price 12s. net.)

SHORTLY before his death, Sir David Gill was able to see the publication of his epoch-making work, *A History and Description of the Cape Observatory*, in which he related the growth of the Cape Observatory from its smallest beginnings to its present position as the formost observatory in the Southern Hemisphere. That volume contained the best possible account of his own scientific work, and the task of his biographer has been materially lightened by the fact that it has, accordingly, not been necessary for him to refer to Gill's scientific work except in so far as it throws light upon his character. The result is a volume which will be read by the general reader with almost as much interest as by the student of astronomy. Gill was a man who in his life had a singular talent for attracting friendship, and Prof. Forbes, one of his life-long friends, has succeeded in giving us such a human picture that as we read we feel the charm of the man; the reader, on reaching the end of the book, will almost feel that he knew Gill. His simplicity of character, his untiring enthusiasm, his force of purpose, his boundless energy for work, his happy married life, his friendships, his sympathy and appreciation for the efforts of others, and also his limitations are all portrayed. Gill was not only a great astronomer, he was a great man.

The way in which Gill came to devote his life to astronomy is typical of the man. His father had a remunerative business as a clockmaker in Aberdeen; in obedience to his father's wishes, but against his own inclinations, which already leaned towards astronomy, he learnt his father's trade and settled down as manager of his father's business. His interest in astronomy had been first stimulated by Clerk Maxwell, under whom he studied in Aberdeen, and who, though not a brilliant lecturer, had the true teacher's gift, the power of stimulating the enthusiasm of his students. Gill in those days commenced observing with a small telescope, and developed a scheme for supplying the town of Aberdeen with the correct time. When later Lord Lindsay founded his observatory at Dun Echt and asked Gill whether he would become its director, Gill at very great pecuniary sacrifice accepted the offer gladly, his decision being backed up by his wife. The years which he had spent as a watchmaker were not really wasted, for he had acquired a mechanical knowledge which later became invaluable to him when at the Cape it was sometimes necessary to make alterations or modifications to the delicate mechanism of some of his instruments, alterations which could not have been trusted to any local mechanic, and which would otherwise have necessitated the parts being sent for repair to Europe with a resulting delay of several months.

How Gill within the course of a few years rapidly became one of the foremost astronomers in the world, and how he came to be appointed Her Majesty's Astronomer at the Cape, must be read in this book. The Cape Observatory at that time was in a deplorable condition; it was in a forlorn spot, the grounds were neglected, only a rough muddy road led up to it, and the instrumental equipment was very poor. It was a splendid opportunity for Gill, and one which he was not slow to seize. Gradually the observatory was transformed: the site was enclosed, trees planted, a good road was made and houses built for the assistants; from its former desolate condition it was transformed into a pleasant place of labour, and became a happy colony with Gill as its genial chief, taking a personal

interest in the doings of all his assistants and in their families. On his return from a visit to Europe all in turn would come in to see him down to the youngest. The instrumental equipment was increased from a small transit-circle, a 7-inch equatorial and an old photoheliograph, by the addition of two heliometers (4-inch and 7-inch), a 6-inch refractor, the astrographic telescope, the Victoria spectroscopic telescope, the reversible transit-circle, a zenith telescope, and a 3-foot altazimuth. Much care and thought was bestowed by Gill upon the design and housing of these instruments, and his reversible transit with its wonderfully stable meridian marks is probably the finest transit-circle in the world. All of them, except one, were obtained by Gill from the guardians of the public purse by his persuasive enthusiasm, his tireless energy, and his refusal to accept "no" as an answer. The other telescope, the fine Victoria telescope, was a gift from a friend and admirer. His method of dealing with Government officials is well illustrated by the following account of the manner in which he obtained his heliometer:

"On arriving at the Admiralty one morning he found that the question had passed from the Hydrographic Department and that before reaching the Treasury would pass through many hands and might be settled in about three weeks. After careful inquiries on general procedure he traced the documents and cheerily interviewed the official in whose hands they were and explained the importance of the instrument and its uses. Thanked for his kindness in calling, he was told the request would receive early attention and would probably be out of that room in a week or so; but Gill pointed out that it was essential that it should be through all departments of the Admiralty and sanctioned by the Treasury to enable him to announce to the Astronomical Society Meeting that evening that the Government had sanctioned the purchase of the instrument. After suggesting that the official could write his brief minute at once as well as a week later his views prevailed, the minute was written, and he was entrusted with the documents for conveyance to the officer who was to deal with them next. The process was repeated and he hied him to the Treasury where he added to his former plea for haste the example of the businesslike way the Admiralty had dealt with the matter. The Treasury people humoured him, but the last man urged the utter impossibility of final Treasury sanction as the Financial Secretary was not in his office. Inquiry as to his whereabouts proved him to be at the House of Commons; so Gill hastened there and after explanations the Secretary agreed to the provision of the heliometer, and a very happy Gill drove at once to the R.A.S. and made his announcement."

But the transformation which he brought about in the Cape Observatory is but a small part of his work. The geodetic survey of South Africa is due to his initiative, and its success to the manner in which he pulled it through on several occasions when very near failure. He took the lead in various international projects such as the organisation of the various observatories of the world for the astrographic chart and catalogue. Every one consulted him and looked to him for advice. Even more so was this the case after his retirement from the Cape when he settled down in London. Nor must we forget the part which he took in stimulating the enthusiasm of young astronomers; several went out to the Cape and worked and lived with him for a time, forming friendships which lasted through life; some of the credit for their subsequent work must be given to Gill.

But his capacity lay not only in his powers of organisation and of directing and stimulating others. He was himself pre-eminently a born observer; he was never happier than when observing, and in spells of fine weather would stay up night after night observing with the heliometer into the early hours of the morning. As Lady Gill wrote, "When David comes in after a night's work with his old heliometer he is just daft, laughing and joking. He was just the same with the

telescope in his father's garden when we were first married. So it was at Dun Echt, and exactly the same at Ascension. And so it will be as long as he can look through a telescope." The excellence of the results which he obtained with the heliometer, admittedly the most difficult of all astronomical instruments to use, testifies to his skill. The value which he derived for the solar parallax is accepted as the correct value to-day; his determination of the Moon's mass is the best yet made. To this actual skill in observing was added an inimitable skill in discussing observations, in finding out all possible sources of error and in arranging for their removal, and it was largely due to the excellence of his original observations that it was possible in some instances to detect unsuspected sources of error.

Prof. Forbes has given us a delightful picture of the personal side of Gill's life, illustrated by a large store of good anecdotes. One of the most prominent of his traits was his love for good music; on one occasion "Santley was singing 'Duncan Grey' at a concert in Cape Town, and Sir David had his seat on a platform. Each verse excited him more than the last, and, oblivious of all but the song, at the close of each verse he pushed back his chair a little to catch the sound better, until, to the horror of his wife, who sat in the body of the hall, he was within a few inches of the stair leading down from the platform. Had there been one more verse he must have turned a somersault down the steps, and all his friends were relieved when the song ended without a catastrophe." He was also a keen art critic. He loved outdoor sport, and was a first-class shot, and in his younger days won many prizes and medals in competitions; the book contains an excellent account of his first deer-stalk, when he succeeded in killing the deer by an almost impossible shot which his gillie had advised him not to attempt. His home life was of the happiest, and between him and his wife there existed the most perfect sympathy and understanding. The book must be read to appreciate all that his wife was to him.

Astronomers have to thank Prof. Forbes for writing such an excellent account of the life of one who was pre-eminent in astronomy; although it is the oldest of the sciences, astronomy is now perhaps somewhat of a bypath in science, and the general reader may be inclined to think that the life of an astronomer can be of but little interest to him. If he will start to read this book his opinion will change before he has proceeded very far, and he will realise why, when Gill died, to his astronomical friends their regret "for the great loss to astronomy was overshadowed by a human sorrow in losing a friend of magnetic personality and simple-hearted character."

H. S. J.

Dante and the Early Astronomers. By M. A. ORR (Mrs. John Evershed, Kodaikanal). [Pp. xvi + 508, with 9 plates and 53 figures.] (London: Gall & Inglis, 1914. Price 15*s*. net.)

THERE was probably no study which attracted Dante more than that of astronomy; even in his earlier days, before learning had become to him the passion which it was later, astronomy had a special fascination for him and appealed to him in many ways. As Mrs. Evershed says, "the beauty of the skies stirred his imagination, their suggested symbolism touched his religious sense; the harmony of the celestial movements and the accuracy with which they can be foretold delighted his instinct for order and precision." The astronomical references in his writings are very numerous, and indicate that Dante had read all the best works on astronomy available to him, and was acquainted with all the theories then held to explain the motions of the heavenly bodies. To the

average reader, however, many of these references may seem involved and difficult to understand; this is partly because certain facts about the solar system and the stars which are now common knowledge were unknown in Dante's time, and to understand many of his references it is necessary to view them from his view-point; again, the average reader of to-day is quite unfamiliar with the face of the sky and with the motions of the sun, moon, and planets in the different constellations, so that Dante's many picturesque methods of indicating a season or length of time convey absolutely no meaning.

The purpose of Mrs. Evershed's book is to remove these difficulties and to enable Dante's astronomical allusions to be understood. The book is divided into two parts; in the first is given an account of the elementary facts of astronomy which will help the non-astronomical reader to understand what follows; a sketch is then given of the various attempts made to explain these movements from the earliest times until the time of Dante. This sketch, which occupies about 200 pages, is admirably and concisely done, and forms a valuable summary of the early history of astronomy; for the benefit of the general reader all unnecessary technicalities are avoided. In the second part is given an account of the popular astronomy in Italy in Dante's time and of the books on astronomy which he probably used; the thoroughness of his knowledge is illustrated. This is followed by a careful discussion of Dante's numerous references and by a critical examination of some of the difficult passages whose interpretation is more or less uncertain. The aim throughout has been, however, not so much to interpret the references for the reader as to provide him with sufficient information to enable him intelligently to interpret them for himself: in this aim Mrs. Evershed has been eminently successful, and we can strongly recommend her charming book to all students and readers of Dante. We may add that the illustrations throughout are excellent and have been carefully thought out.

H. S. J.

PHYSICS

The Emission of Electricity from Hot Bodies. By PROF. O. W. RICHARDSON, F.R.S. [Pp. vi + 304, with 35 diagrams. Monographs on Physics Series.] (London: Longmans, Green & Co., 1916. Price 9s. net.)

IT is but seldom that a branch of science is so completely dominated by one man as is the subject of this book by Prof. Richardson. Other workers have played an honourable part, notably H. A. Wilson, Prof. Horton, and Irvine Langmuir; but our knowledge of thermionics would be much scantier and very much less precise than it is at present without the guidance that his work has provided. The explanation is not far to seek, for the subject makes demands of no ordinary character on the mathematical and physical knowledge of the investigator and on his skill and patience in overcoming experimental difficulties of unusual severity. The pioneer work has been done; but many points still need elucidation, and one of the most pleasing features of the book is the care with which the author indicates, again and again, questions which should repay investigation. It is a mine of suggestions for any one seeking an interesting line of research.

With the exception of the electrical properties of flames and certain technical applications, the whole of the phenomena embraced by the title Thermionics are dealt with, and very little, if any, work of importance has been omitted. From the general point of view the interest still centres very largely on the rival theories of the emission of the electrons; as to whether it is to be attributed to the purely thermal increase in their kinetic energy or to chemical

action. This question forms a motif underlying the greater part of the book, but the matter is specifically discussed in a section which is a model of minute scientific analysis. Certainly in the case of tungsten, which, owing to its high melting-point, is specially suitable for thermionic investigations, Prof. Richardson has shown that no chemical action can occur; but just lately (*Phys. Rev.*, November 1916) Stoelkle has published an account of very careful experiments with molybdenum which show that Richardson's statement that "considerable changes in the amount and nature of the gases present in the tube do not change the electron emission" does not apply to that metal, though under suitable conditions it seems that a purely thermal emission may take place. The commonly accepted equation for the variation of the saturation current with temperature, $i = AT^{\frac{1}{2}} \epsilon^{-\frac{b}{T}}$, can be deduced equally well on each theory, and so far experiment cannot discriminate between this equation and that obtained by the author (in which $T^{1/2}$ is replaced by T^2) both from the quantum theory, and from thermodynamics on the reasonable assumption that the change of energy which accompanies the departure of each electron from the hot body is a linear function of the temperature. This matter seems to require more comment than it receives. Prof. Richardson says "obviously both formulæ cannot be correct"; but that both are equally well satisfied by experiment does not seem very creditable to the experiments unless the exact effect of the preponderating influence of the exponential term is pointed out.

The bearing of the work on contact potential difference is discussed at length, and while this vexed question is still left open the recent experiments of Lester seem to show that the excess of kinetic energy over the equilibrium value which an electron, inside a metal, has to lose in order to escape with zero velocity, is the same for many elements, from which it would follow that they should show no contact E.M.F. in a good vacuum. In the chapter on the "Energetics of Electron Emission," a full account is given of the experiments which the author carried out, in collaboration with F. C. Brown, on the distribution of velocities among the electrons—the first direct experimental verification of Maxwell's distribution law. This chapter also includes an account of the latent thermal effects. The measurements of the loss of energy which accompanies the escape of electrons from metals is in good agreement with that indicated by the saturation current equation; but the experimental difficulties involved in the determination of the gain of energy when an electron enters a metal have not yet been overcome satisfactorily and only in the case of platinum is reasonable agreement obtained.

The latter part of the book is devoted to the question of the emission of positive ions from hot metals and of ions generally from heated salts and as a result of chemical action. The thermodynamical and mathematical theory is not obtrusive and can easily be avoided by those not specially interested in it. The book can be recommended very cordially, not merely as an account of thermionics, but for the instruction it gives in an experimental technique which requires a knowledge of almost all branches of physics for its successful development.

D. Orson Wood.

A Treatise on the Theory of Alternating Currents. By Alexander Russell, M.A., D.Sc., M.I.E.E. Vol. II. [Pp. xiii + 566, with 239 figures. Second edition.] (Cambridge: at the University Press, 1916. Price 15s. net.)

The second volume of this well-known textbook is devoted to the theory of alternating current machines and to questions pertaining thereto. It is intended for

students and engineers who possess a considerable knowledge of mathematics, and it will not be appreciated at all by those who prefer this aspect of the subject to be reduced to a minimum. However, mathematical equations usually become rather cumbersome when they are required to take into account the actual conditions which obtain in practice, and, as might be expected, *the* feature of the book is the thoroughly practical spirit in which the theory is given.

It is only ten years since the first edition was published and, in the interval, no very remarkable progress has been made. The sections dealing with armature reaction and the induction motor have been rewritten and considerable changes have been made in the treatment of power transmission. Here Dr. Russell gives the usual solution of the general equation simplified by means of the distortionless wave condition, and then shows how a general solution may be obtained with the help of complex hyperbolic functions. For the convenience of the reader this section is preceded by a chapter on the theory of these functions and a table of their numerical values. The chapter on harmonic analysis is, very largely, a reprint of the paper read by the author before the Physical Society in 1915. It is shown how, by applying Weddle's rule for areas, any desired harmonic can be calculated independently of the others, thus avoiding the accumulation of errors which is apt to occur when the value of each depends on that preceding it.

A book of this kind gains much of its importance from its value as a work of reference, and this is much enhanced by the explanatory list of the symbols employed which is given at the beginning; while another admirable feature is the brief bibliography which concludes each chapter. Altogether a notable work which should find a prominent place on the bookshelf of every electrical engineer.

D. ORSON WOOD.

Atoms. By JEAN PERRIN. Authorised Translation from the Fourth Revised Edition by D. LL. HAMMICK. [Pp. xiv+211, with 16 illustrations.] (London: Constable & Co., 1916. Price 6*s*. net.)

PROF. PERRIN'S book in the original was already recognised to be a masterpiece of exposition, and a contribution of the very first importance to scientific literature. We are therefore justly indebted to the translator for having made it even more accessible than it was to English readers through the present excellent translation. The book is one which should make a very wide appeal. We can give no truer estimate of it than by simply saying that it is one of the very few really indispensable books. The following brief notice of its principal contents will suffice to indicate its scope.

The main feature of the work is the high philosophic level which is maintained throughout. This is especially true of the introduction, which deserves to be read and pondered deeply.

The first chapter deals with molecules and atoms in general, with the laws of chemical combination, the gas laws, Avogadro's hypothesis, the periodic system, the elements of the theory of dilute solutions, and the theory of electrolytic dissociation. These are familiar matters, but the exact and critical way in which they are treated gives them a new significance. In the second chapter we have a more detailed account of gases from the molecular standpoint, especially the calculation of molecular magnitudes. This leads us on naturally to Perrin's own work on the Brownian movement exhibited by emulsion particles, and the fundamental importance of the results thus obtained in relation to the assumptions upon which the kinetic theory of fluids rests. Chaps. III. and IV. are devoted to

a very complete discussion of these investigations, including the verification of the Einstein expressions. This is followed in turn by an account of Smoluchowski's theory of fluctuations, as applied to critical opalescence, and to the cause of the blue colour of the sky (first explained by Lord Rayleigh), and to other phenomena. Perrin then takes up the problem of radiation and the concept of the quantum, particularly in relation to atomic heats and the calculation of molecular quantities. One cannot but be impressed by the essential unity in aim which underlies investigations apparently quite unconnected with one another.

The two concluding chapters deal with the measurement of the electronic charge and allied quantities which enter into the problem of the structure of atoms, their genesis and destruction. There is a valuable table on page 206 giving the results obtained for the Avogadro Number by the various methods employed. The order of agreement is very striking.

The book is one of extreme interest, and should be in the hands of every student of science.

W. C. M. LEWIS.

A Textbook of Elementary Chemistry. By ALEXANDER SMITH, B.Sc. (Edin.), Ph.D. (Munich), Professor and Head of the Department of Chemistry in Columbia University. [Pp. x + 457, with 98 figures and 6 plates. First British edition November 1915.] (London: G. Bell & Sons; New York: The Century Co. Price 5*s*. net.)

A Laboratory Outline of Elementary Chemistry. By the same Author. [Pp. 152, with 22 figures. British Edition October 1915.] (London: G. Bell & Sons; New York: The Century Co. Price 2*s*. net.)

BOTH of these companion elementary works have been written with the clarity and interest which Prof. Smith has infused into his larger textbooks. They are intended primarily for the scholar undergoing training in chemistry as a general ground-work subject without the specific intention of specialising in the subject. The author has consequently allowed himself more scope in the statements of interesting collateral facts than is usually found in elementary textbooks.

Such information, for instance, as the digestion of starch, fats and proteins, fuel values, normal diet and food prices might seem out of place in a primary textbook, but they are here so simply explained and aptly inserted that their inclusion forms one of the admirable features of the present volume. Baker's bread, ventilation, the world's source of energy, cellulose, mortar, colloidal suspensions, magnalium, coal analyses, removal of stains, tin-plate, are a few more headings chosen at random which serve to illustrate the general broadness of treatment.

On the other hand, the textbook does not neglect the purely systematic ground-work of chemistry, and the author is at pains to teach the scholar the elements of correctly interpreting and formulating chemical reactions, and warns him often of the pitfalls which are experienced by the majority of young students. The system of separate paragraphs with heavy type headings is another commendable feature, and it leaves the book free from any of the discouraging feeling presented by unrelieved pages of text. Each chapter closes with a set of well-considered questions, and the plates have been excellently chosen.

The practical volume contains a series of exercises dealing with the subject being treated in the parallel sections of the textbook. Each has the headings, Object, Apparatus and Materials, followed by descriptive directions and sugges-

tions for recording observations. In this volume also much originality is shown in the substances chosen for experimentation and the methods to be employed. In spite of this we are afraid, however, that in most cases a great amount of the success of experimental work of young students is beyond the powers of the most carefully compiled book directions and lies in the hands of the teacher.

In this edition of the textbook two new chapters have been added, one on the Laws of Chemical Combination and the Experimental Determination of Equivalent and Atomic Weights, and the other on the Periodic Classification of the Elements. In the practical book corresponding exercises on the determination of Equivalent and Molecular Weights have been included which are absent in the previous American editions.

C. S. G.

An Introduction to the Physics and Chemistry of Colloids. By EMIL HATSCHEK. [Pp. ix + 105, with 17 illustrations. Second edition.] (London: J. & A. Churchill, 1916. Price 3*s*. net.)

THIS little volume belongs to that very excellent series of "Text-Books of Chemical Research and Engineering" edited by W. P. Dreaper, and without doubt it has proved itself one of the best introductory volumes extant on this difficult and to little known subject.

Starting from the original observations of Graham, the unfolding of the several properties which are associated with the colloidal state is historically traced, and before the third chapter has been reached the reader has become acquainted with the order of the size of colloidal particles and the pioneer work of Bechhold, Zsigmondy and Siententopf, and Lord Rayleigh in this sphere.

Next comes the disperse phase conception and its viscosity variations consequent on its solid or liquid state, and this leads on to a description and explanation of the Brownian movement and the possession of electric charges by colloidal particles. Emulsoids, suspensoids, protective colloids and their "gold values," emulsions, the phenomenon of interfacial tension, the Hofmeister series of albumens and semi-colloids are then surveyed. Several chapters are devoted to gels and the various properties which are associated with them, whilst Chaps. VIII. and IX. are concerned with the complex phenomena of colloidal adsorption. The final chapter contains a general survey of the whole subject, and discusses critically the theories advanced in explanation. The great applicability of colloidal science to many of the problems of industrial work is pointed out, and the need for further research is emphasised.

This edition is supplemented with an Appendix on experimental technique, which, in view of the unfamiliar nature of much of the practical work associated with colloidal investigations, will be found to be very helpful.

C. S. G.

ZOOLOGY

The Migrations of Fish. By A. MEEK, M.Sc. [Pp. xviii + 427, with 12 plates and 128 illustrations.] (London: Edwin Arnold, 1916. Price 16*s*. net.)

THE problem of the supply of food fish is one of the greatest importance in this country, surrounded as it is by the sea, and moreover it is beyond doubt that in many parts of the coast the fishing which might employ many thousands of persons has been declining for years past. At the present time the question of food supply is an urgent one, and with the advent of peace there is little doubt that the means of employing the millions that will return to this country will also

become a pressing problem that needs preparation in advance. The fishing industry if properly revived might absorb many in an occupation profitable alike to themselves and the country. Before any steps can be taken in these matters or in the regulation of the fishery when formed, knowledge of not only the life-history and movements of food fishes, but of all fish and their food, is required. The migrations of fish are in the main determined by two factors: search for food and a place to serve as a reproductive centre, *i.e.* a spawning ground. Hence the usefulness of a book like the present volume at a time when there is need for the information it contains being much more widespread than it has been heretofore.

The title of the book is in our opinion not quite well chosen, for it covers more ground than is there indicated. Quite half the book, if not more, is devoted to the geographical distribution of fish, an allied problem but one that could easily be dealt with separately, and one that is in many cases only very remotely connected with the actual migrations of fish.

A large amount of material has been gathered together from many original sources and is here rendered available to the general reader for the first time. Throughout the book footnotes guide one to the original sources of many of the observations referred to in the text, and in addition a short list of the most important general papers on the subject-matter of each chapter is provided at its end. Though the writing is not at all times easy to follow, mostly in the theoretical parts, the descriptions and accounts are on the whole quite lucid.

Note must be taken of the illustrations. Many useful maps giving the distribution of various families or genera are provided in addition to sketches of adult and larval stages of many species. The plates reach a high level throughout and in some cases are excellent, and the printer has in them set a standard of reproduction that we should like to see followed in other books.

The need for such a book has already been indicated, and this, together with the fact that it is the first attempt to deal with the subject in an exhaustive way in English, should ensure it a wide circulation. It is useful, well printed, and well arranged.

C. H. O'D.

Changes in Shade, Colour, and Pattern in Fishes, and their bearing on the problems of adaptation and behaviour, with especial reference to the flounders Paralichthys and Ancylopsetta. By S. O. MAST. "Bulletin Bureau of Fisheries." Vol. XXXIV., 1914. Document No. 821. Issued April 10, 1916. Price 40 cents.

The Structure and Growth of the Scales of the Squeteague and the Pigfish as indicative of life-history. By H. F. TAYLOR. "Bulletin Bureau of Fisheries." Vol. XXXIV., 1914. Document No. 823. Issued September 23, 1916. Price 20 cents.

The Fairport Fisheries Biological Station: its equipment, organisation, and functions. By R. E. COKER. "Bulletin Bureau of Fisheries." Vol. XXXIV., 1914. Document No. 829. Issued July 7, 1916. Price 20 cents.

Notes on the Embryology and Larval Development of Five Species of Teleostean Fishes. By A. KUNTZ. "Bulletin Bureau of Fisheries." Vol. XXXIV., 1914. Document No. 831. Issued August 3, 1916. Price 10 cents.

FROM a number of "Documents" received in 1916 from the United States Bureau of Fisheries the above four are selected as being of more than local interest, and

as typical of the class of research carried on under the direction of Dr. H. M. Smith, the able Commissioner of Fisheries of the United States.

Mr. Mast's paper refers to the changes that occur in the shade, colour, and pattern in fishes, with special reference to their extent and biological significance, and the factors involved in their production. These changes are classed as psychic, sexual, and adaptive. The paper, which is magnificently illustrated (19 plates), is, unlike many technical contributions to zoology, of considerable general interest, and likely to appeal to other than professional naturalists. Mr. Mast experimented with two American flounders, Paralichthys and Ancylopsetta, keeping them under various conditions of background and light. Full details of the experiments, and a summary of results, are given. It is hardly necessary to say that a careful perusal of this work is indispensable to all students of protective coloration or colour-changes in animals. All the Documents published by the United States Bureau of Fisheries may be obtained from the Superintendent of Documents, Government Printing Office, Washington, D.C., U.S.A., and in the case of those under review, at the prices stated above. We must congratulate Mr. Mast on a most valuable contribution to the subject.

Now that scale examination is fashionable, fishery experts all over the world will welcome Mr. Taylor's paper. Taking as material the Squeteague (*Cynoscion regalis*) and the Pigfish (*Orthopristis chrysopterus*), Mr. Taylor investigates the scale characters employed in the determination of life-history, their origin, constancy, bearing on life-history, the various methods of detecting them, together with other observations not closely allied to the main subject. From a practical fishery standpoint an important section of his work is that devoted to the application of scale characters to age determination. Although Mr. Taylor deals with species not known in Europe, his conclusions are of considerable general interest, if, indeed, conclusions they can be called, since the last word has by no means been said on the subject.

Dr. Coker gives an account of the Fairport Fisheries Biological Station, of which institution he was formerly Director. Fairport is situated on the Mississippi River about half-way between St. Paul and the mouth of the Ohio, and consequently is devoted to the investigation of problems concerning fresh-water fish. It is not to the credit of British Fishery Administration that no similar institution exists in the British Isles. Of course, the fresh-water fish and fisheries of the United States are quite unlike those of the British Isles, and some of the questions which loom large in the former country, *e.g.* mussel propagation, are not met with here. Nevertheless the fresh-water fish and fisheries of this country are of sufficient importance to justify the erection and equipment of a station devoted to their investigation. In this connection a description of the buildings and equipment at Fairport is valuable.

The paper by Dr. Kuntz, like the two first, is a contribution from the United States Fisheries Biological Station, Beaufort, N.C. It deals with the fertilised egg and early development of five American Teleosts. Students of Teleostean development will be interested to compare the stages with closely allied European or Asiatic forms.

J. T. J.

The Hunting Wasps. By J. HENRI FABRE. Translated by ALEXANDER TEIXEIRA DE MATTOS, F.Z.S. [Pp. ix + 393.] (London: Hodder & Stoughton, 1916. Price 6*s.* net.)

THE present volume is a further instalment in the series of translations of the works of J. Henri Fabre, and is in the main from vol. i. of his *Souvenirs Entomo-*

logiques. It forms the first of three volumes on wasps. For some reason or other the work of Fabre was very little known in his own country, and hardly at all in this until comparatively recently. He was a magnificent observer with an extraordinary skill and patience, and when these gifts are combined, as they were in his case, with great command of language and power of expression, the result is the production of most interesting and readable books. One is inclined to regard Fabre as a rather austere hermit-like man, but although his last years were spent in seclusion, he was not always so. Read, for example, Chap. XI. in the present work, "The Ascent of Mont Ventoux," and you cannot help entering into the naturalist's zest for the "Homeric" repast partaken after a long hard morning's climb. His love of nature, outdoor activity, and the presence of congenial company is everywhere apparent, and this chapter throws a delightful side-light on his character. The remaining nineteen chapters deal with the life histories of those wonderful insects, the hunting wasps, Cerceris, Sphex, Ammophila, and Bembex. Even when allowance is made for the almost poetic imagery in which the accounts are clothed, it is impossible not to recognise that they record some of the most remarkable of nature's fairy tales. To provide for the future of their offspring, these wasps dig some sort of a burrow in which the egg is laid, and which serves as a larder for the young. This larder needs filling, and here the difficulty arises in most cases, for the food must be fresh, and yet at the same time not so active that it can by its movements damage the young larva. In one case this demand is met by bringing fresh food as required. In the remainder the mother, with an uncanny knowledge of the internal anatomy of her prey which she has not seen, stings in such a way as to paralyse the motor nerve centres, and yet at the same time leave the animal alive so that it will be fresh and harmless when the egg hatches some time later. Various experiments to test the powers and limitations of the instinct of these wasps are also recorded and discussed.

The whole book is one of enthralling interest to both scientist and layman alike, and one that once begun cannot be laid aside until it is finished, by which time it will have provided much food for reflection.

Mr. de Mattos has supplied a very good translation, one that retains the form and spirit of the original with remarkable fidelity.

C. H. O'D.

Memoirs of the Indian Museum. Vol. V. Fauna of the Chilka, No. 4, July 1916. Price Rs. 8.8.

Records of the Indian Museum. Vol. VIII, Part IX. Zoological Results of the Abor Expedition, 1911-12. August 1916. Price Rs. 2.

Ditto Vol. XII. Pt. I. Feb. 1916. Price Rs. 2.

Ditto Vol. XII. Pt. III. May 1916. Price Rs. 2.

Ditto Vol. XII. Pt. IV. Aug. 1916. Price Rs. 2.

All published by order of the Trustees of the Indian Museum and printed at the Baptist Mission Press, Calcutta.

The above list brings the publications of the Indian Museum up to August 1916. The part of the Memoirs continues the account of the Fauna of Lake Chilka already reviewed in these columns. A description is now given of the Mollusca Gastropoda and Lamellibranchiata, with an account of the anatomy of the common Solen; the Mollusca Nudibranchiata; stages in the Life-History of Gobius, Petroscirtes and Hemirhamphus; the Cumacea and the first part of the fish of the Lake. This last paper by Dr. B. L. Chaudhuri of the Indian Museum deals with the Selachii and Batoidei together with the two sub-orders of the Teleostei,

viz. the Malacopterygii and Ostariophysi. Detailed criticism must be reserved until the publication of Dr. Chaudhuri's complete paper, but we have already sufficient to show that this, the most important constituent of the fauna of the Lake, will be dealt with in a proper and scientific manner. With each succeeding paper on the fish fauna of India we are more and more compelled to the conclusion that the volumes by Day (1889) are hopeless as guides to the fish of India. We should like the Editor of the "Fauna of British India" to arrange with the Superintendent of the Indian Museum for a re-issue of the volumes on fish in that series. We believe there would be a real demand for such a work amongst the sportsmen and naturalists in India.

The part of the records devoted to the zoological results of the Abor expedition of 1911-12 deals with the Terrestrial Isopoda and the sixth part of the Mollusca; the former by Dr. Collinge, the latter by Lieut.-Col. Godwin-Austen.

The remainder of the parts of the "Records" are devoted to, for the most part, faunistic descriptions of various groups of Indian animals, including Odonata, Lignicolous Beetle larvæ, Terrestrial Isopoda, two new fish from Chilka, a new Chlamys from Calcutta, and a description of a Coleopterous larva (Lasiodactylus).

There are also papers on Aphides, Cestoda, Dragonflies, and Mollusca from Cochin and Ennur.

In Vol. XII., Pt. I., there is an interesting account of the Hydrozoon *Campanula ceylonensis*, Browne, by Major Lloyd and Dr. Annandale. This is a brackish water species found in great numbers near Calcutta.

It is impossible to review in any detail the numerous papers printed in the above publications. At a time when scientific work in Europe is being reduced to a minimum it is refreshing to contemplate a part of the world where conditions appear to be more tranquil.

J. T. J.

Laboratory Manual in General Microbiology. Prepared by the Laboratory of Bacteriology, Hygiene, and Pathology, Michigan Agricultural College. [Pp. xvi + 418, with numerous illustrations.] (New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1916. Price 10*s*. 6*d*. net.)

In criticising this Laboratory Manual, it should be remembered that it is supposed to be used by students who are being directed in their work by a competent instructor, and therefore omissions which seem at first sight to reduce its usefulness must to a very large extent be regarded as of little real demerit. Part I. deals with laboratory methods which are used in the study of micro-organisms, and some of the methods are those specially useful in dealing with agricultural problems, and are therefore not found in the ordinary text-books. This part of the book gives all that is necessary to enable any ordinary student to do satisfactory work, and should be of the greatest value to those who are trying to get a good working acquaintance with bacteriological methods. Part II. deals with the physiology of micro-organisms, and though parts of this are good, yet, on the whole, an attempt has been made to do too much, and in consequence some of the exercises are scrappy and rather confusing than otherwise. Much of the work here is of extreme value and it is to be hoped that in a future edition this part will be elaborated and extended, *e.g.*, the exercises in the enzymes. Applied bacteriology, including the bacteriology of the air, water, sewage, and soil, is treated in Part III., and in addition some exercises are given in dairy and plant microbiology, and a few on animal diseases and immunity. These latter are, we think, the weakest in the whole work, and some parts strongly

support the view that bacteriologists who deal with diseased conditions in animals and in man should be trained in veterinary medicine or in human pathology.

Thus in the preparation of bacterial vaccines it is suggested that *the most numerous type* of colony should be taken, but as most of those who have actual experience with vaccines know, such a vaccine may be, and very commonly is, utterly useless. The only other adverse criticism I would offer is of the Plates I. to VI. I take it that these are supposed to represent models of moulds, bacteria, etc., though there is no indication to this effect, but in any case they appear to me to be most confusing and give a very erroneous impression. There is a useful appendix, and a list of works of reference. Taken all over, the work will prove of considerable value to laboratory workers, and will certainly serve to make the worker more independent of his teacher—a claim which is made by the authors.

J. M. BEATTIE.

Mosquitoes and Their Relation to Disease. Their Life-History, Habits, and Control. By F. W. EDWARDS, B.A., Assistant in the Department of Entomology, British Museum (Natural History). Economic Series No. 4. [Pp. 19, with 6 figures.] (London: Printed by Order of the Trustees of the British Museum, 1916. Price 1*d.*)

THIS pamphlet is apparently issued for the instruction of sanitary officers who have to deal with mosquitoes as carriers of disease, but is little more than a *réchauffé* of numbers of such pamphlets published during nearly twenty years in many languages. We must gather from internal evidence that the work is not based upon personal sanitary experience in the field, because too much is made of academical expedients such as fish and culicifuges, which generally give more trouble than they are worth, while the numerous important details of reduction of breeding-places are nearly all lumped together under the misleading term "drainage." Nothing has been more harmful to the cause of the prevention of mosquito-borne diseases in general than such academical works, because they divert attention from practical ones already written. Thus, in the short bibliography at the end, the book on the Prevention of Malaria by twenty practical workers and myself is not even mentioned; while a work by the late Sir Rubert Boyce, which was nothing but a popular exposition of my previous teaching, heads the list. The author does not appear to be fully acquainted with the history of the subject, for the names of Low and of James, who first indicated that the filariæ which cause elephantiasis enter by route of the insects' proboscis, are entirely omitted from the list of workers on that subject, while the list of men who connected malaria with mosquitoes is headed by the piratical Grassi (see p. 669), apparently because he is a zoologist. The pamphlet is printed "By Order of the Trustees of the British Museum." It would be advisable if so-called scientific public bodies of this kind would take more care as to the facts before giving currency to such incorrect histories of scientific work.

R. R.

Geodetic Surveying. By EDWARD R. CARY, C.E. [Pp. x + 280, with 100 figures and illustrations and numerous tables.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Limited, 1916. Price 10*s.* 6*d.* net.)

THE author opens his work with six pages of "Introduction"—two pages of which are taken up by figures. In the remaining four pages the author gives in outline

the contents of the book, and gives it in such a manner as to be of real value to the student and to inspire his confidence and interest in the work.

The opening words are a definition of Geodetic Surveying, which is given as "that branch of surveying in which by the most precise methods, the geographic positions of points are found and the form and dimensions of the earth are determined."

These introductory words, in addition to serving as a definition, almost seem to serve the purpose of a text, because "precision" and "method" are the keynotes to the whole volume—"cost" at the same time receiving careful consideration.

After the Introduction, the eight chapters in order are: (I.) Reconnaissance. (II.) Base Lines. (III.) Horizontal Angles. (IV.) Adjustment of the Horizontal Angles. (V.) Computation of Geodetic Latitudes, Longitudes, and Azimuths. (VI.) Map Projections. (VII.) Trigonometric Levelling. (VIII.) Precise Levelling. Then follow two Appendices: (1) Time, Longitude, Latitude, and Azimuth (*i.e.* Astronomical Surveying); (2) The Method of Least Squares.

In strongly recommending this book as a textbook, it should be pointed out that a most careful study of the author's treatment of his subject has greatly strengthened one's idea that the technical instruction so necessary to the engineer can be made of definite and weighty educational value. The author has a particular gift in this direction.

Due attention has been paid to "cost"—a side so sadly neglected (unnecessarily so) in the academic training of engineers. The author is to be further congratulated on the fact that, in a volume so thoroughly scientific, he has not hesitated, here and there, to introduce the question of "proper equipment" for a surveying party—not stopping short after giving the necessary "instruments" but adding the number of horses, mules, waggons, tents, bedding, etc., that will be required.

The author is the Professor of Railroad Engineering and Geodesy in the Rensselaer Polytechnic Institute, and his work, so thorough in every detail, is a standing indictment against the British method of engineering education, where in some Universities and many institutions of University rank there is only *one* professor of engineering. This volume alone shows that the student in civil engineering when he studies surveying should be taught by a *professor* of the subject, and that each college professing to teach civil engineering should have at least four *professors*. The same holds good for other branches of engineering, so that sixteen to twenty professors with a suitable number of assistants would be required in a University engineering school.

The many excellent textbooks now being published in U.S.A. point to the fact that the young American engineer is being instructed and *educated* by picked trained brains, and that his British competitor will, in the near future—unless our engineering schools are re-modelled—be at a very serious disadvantage.

J. Wemyss Anderson.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Mathematical Analysis. By Édouard Goursat, Professor of Mathematics in the University of Paris, translated by Earle Raymond Hedrick, Professor of Mathematics in the University of Missouri. Vol. I. Derivatives and Differentials, Definite Integrals, Expansion in Series, Applications to Geometry. Vol. II., Part I., The Function of a Complex Variable. Translated by Earle Raymond Hedrick and Otto Dunkel, Instructor in Mathematics, the University of Missouri. London: Ginn & Co., and Boston, New York and Chicago, 1916. (Pp., Vol. I. viii + 548, Vol. II. x + 259.) Price 16*s*. and 11*s*. 6*d*. respectively.

Functions of a Complex Variable. By Thomas M. Macrobert, M.A., B.Sc., Lecturer in Mathematics in the University of Glasgow. London: Macmillan & Co., St. Martin's Street, 1917. (Pp. xiv + 298.) Price 12*s*. net.

Differential and Integral Calculus. By Clyde E. Love, Ph.D., Assistant Professor of Mathematics in the University of Michigan. New York: The Macmillan Company, 1916. (Pp. xviii + 243.) Price 9*s*. net.

Theory of Errors and Least Squares. A Textbook for College Students and Research Workers. By LeRoy D. Weld, M.S., Professor of Physics in Coe College. New York: The Macmillan Company, 1916. (Pp. xii + 190.) Price 5*s*. 6*d*. net.

Mr. S. Ramanujan's Mathematical Work in England. Report by Mr. G. H. Hardy to the University of Madras. Cambridge: Printed at the University Press, 1916. (Pp. 1–13.)

The Geometrical Lectures of Isaac Barrow. Translated, with Notes and Proofs, and a Discussion on the Advance made therein on the Work of his Predecessors in the Infinitesimal Calculus. By J. M. Child, B.A., B.Sc. The Open Court Series of Classics of Science and Philosophy, No. 3. Chicago and London: The Open Court Publishing Company, 1916. (Pp. xiv + 218.) Price 4*s*. 6*d*.

Elementary Dynamics of the Particle and Rigid Body. By R. J. A. Barnard, M.A., Professor of Mathematics, Royal Military College of Australia. London: Macmillan & Co., St. Martin's Street, 1916. (Pp. vii + 374.) Price 6*s*. net.

Sur les Problèmes Célèbres de la Géométrie Élémentaire. Non Résoluble avec le Règle et le Compas. Par F. Gomes Teixeira. Coimbre: Imprimérie de l'Université, 1915. (Pp. 132.)

The Weather Map. An Introduction to Modern Meteorology. By Sir Napier Shaw, F.R.S., Director of the Meteorological Office. Published by the Authority of the Meteorological Committee. London: Printed under the Authority of His Majesty's Stationery Office by Darling & Son, Ltd., Bacon Street, E., 1916. (Pp. 94.) Price 4*d*.

Practical Experiments in Heat and Light. By W. St. B. Griffith, M.A., B.Sc., and P. T. Petrie, M.Sc., A.M.Inst.C.E. London: Rivingtons, 1916. (Heat, Pp. vii + 123, with 28 diagrams; Light, Pp. vii + 112, with 23 diagrams.) Price, bound together in one volume, 3*s*. 6*d*. net. Or in two parts—Heat, 2*s*. net; Light, 1*s*. 6*d*. net.

A book intended for use in school laboratories, and designed to be used in conjunction with lectures or a textbook. It contains a good selection of experiments described with full detail in simple language so that they can be performed with a minimum of assistance from the master. Any teacher who has not yet made a choice from the many books of this type already available will probably find it repay him to examine this one.

Theoretical Chemistry, from the Standpoint of Avogadro's Rule and Thermodynamics. By Prof. Walter Nernst, Ph.D., of the University of Berlin. Revised in Accordance with the Seventh German Edition. By H. T. Tizard, M.A., Fellow of Oriel College, Oxford. London: Macmillan & Co., St. Martin's Street, 1916. (Pp. xix + 853.) Price 15*s*. net.

Chemical Discovery and Invention in the Twentieth Century. By Sir William A. Tilden, F.R.S., D.Sc., LL.D., Sc.D., Prof. Emeritus of Chemistry in the Imperial College of Science and Technology. London: George Routledge & Sons, Ltd., Broadway House, Carter Lane, E.C.; New York: E. P. Dutton & Co. (Pp. xvi + 487, with 150 illustrations.) Price 7*s*. 6*d*. net.

One Hundred Chemical Problems. Suitable for University, Local and Other Examinations. By E. Arthur Mason, B.Sc., Queen Mary's School, Walsall. London: G. Bell & Sons, Ltd., York House, Portugal Street, Kingsway, W.C., 1917. (Pp. 8.) Price 6*d*. net.

A Textbook of Thermochemistry and Thermodynamics. By Prof. Otto Sackur, Ph.D., late of the Kgl. Friederich-Wilhelm University in Berlin. Translated and Revised by G. E. Gibson, Ph.D., Instructor in Chemistry at the University of California. London: Macmillan & Co., Ltd., St. Martin's Street, 1917. (Pp. xvi + 439.) Price 12*s*. net.

General Chemistry for Colleges. By Alexander Smith, Professor of Chemistry and Head of the Department, Columbia University. Second Edition, Entirely Rewritten. London: G. Bell & Sons, Ltd., 1916. (Pp. x + 662.). Price 6*s*. 6*d*. net.

Economic Geology. By Heinrich Ries, A.M., Ph.D., Professor of Geology at Cornell University. Fourth Edition, Thoroughly Revised and Enlarged. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1916. (Pp. xviii + 856, with 291 figures and 75 plates.) Price 17*s*. net.

The Rhythmic Deposition of Flint. By Prof. Grenville A. J. Cole, M.R.I.A., F.G.S. Extracted from the *Geological Magazine*, N.S., Decade VI. Vol. IV. pp. 64–8, February 1917.

A Naturalist in Borneo. By the late Robert W. C. Shelford, of Emmanuel College, Cambridge, M.A., F.L.S., F.E.S., late Curator of the Sarawak Museum and Assistant Curator of the Hope Department of Zoology, Oxford University Museum. Edited, with a Biographical Introduction, by Edward B. Poulton, D.Sc., LL.D., F.R.S., Hope Professor of Zoology and Fellow of Jesus College, Oxford. London: T. Fisher Unwin, Ltd., 1916. (Pp. xxviii + 331, with 32 illustrations.) Price 15*s*.

Recent Progress in the Study of Variation, Heredity, and Evolution. By Robert Heath Lock, M.A., Sc.D., sometime Fellow of Gonville and Caius

College, Cambridge; late Assistant Director of the Royal Botanic Gardens, Ceylon. Revised by Leonard Doncaster, Sc.D., F.R.S., Fellow of King's College, Cambridge. Biographical Note by Bella Sidney Woolf (Mrs. R. H. Lock). London: John Murray, Albemarle Street, W., 1916. (Pp. xxiii + 336, with 6 portraits and 47 figures in the text.) Price 6*s*. net.

Morphology of Invertebrate Types. By Alexander Petrunkevitch, Ph.D., Assistant Professor of Zoology in the Sheffield Scientific School of Yale University. New York: The Macmillan Company, 1916. (Pp. xiii + 263, with 50 figures in the text.)

Records of the Indian Museum. Vol. XII., Parts V., VI., VII., and VIII., 1916. Calcutta: Published by order of the Trustees of the Indian Museum. Printed at the Baptist Mission Press. Price 2 rupees each.

Memoirs of the Indian Museum. Vol. VI., Nos. 2 and 3, 1916. Calcutta: Published by order of the Trustees of the Indian Museum. Printed at the Baptist Mission Press. Price 2 rupees 8 annas and 2 rupees respectively.

A Critique of the Theory of Evolution. By Thomas Hunt Morgan, Professor of Experimental Zoology in Columbia University. Lectures delivered at Princeton University, February 24, March 1, 8, and 15, 1916. Princeton: Princeton University Press. London: Humphrey Milford, Oxford University Press, 1916. (Pp. x + 197, with 95 illustrations.) Price 6*s*. 6*d*. net.

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This is one of the Oxford German Series by American Scholars, and consists of a series of excellent essays designed to teach the English reader both natural science and the German language.

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A useful publication designed to assist students in the choice of books, and is compiled so that the nature and price of any book can be rapidly ascertained. It includes works on Modern History, Geography, Economics, Philosophy, English and other Modern Classics, Literature of the Ancient World, General Literature, and Natural Science and Medicine.

ANNOUNCEMENTS

ROYAL ASTRONOMICAL SOCIETY. Meetings 5 p.m., April 13, May 11, June 8.

ROYAL HORTICULTURAL SOCIETY. Lectures at 3 p.m. April 11, 17, 24, May 8, 22. Also Lectures on June 5, 19. Admission 2*s*. 6*d*.

CHEMICAL SOCIETY. Meetings 4 p.m. April 19, May 3, 10, 17 (Lecture by Mr. A. Chaston Chapman), June 7, 21.

THE INSTITUTION OF MECHANICAL ENGINEERS. General Meeting 6 p.m., April 20. Graduates' Meetings are temporarily suspended.

The Advisory Council of the Government's Department of Scientific and Industrial Research have added to the list of their technical Committees a Standing Committee on Glass and Optical Instruments. This Committee appointed a series of Sub-Committees to which various special problems were referred: Raw materials for glass and glass-making—Optical properties of a large range of glasses—General physical and chemical properties of glass and glassware for scientific and industrial purposes—Testing and standardising of glassware—Workshop technique—X-ray glass apparatus—Optical calculations and lens designing—Optical instruments—Translation of foreign works on optics. They invite manufacturers who have experienced difficulties requiring investigations for their solution in connection with subjects of glass and optical instruments, or who desire to make suggestions for special researches on these, to communicate in the first instance with the Secretary of the Research Department, Great George Street, Westminster, S.W.

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